172

ANNUAL REPORT

OF THE

BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING

THE OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION

TO

JULY, 1890.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1891.

FIFTY-FIRST CONGRESS, SECOND SESSION.

Concurrent resolution adopted by the House of Representatives March 2, 1891, *and by the Senate March* 3, 1891.

Resolved by the House of Representatives (*the Senate concurring*), That there be printed of the Reports of the Smithsonian Institution and of the National Museum for the year ending 30th June, 1890, in two octavo volumes, 19,000 extra copies; of which 3,000 copies shall be for the use of the Senate, 6,000 copies for the use of the House of Representatives, 7,000 copies for the use of the Smithsonian Institution, and 3,000, copies for the use of the National Museum.

From the library of Stephen D. Peet

LETTER

FROM THE

SECRETARY OF THE SMITHSONIAN INSTITUTION,

ACCOMPANYING

The annual report of the Board of Regents of the Institution to the end of June, 1890.

SMITHSONIAN INSTITUTION,
Washington, D. C., July 1, 1890.

To the Congress of the United States:

In accordance with section 5593 of the Revised Statutes of the United States, I have the honor, in behalf of the Board of Regents, to submit to Congress the annual report of the operations, expenditures, and condition of the Smithsonian Institution for the year ending June 30, 1890.

I have the honor to be, very respectfully, your obedient servant,

S. P. LANGLEY,
Secretary of Smithsonian Institution.

Hon. LEVI P. MORTON,
President of the Senate.

Hon. THOMAS B. REED,
Speaker of the House of Representatives.

ANNUAL REPORT OF THE SMITHSONIAN INSTITUTION TO THE END OF JUNE, 1890.

SUBJECTS.

1. Proceedings of the Board of Regents for the session of January, 1890.

2. Report of the Executive Committee, exhibiting the financial affairs of the Institution, including a statement of the Smithson fund, and receipts and expenditures for the year 1889–'90.

3. Annual report of the Secretary, giving an account of the operations and condition of the Institution for the year 1889–'90, with statistics of exchanges, etc.

4. General appendix, comprising a selection of miscellaneous memoirs of interest to collaborators and correspondents of the Institution, teachers, and others engaged in the promotion of knowledge.

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THE SMITHSONIAN INSTITUTION.

MEMBERS EX OFFICIO OF THE "ESTABLISHMENT."

(January, 1890.)

BENJAMIN HARRISON, President of the United States.
LEVI P. MORTON, Vice-President of the United States.
MELVILLE W. FULLER, Chief-Justice of the United States.
JAMES G. BLAINE, Secretary of State.
WILLIAM WINDOM, Secretary of the Treasury.
REDFIELD PROCTOR, Secretary of War.
BENJAMIN F. TRACY, Secretary of the Navy.
JOHN WANAMAKER, Postmaster-General,
W. H. H. MILLER, Attorney-General.
CHARLES E. MITCHELL, Commissioner of Patents.

REGENTS OF THE INSTITUTION.

(List given on the following page.)

OFFICERS OF THE INSTITUTION.

SAMUEL P. LANGLEY, *Secretary.*

Director of the Institution and of the U. S. National Museum.

G. BROWN GOODE, *Assistant Secretary.*

WILLIAM J. RHEES, *Chief Clerk.*

REGENTS OF THE SMITHSONIAN INSTITUTION.

By the organizing act approved August 10, 1846 (Revised Statutes, Title LXXIII, section 5580), "The business of the Institution shall be conducted at the city of Washington by a Board of Regents, named the Regents of the Smithsonian Institution, to be composed of the Vice-President, the Chief-Justice of the United States [and the Governor of the District of Columbia], three members of the Senate, and three members of the House of Representatives, together with six other persons, other than members of Congress, two of whom shall be resident in the city of Washington, and the other four shall be inhabitants of some State, but no two of the same State."

REGENTS FOR THE YEAR 1890.

The Vice-President of the United States:
LEVI P. MORTON.

The Chief-Justice of the United States:
MELVILLE W. FULLER, elected Chancellor, and President of the Board January 9, 1889.

United States Senators: Term expires.
JUSTIN S. MORRILL (appointed February 21, 1883)..............Mar. 3, 1891.
SHELBY M. CULLOM (appointed March 23, 1885, and Mar. 28, 1889). Mar. 3, 1895.
RANDALL L. GIBSON (appointed Dec. 19, 1887, and Mar. 28, 1889)..Mar. 3, 1895.

Members of the House of Representatives:
JOSEPH WHEELER (appointed Jan. 5, 1888, and Jan. 6, 1890)....Dec. 23, 1891.
BENJAMIN BUTTERWORTH (appointed January 6, 1890).......Dec. 23, 1891.
HENRY CABOT LODGE (appointed January 6, 1890).............Dec. 23, 1891.

Citizens of a State:
HENRY COPPÉE, of Pennsylvania (first appointed Jan. 19, 1874)..Dec. 26, 1891.
JAMES B. ANGELL, of Michigan (first appointed Jan. 19, 1887)..Jan. 19, 1893.
ANDREW D. WHITE, of New York (first appointed Feb. 15, 1888)..Feb. 15, 1894.
[Vacancy]

Citizens of Washington:
JAMES C. WELLING (first appointed May 13, 1884)..............May 22, 1896.
MONTGOMERY C. MEIGS (first appointed December 26, 1885)....Dec. 26, 1891.

Executive Committee of the Board of Regents.

JAMES C. WELLING, *Chairman.* HENRY COPPÉE. MONTGOMERY C. MEIGS.

JOURNAL OF PROCEEDINGS OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION.

WASHINGTON, *January* 8, 1890.

The stated annual meeting of the Board of Regents of the Smithsonian Institution was held this day at 10.30 o'clock A. M.

Present: The Chancellor, Chief-Justice MELVILLE W. FULLER; Hon. J. S. MORRILL, Hon. S. M. CULLOM, Hon. JOSEPH WHEELER, Hon. HENRY CABOT LODGE, Gen. M. C. MEIGS, Dr. ANDREW D. WHITE, Dr. J. B. ANGELL, Dr. HENRY COPPÉE, Dr. J. C. WELLING, and the Secretary, S. P. LANGLEY.

An excuse for non-attendance was read from the Hon. BENJAMIN BUTTERWORTH, and the Secretary stated that he had been informed, unofficially, that Senator R. L. GIBSON was detained in New York by illness.

The following letter was read to the Board:

NEW HAVEN, CONNECTICUT, *December* 31, 1889.

I regret that I find it advisable, from considerations of health and prudence, to resign the position which I have held for so many years as a member of the Board of Regents of the Smithsonian Institution. With the best wishes for the prosperity of the Institution and the assurance of the highest personal regard for the members of the Board, I am,

Very truly yours,

NOAH PORTER.

To S. P. LANGLEY,
Secretary of the Smithsonian Institution.

On motion of Dr. Coppée, it was

Resolved, That the Board having received the resignation of Dr. Noah Porter as a Regent, accept it with an expression of their regret and with assurances of their high personal esteem.

The Journal of the Proceedings of the Board at the meeting of January 9, 1889, was read and approved.

The secretary announced the appointment (January 6, 1890) by the honorable the Speaker of the House of Representatives of the following members of the House as Regents:

Mr. BENJAMIN BUTTERWORTH, of Ohio.

Mr. HENRY CABOT LODGE, of Massachusetts.

Mr. JOSEPH WHEELER, of Alabama.

Dr. Welling, in presenting the report of the Executive Committee for the fiscal year ending June 30, 1889, called the attention of the Board to the statement on page 5, under the head of International Exchanges (which sets forth that an amount has been expended in this department beyond the annual appropriation made by Congress, entailing annual loss upon the fund of the Smithsonian Institution) and to the recommendation that Congress be requested to make appropriations to reimburse the Smithsonian fund.

On motion it was—

Resolved, That the Regents instruct the Secretary to ask of Congress legislation for the repayment to the Institution of the amount advanced from the Smithsonian fund for governmental service in carrying on the exchanges.

The report of the committee was then approved.

On motion of Dr. Welling it was also—

Resolved, That the income of the Institution for the fiscal year ending June 30, 1891, be appropriated for the service of the Institution, to be expended by the Secretary, with the advice of the Executive Committee, upon the basis of the operations described in the last annual report of said committee, with full discretion on the part of the Secretary as to items of expenditures properly falling under each of the heads embraced in the established conduct of the Institution.

The Secretary, in presenting his report for the year ending June 30, 1889, referred especially to the fact that the Smithsonian Institution is now, and has been for some time, paying out an increasingly large portion of its annual income in service that inures either directly or indirectly to the benefit of the Government, rather than to its legitimate application for the immediate "increase and diffusion of knowledge;" and in this connection quoted the opinion of Professor Henry, expressed as long since as 1872, that the Government should then have paid the Institution $300,000 for the use of the present building alone.

He did not ask for any immediate action, but invited the attention of the Regents to this condition of the relation of the Institution's affairs to those of the Government, a general condition of which the loss of the rent of the building might be taken as a single example.

The late Secretary had intended to provide an astro-physical observatory on a modest scale, the building for which would probably cost not over ten or fifteen thousand dollars, and with the expectation that if this amount were contributed by private citizens and the building placed on Government land, Congress would make an appropriation for purchasing the apparatus, and also a small annual appropriation necessary for maintenance. This amount having been pledged by responsible parties, the Secretary had ordered some of the principal pieces of apparatus which would take a long time to construct. A number of valuable pieces had also been loaned to the Institution, and to supply provisional needs, a cover for all these in the form of a small temporary

erection has been put up south of this building. This will enable the apparatus to be used, but it is not the "observatory" in question, which, if Congress makes the necessary appropriation, will probably be erected at some future time in some suburban site under the Regents' control.

In this connection he presented a copy of the will of the late Dr. Jerome H. Kidder, and letters from his executor, accompanied by a copy of an unsigned codicil. The Secretary stated that Dr. Kidder was a former officer of the U. S. Navy, who several years ago made a bequest of $10,000 to the Smithsonian Institution to be employed for certain biological purposes. Dr. Kidder afterwards informed the Secretary that owing to changes in his domestic circumstances, he had reduced the amount to $5,000 and changed the purpose of the bequest, which he was desirous to see applied to the astro-physical observatory in question. It appears however that though this was well known to Dr. Kidder's family and friends to be his deliberate purpose, he did not actually execute this provision to his will, but having ordered a codicil to that effect to be drawn, was stricken with so sudden an illness that he was unable to sign it. (The Secretary read two letters from the executor stating, in substance, that the family would cheerfully pay the $10,000, but that it earnestly desired to see this sum applied to the astro-physical observatory, in which Dr. Kidder's whole interest was lately engaged.)

After the clauses of the will and the codicil had been read a discussion followed, from which it appeared to be the opinion of the Board that if the Regents accepted, in accordance with the wishes of the family and the executors, the deliberate purpose of the testator in regard to the object of the bequest, they should be guided by this purpose also in regard to the amount which they should receive.

Mr. Morrill then offered the following preamble and resolution, which was adopted:

Whereas the late Jerome H. Kidder having, in a will drawn up some years before his death, bequeathed the sum of $10,000 to the Smithsonian Institution for purposes connected with the advancement of science, did in a codicil to said will, drawn under his direction during his last hours, but which his sudden death prevented him from executing, reduce the amount of his bequest to $5,000, which he desired should be applied toward the establishment of an astro-physical observatory: It is

Resolved, That the Executive Committee of the Board of Regents be authorized to accept, as finally and decisively indicative of the wishes of the testator the provisions of the codicil bequeathing $5,000 for the purpose of an astro-physical observatory, and that they be authorized to decline to accept from his executors more than this sum; provided, however, that before doing so they can receive sufficient assurance that the Institution will be protected against any liability.

The Secretary exhibited recently prepared sketch plans for a new Museum building, and called the attention of the Regents to their recommendation to Congress, in January, 1883, of the need of enlargement.

Since this resolution, the collections of the Museum have enormously increased, so that before a new building could now be completed the material pressing for display would more than cover the entire area of such a building as the present one. It seems absolutely necessary that the new building should contain, beside a basement, at least two stories, it being indispensable to have, apart from the purposes of display, upper rooms for the preparation of the exhibits below.

The price of material has risen very greatly, so that, owing to these combined causes, the estimate of 1883 is not applicable to the wants of to-day. The Secretary did not conceive that any supplementary action on the part of the Regents was now needed, but submitted these plans and estimates that they might be advised of the probable very considerable increase in the sum that it would now be necessary to ask of Congress.

The Chief Justice, being obliged to leave here, resigned the chair to Senator Morrill.

The Secretary stated that in connection with this subject of the plans he would present a letter from Mr. Cluss, of the firm of Cluss & Schulze, architects, asking for "an equitable compensation" for professional services and expenses in former years, in connection with a proposed building for the Museum.

On motion of General Meigs, it was

Resolved, That Messrs. Cluss & Schulze be informed that the question of compensation to them for plans for a new Museum building will be considered when they shall present such a bill as can be submitted for Congressional action.

The Secretary recalled to the attention of the Regents a statement made at their last meeting, to the fact that bills had been brought before Congress making an appropriation for the purpose of establishing a Zoological Park under a Board of Commissioners, of whom the Secretary of the Smithsonian Institution was one, and directing this Commission, after purchasing and laying out the land and erecting the necessary buildings, to turn it over to the Regents. The bill as since actually passed, however, only instructed the Commissioners to purchase the land; and, while declaring the Park to be for the advancement of science, gave no intimation of the intent of Congress about its ultimate disposal. This Commission has nearly completed the purchase, and the time has now arrived when the Park may advantageously be placed under scientific direction. He could not, of course, anticipate what the final action of Congress would be in the matter, but he was authorized to state that the Commission would feel satisfied if Congress should place the Park under the Regents' control. There is an increasing collection of animals already in the Regents' care, and an appropriation of $50,000 has been asked for, to provide for its establishment in the newly acquired Park, which, within its large area, would also provide suitable retirement for the small physical observatory already

alluded to. He expressed the hope that a bill providing for both measures would have the support of the Regents in the Senate and in the House.

After listening to statements by the Secretary relative to the estimates for the ensuing year, and also to the subject of the desirability of obtaining legislation relative to a statue of Professor Baird, the Regents considered the subject of a more convenient time for their annual meeting in January; and on motion of Senator Cullom it was—

Resolved, That hereafter the time of the annual meeting of the Board of Regents shall be on the fourth Wednesday in January of each year.

Mr. Wheeler called the attention of the Board to the death of their late colleague, the Hon. S. S. Cox, and on his motion it was—

Resolved, That a committee be appointed, of which the Secretary shall be chairman, which shall be authorized to prepare resolutions on the services and character of the late S. S. Cox, and to make the same of record.

The chairman announced as the committee, the Secretary, General Wheeler, Dr. Welling, Mr. Lodge.

The committee submitted the following report and resolutions, which were unanimously adopted:

To the Board of Regents:

Your committee report that the Hon. S. S. Cox was first appointed a Regent of the Smithsonian Institution December 19, 1861, and that he filled that office, except for intervals caused by public duties, to the time of his death.

While he was a regular attendant at all the meetings of the Board, he was ever ready to advance the interests of the Institution and of science, either as a Regent or as a member of Congress; and although such men as Hamlin, Fessenden, Colfax, Chase, Garfield, Sherman, Gray, and Waite, in a list comprising Presidents, Vice-Presidents, Chief Justices, and Senators of the United States were his associates, there were none whose service was longer or more gratefully to be remembered, nor perhaps any to whom the Institution owes more than to Mr. Cox.

The regard in which his brother Regents held Mr. Cox's accuracy of characterization, and his instinctive recognition of all that is worthiest of honor in other men, may be inferred from the eulogies which he was requested by them to deliver, among which may be particularly mentioned the one at the commemoration in honor of Professor Henry in the House of Representatives; but though these only illustrate a very small part of his services as a Regent, your committee are led by their consideration to recall that his first act upon your Board was the preparation and delivery of an address, at the request of the Regents, on their late colleague, Stephen A. Douglas, and that on this occasion he used words which your committee permit themselves to adopt, as being in their view singularly characteristic of Mr. Cox himself:

"It was not merely as one of its Regents that he showed himself the true and enlightened friend of objects kindred to those of this establishment. He ever advocated measures which served to advance knowledge and promote the progress of humanity. The encourage-

ment of the fine arts, the rewarding of discoverers and inventors, the organization of exploring expeditions, as well as the general diffusion of education were all objects of his special regard."

In view of these facts it is—

Resolved, That in the death of Hon. Samuel Sullivan Cox the Smithsonian Institution has suffered the irreparable loss of a long-tried friend, the Board of Regents of a most valued associate and active member during fifteen years of service, and the country of one of its most distinguished citizens.

Resolved, That the Board of Regents desire to express their deep sympathy with the bereaved family of the deceased, and direct that a copy of these resolutions be transmitted to the widow of their late associate.

On motion of Senator Cullom, the Board adjourned *sine die*.

REPORT OF THE EXECUTIVE COMMITTEE OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION.

(For the year ending 30th of June, 1890.)

To the Board of Regents of the Smithsonian Institution:

Your executive committee respectfully submits the following report in relation to the funds of the Institution, the appropriations by Congress for the National Museum and other purposes, and the receipts and expenditures for the Institution, the Museum, etc., for the year ending 30th June, 1890:

SMITHSONIAN INSTITUTION.

Condition of the fund July 1, 1890.

The amount of the bequest of James Smithson deposited in the Treasury of the United States, according to the act of Congress of August 10, 1846, was $515,169. To this was added by authority of Congress (act of February 8, 1867) the residuary legacy of Smithson and savings from annual income and other sources, $134,831. To this $1,000 have been added by a bequest of James Hamilton, $500 by a bequest of Simeon Habel, and $51,500 as the proceeds of the sale of Virginia bonds owned by the Institution, making in all, as the permanent Smithson fund in the United States Treasury, $703,000.

Statement of the receipts and expenditures from July 1, 1889, *to June* 30, 1890.

RECEIPTS.

Cash on hand July 1, 1889		$11,757.47	
Interest on fund July 1, 1889	$21,090.00		
Interest on fund January 1, 1890	21,090.00		
		42,180.00	
			$53,937.47
Cash from sales of publications	416.01		
Cash from repayments of freight, etc	3,489.50		
		3,905.51	
Cash from executors of Dr. Jerome H. Kidder, for astro-physical research		5,000.00	
Cash from Dr. Alex. Graham Bell, for astro-physical research		5,000.00	
			13,905.51
Total receipts			67,842.98

EXPENDITURES.

Building:			
Repairs, care, and improvements	$1,576.97		
Furniture and fixtures	92.21		
		$1,669.18	
General expenses:			
Meetings	409.40		
Postage and telegraph	222.00		
Stationery	269.85		
General printing	361.60		
Incidentals (fuel, gas, etc.)	1,723.77		
Library (books, periodicals, etc.)	1,029.46		
Salaries*	17,688.77		
		21,704.85	
Publications and research:			
Smithsonian Contributions	3,482.89		
Miscellaneous Collections	378.26		
Reports	815.16		
Researches	100.00		
Apparatus	6,105.60		
Explorations	1,530.00		
Museum	70.30		
		12,482.21	
Literary and scientific exchanges		1,794.09	
Total expenditures			37,650.33
Balance unexpended June 30, 1890			3,0192.6

The cash received from sales of publications, repayments for freight, etc., is to be credited on items of expenditure, as follows:

Meetings	$14.60		
Postage and telegraph	1.92		
General printing	14.50		
Incidentals	262.03		
Salaries	971.97		
		$1,265.02	
Smithsonian Contributions	115.19		
Miscellaneous Collections	273.72		
Reports	24.10		
		413.01	
Apparatus		7.50	
Explorations		430.00	
Exchanges		1,789.98	
			$3,905.5

The net expenditures of the Institution for the year ending June 30, 1890, were, therefore, $33,744.82, or $3,905.51 less than the gross expenditure, $37,650.33, above given.

All moneys received by the Smithsonian Institution from interest, sales, refunding of moneys temporarily advanced, or otherwise, are deposited with the Treasurer of the United States to the credit of the Secretary of the Institution, and all payments are made by his checks on the Treasurer of the United States.

*In addition to the above $17,683.77 paid for salaries under general expenses, $1,850.04 were paid for services, viz, $1,500 from the building account, and $350.0 from the library account.

Your committee also presents the following statements in regard to appropriations and expenditures for objects intrusted to the care of the Smithsonian Institution by Congress:

INTERNATIONAL EXCHANGES.

Appropriation by Congress for the fiscal year ending June 30, 1890, "for expenses of the system of international exchanges between the United States and foreign countries under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employés" (Sundry civil act, March 2, 1889. Public 154, p. 16)			$15,000.00

Expenditures from July 1, 1889, to June 30, 1890.

Salaries or compensation:			
1 curator, 12 months, at $208.33	$2,499.96		
1 clerk, 12 months, at $150	1,800.00		
1 clerk, 12 months, at $110	1,320.00		
1 clerk, 12 months, at $80	960.00		
1 clerk, 12 months, at $75	900.00		
1 clerk, 11 months, at $75	825.00		
1 clerk, 12 months, at $70	840.00		
1 copyist, 4 months, at $30	120.00		
1 copyist, 8 months, at $35	280.00		
1 copyist, 9 days, at $60	17.42		
1 copyist, 27 days, at $30	27.86		
1 copyist, 1 month, at $30	30.00		
1 packer, 12 months, at $75	900.00		
1 packer, 12 months, at $50	600.00		
1 laborer, 12 months, at $40	480.00		
1 laborer, 8 days, at $1.50	12.00		
1 laborer, 6½ days, at $1.50	9.75		
1 laborer, 4 days, at $1.50	6.00		
1 laborer, 3½ days, at $1.50	5.25		
1 laborer, 3½ days, at $1.50	5.25		
1 agent (Germany), 12 months, at $83.33⅓	1,000.00		
1 agent (England), 12 months, at $41.66⅔	500.00		
Total salaries or compensation		$13,138.49	
General expenses:			
Freight		998.67	
Packing boxes		443.41	
Printing		146.00	
Postage		144.52	
Stationery and supplies		116.92	
Total expenditure international exchanges			14,988.01
Balance July 1, 1890			11.99

NORTH AMERICAN ETHNOLOGY.

Appropriation by Congress for the fiscal year ending June 30, 1890, "for the purpose of continuing ethnological researches among the American Indians under the direction of the Secretary of the Smithsonian Institution, including salaries or compensation of all necessary employés." (Sundry civil act, March 2, 1869. Pub. 154, p. 16.)	40,000.00
Balance, July 1, 1889	13,491.22
	53,491.22

The actual conduct of these investigations has been continued by the Secretary in the hands of Major J. W. Powell, Director of the Geological Survey.

Ethnology—Expenditures from July 1, 1889, *to June* 30, 1890.

Classification of expenditures (A).

(*a*) Salaries or compensation:		
2 ethnologists, at $3,000 per annum	$6,000.00	
1 ethnologist, per annum	2,400.00	
1 archæologist, per annum	2,400.00	
3 ethnologists, at $1,800 per annum	5,400.00	
1 assistant ethnologist, at $1,500 per annum, 1 month	125.00	
1 assistant archæologist, at $1,500 per annum, 3 months	375.00	
1 assistant ethnologist, at $1,500 per annum, 3 months	375.00	
1 assistant ethnologist, per annum	1,400.00	
1 assistant archæologist, at $1,400 per annum, 3 months	350.00	
1 assistant ethnologist, per annum	1,200.00	
1 assistant ethnologist, at 1,200 per annum, 3 months 17 days	354.84	
1 assistant ethnologist, at $1,200 per annum, 9 months	900.00	
1 assistant ethnologist, at $1,200 per annum, 9 months	900.00	
1 assistant ethnologist, at $1,000 per annum, 9 months	750.00	
1 stenographer, per annum	1,000.00	
1 assistant ethnologist, at $900 per annum, 5 months 25 days	437.50	
1 assistant ethnologist, at $720 per annum, 6 months 6 days	376.00	
1 ethnologic aid, at $900 per annum, 5 months 25 days	437.50	
1 ethnologic aid, at $600 per annum, 7 months 5 days	308.05	
1 copyist, per annum	900.00	
1 modeller, per annum	720.00	
1 modeller, at $660 per annum, 6 months 6 days	340.65	
1 modeller, at $600 per annum, 9 months	450.00	
1 modeller, at $660 per annum, 1 month	55.00	
1 modeller, 2 months, at $60, $120; 1 month, at $55; 9 months, at $50, $450	625.00	
1 modeller, at $720 per annum, 2 months	120.00	
1 modeller, at $480 per annum, 3 months	120.00	
1 copyist, per annum	720.00	
1 copyist, at $600 per annum, 9 months	450.00	
2 clerks, at $600 per annum	1,200.00	
1 clerk, per annum	720.00	
1 messenger, per annum	600.00	
1 messenger, at $480 per annum, 1 month 23 days	70.66	
1 modeller, at $480 per annum, 3 months 24 days	150.97	
1 interpreter, at $900 per annum, 3 months	225.00	
Unclassified or special jobs or contracts	875.00	
Total salaries or compensation		$33,831.17
(*b*) Miscellaneous:		
Travelling expenses	3,958.34	
Transportation of property	336.43	
Field supplies	752.84	
Field supplies for distribution to Indians	131.36	
Instruments	5.18	

(*b*) Miscellaneous—Continued.

Laboratory material	$51.28	
Books for library	756.12	
Stationery and drawing material	330.45	
Illustrations for report	637.08	
Office furniture	392.38	
Office supplies and repairs	206.76	
Telegrams	.70	
Specimens	18.00	
		$7,576.92
Total expenditure		41,408.09
Bonded railroad accounts settled by United States Treasury		50.05
Total expenditure North American ethnology		41,458.14
Balance, July 1, 1890, to meet outstanding liabilities		12,033.08

Expenditures reclassified by subject-matters (B).

Sign language and picture writing	4,440.81
Exploration of mounds, eastern portion of United States	6,258.33
Researches in archæology, southwestern portion of United States	9,028.77
Researches, language of North American Indians	13,783.37
Salaries, office of director	4,209.64
Illustrations for report	673.46
Contingent expense	3,013.71
	41,408.09
Bonded railroad accounts settled by United States Treasury	50.05
Total expenditures	41,458.14

SUMMARY.

July 1, 1889:		
Balance on hand	$13,491.22	
Appropriation for North American ethnology, 1890	40,000.00	
		53,491.22
Expenditures		41,458.14
Balance on hand July 1, 1890		12,033.08
Which balance is deposited as follows:		
To credit of disbursing agent		2,581.38
In the United States Treasury		9,451.70
		12,033.08

NATIONAL MUSEUM.

PRESERVATION OF COLLECTIONS JULY 1, 1889, TO JUNE 30, 1890.

Appropriation by Congress for the fiscal year ending June 30, 1890, "for the preservation, exhibition, and increase of the collections from the surveying and exploring expeditions of the Government, and from other sources, including salaries or compensation of all necessary employés" (Sundry civil act, March 2, 1889. Public 154, p. 16)	140,000.00

Expenditures from July 1, 1889, *to June* 30, 1890.

Salaries or compensation.*	
Direction:	
1 Assistant Secretary Smithsonian Institution, in charge U. S. National Museum, 12 months, at $333.33	$3,999.96
Scientific staff:	
1 curator, 12 months, at $200	2,400.00
1 curator, 12 months, at $200	2,400.00
1 curator, 12 months, at $200	2,400.00
1 curator, 12 months, at $175	2,100.00
1 curator, 9 months, at $175	1,575.00
1 curator, 3 months, at $175	525.00
1 curator, 12 months, at $150	1,800.00
1 curator, 8 months 11 days, at $150	1,253.23
1 curator, 12 months, at $125	1,500.00
1 curator 11 months, at $100	1,100.00
1 acting curator, 12 months, at $150	1,800.00
1 assistant curator, 12 months, at $133.33	1,599.96
1 assistant curator, 12 months, at $133.33	1,599.96
1 assistant curator, 9 months, at $50, $450; 3 months, at $125, $375	825.00
1 assistant curator, 12 months, at $100	1,200.00
1 assistant curator, 5 months, 19 days, at $100	561.29
1 agent, 12 months, at $100	1,200.00
1 collector, 12 months, at $80	960.00
1 aid, 12 months, at $80	960.00
1 aid, 6 months 18 days, at $80	526.45
1 aid, 12 months, at $75	900.00
1 aid, 4 months 23 days, at $75	355.65
1 aid, 12 months, at 65	780.00
1 aid, 11 months 16 days, at $60	690.97
1 aid, 8 months 10 days, at $55	457.74
	31,470.25
Clerical staff:	
1 chief clerk, 12 months, at $175	2,100.00
1 corresponding clerk, 12 months, at $158.33	1,899.06
1 registrar, 12 months, at $158.33	1,899.96
1 disbursing clerk, 12 months, at $100	1,200.00
1 draftsman, 12 months, at $83.33	999.96
1 assistant draftsman, 12 months, at $40	480.00
1 clerk, 4 months 20 days, at $125	580.65
1 clerk, 12 months, at $115	1,380.00
1 clerk, 12 months, at $115	1,380.00
1 clerk, 12 months, at $100	1,200.00
1 clerk, 12 months, at $100	1,200.00
1 clerk, 12 months, at $90	1,080.00
1 clerk, 12 months, at $90	1,080.00
1 clerk, 11 months 22 days, at $83.33	969.86
1 clerk, 12 months, at $75	900.00
1 clerk, 12 months, at $70	840.00

*NOTE.—The payments of salaries for parts of months in January, March, July, August, October, and December are made on the basis of 31 days, and for the other months (except February) at 30 days.

Clerical staff—Continued.

1 clerk, 10 months 15 days, at $70	$733.87
1 clerk, 6 months, at $55, $330; 6 months, at $60, $360	690.00
1 clerk, 12 months, at $60	720.00
1 clerk, 3 months, at $45, $135; 3 months, 25 days, at $60, $228.39	363.39
1 clerk, 1 month 17 days, at $60	92.90
1 clerk, 12 months, at $55	660.00
1 clerk, 12 months, at $55	660.00
1 clerk, 2 months, at $55	110.00
1 clerk, 12 months, at $50	600.00
1 clerk, 12 months, at $50	600.00
1 clerk, 12 months, at $50	600.00
1 clerk, 9 months, at $50	450.00
1 clerk, 2 months, at $75, $150; 8 months, at $60, $480; 2 months, at $55, $110	740.00
1 stenographer, 3 months 18 days, at $100	364.29
1 typewriter, 12 months, at $50	600.00
1 copyist, 12 months, at $55	660.00
1 copyist, 12 months, at $50	600.00
1 copyist, 12 months, at $50	600.00
1 copyist, 12 months, at $50	600.00
1 copyist, 12 months, at $50	600.00
1 copyist, 4 months, at $46.66	186.64
1 copyist, 12 months, at $45	540.00
1 copyist, 3 months 8 days, at $40	130.67
1 copyist, 5 months 19 days, at $40	224.52
1 copyist, 12 months, at $40	480.00
1 copyist 8 months 16 days, at $40	340.65
1 copyist 12 months, at $40	480.00
1 copyist, 7 months 16 days, at $40	301.33
1 copyist, 2 months 28 days, at $40, $117.33; 29 days, at $1.50, $43.50	160.83
1 copyist, 12 months, at $35	420.00
1 copyist, 6 months 9 days, at $35	220.16
1 copyist, 11 months 10 days, at $35	396.29
1 copyist, 7 months 13 days, at $30	223.00
1 copyist, 12 months, at $30	360.00
1 copyist, 5 months 16 days, at $25	137.90
	34,836.83

Preparators:

1 colorist, 12 months, at $110	1,320.00
1 photographer, 12 months, at $158.33	1,899.96
1 taxidermist, 12 months, at $125	1,500.00
1 taxidermist, 3 months, at $115, $345; 4 months, at $40, $160; 5 months, at $15, $75	580.00
1 taxidermist, 12 months, at $80	960.00
1 assistant taxidermist, 8 months, at $60	480.00
1 assistant taxidermist, 12 months, at $60	720.00
1 assistant taxidermist, 10 months, at $60	600.00
1 assistant taxidermist, 12 months, at $60	720.00
1 assistant taxidermist, 3 months 29 days, at $50	196.77
1 preparator, 12 months, at $100	1,200.00
1 preparator, 12 months, at $80	960.00
1 preparator, 12 months, at $75	900.00

Preparators—Continued.

1 preparator, 12 months, at $60	$720.00
1 preparator, 2 months, at $120, $240; 2 months, at $105, $210; 8 months, at $80, $640	1,090.00
1 preparator, 179½ days, at $4 per diem	718.00
	14,564.73

Buildings and labor:

1 superintendent of buildings, 12 months, at $137.50	1,650.00
1 assistant superintendent of buildings, 12 months, at $90	1,080.00
1 watchman, 12 months, at $65	780.00
1 watchman, 12 months, at $60	720.00
1 watchman, 12 months, at $60	720.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 12 months, at $50	600.00
1 watchman, 10 months, at $50	500.00
1 watchman, 8 months 116 days, at $50	593.33
1 watchman, 1 month, at $40; 1 month, at $45; 8 months, at $50, $400; 19 days, at $50, $30.65	515.65
1 watchman, 10 months 19 days, at $50	530.65
1 watchman, 9 months 19 days, at $50	481.67
1 watchman, 12 months, at $45	540.00
1 watchman, 11 months 27 days, at $45	535.50
1 watchman, 12 months, at $45	540.00
1 skilled laborer, 10 months, at $70	700.00
1 skilled laborer, 12 months, at $50	600.00
1 skilled laborer, 4 months 25 days, at $50	244.64
1 skilled laborer, 6 months, at $50	300.00
1 skilled laborer, 3 months 25 days, at $40	153.33
1 skilled laborer, 54 days, at $2.50, $135; 154 days, at $2, $308	443.00
1 skilled laborer, 77 days, at $1.50	115.50
1 laborer, 6 months, at $45, $270; 169 days, at $1.50, $253 50	523.50
1 laborer, 10 months, at $45	450.00
1 laborer, 4 months, at $45	180.00
1 laborer, 9 months, at $40	360.00
1 laborer, 12 months, at $40	480.00
1 laborer, 9 months, at $40, $360; 2 days, at $1.50, $3	363.00
1 laborer, 12 months, at $40	480.00
1 laborer, 64 days, at $1.50	96.00
1 laborer, 11 months, at $40, $440; 19 days, at $47, $29.11; 35 days, at $1.50, $52.50	521.61
1 laborer, 312 days, at $1.50	468.00
1 laborer, 10 months, at $40, $400; 36 days, at $1.50, $54	454.00
1 laborer, 12 months, at $40, $480; 1 day, at $1.71, $1.71	481.71
1 laborer, 103½ days, $1.50	155.25
1 laborer, 12 months, at $40, $480; 3 days, at $1.50, $4.50	484.50
1 laborer, 317 days, at $1.50	475.50
1 laborer, 126½ days, at $1.50	189.75
1 laborer, 12 months, $40, at $480; 1 day, at $1.50	481.50
1 laborer, 100 days, at $1.50	150.00

Buildings and labor—Continued.

1 laborer, 329 days, at $1.50	$493.50
1 laborer, 315 days, at $1.50	472.50
1 laborer, 317½ days, at $1.50	476.00
1 laborer, 161 days, at $1.50	241.50
1 laborer, 254 days, at $1.50	381.00
1 laborer, 51 days, at $1.75	89.25
1 laborer, 12 months, at $40, $480; 1 day, at $1.50	481.50
1 attendant, 12 months less 1 day, at $40	478.71
1 attendant 11 months, at $40, $440; 1 month, at $35	475.00
1 cleaner, 12 months, at $30	360.00
1 cleaner, 155 days, at $1	155.00
1 cleaner, 270 days, at $1	270.00
1 cleaner, 12 months, at $30	360.00
1 cleaner, 12 months, at $30	360.00
1 messenger, 12 months, at $45	540.00
1 messenger, 12 months, at $45	540.00
1 messenger, 3 months, at $35	105.00
1 messenger, 12 months, at $25	300.00
1 messenger, 12 months, at $25	300.00
1 messenger, 9 months, at $25	225.00
1 messenger, 11 months 23 days, at $25	293.55
1 messenger, 2 months 29 days, at $20	59.33
1 messenger, 8 months 4 days, at $20	162.58
1 messenger, 12 months, at $20	240.00
1 messenger, 311 days, $1.25	388.75
	30,985.76

Temporary help.

Scientific staff:		
1 specialist, 26 days, at $150 per month	$125.81	
1 expert, 25 days, at $4 per diem	100.00	
1 aid, 1 month, 25 days, at $55 per month	99.35	
1 aid, 14 days, at $50 per month	23.33	
		348.49
Clerical staff:		
1 clerk, 1 month, at $45 per month	45.00	
1 typewriter, 17 days, at $60 per month	34.00	
1 typewriter, 30 days, at $35 per month	34.14	
1 copyist, 1 month, at $60	60.00	
1 copyist, 1 month 23 days, at $45	79.50	
1 copyist, 1 month, at $40	40.00	
1 copyist, 26 days, at $45	41.78	
		334.42
Preparators:		
1 taxidermist, 2 months, at $50 per month	100.00	
1 preparator, 24 days, at $40 per month	32.08	
1 preparator, 8 days, at $3.20 per diem	25.60	
		157.68
Buildings and labor:		
1 watchman, 1 month 15 days, at $50 per month	74.19	
1 skilled laborer, 2 months, at $45 per month	90.00	
1 laborer, 13 days, at $1.50 per diem	19.50	
1 laborer, 21 days, at $1.50 per diem	31.50	

Temporary help—Continued.

Buildings and labor—Continued.

1 laborer, 6 days, at $1.50 per diem	$9.00	
1 laborer, 5 days, at $1.50 per diem	7.50	
1 laborer, 25 days, at $1.50 per diem	37.50	
1 laborer, 10 days, at $1 per diem	10.00	
1 cleaner, 38 days, at $1 per diem	38.00	
		$317.19
		1,157.78
Special or contract work		1,363.68

SUMMARY.

Salaries, preservation of collections:		
Direction	3,999.96	
Scientific staff	31,470.25	
Clerical staff	34,836.83	
Preparators	14,564.73	
Buildings and labor	30,985.76	
Temporary help	1,157.78	
Special or contract work	1,363.68	
Total salaries or compensation		118,378.99
Miscellaneous:		
Supplies	4,952.67	
Stationery	2,307.60	
Specimens	5,141.48	
Books and periodicals	1,307.61	
Travel	1,646.42	
Freight and cartage	2,416.92	
		17,772.70
Total expenditure to June 30, 1890, preservation of collections		136,151.69
Balance July 1, 1890		3 848.31
Disallowance on a bill for travelling expenses		45
Balance July 1, 1890, to meet outstanding liabilities		3,848.76

FURNITURE AND FIXTURES, JULY 1, 1889, TO JUNE 30, 1890.

Appropriation by Congress for the fiscal year ending June 30, 1890, "for cases, furniture, fixtures, and appliances required for the exhibition and safe-keeping of the collections of the National Museum, including salaries or compensation of all necessary employés" (Sundry civil act, March 2, 1889. Public 154, p. 16)	30,000.00

Expenditures from July 1, 1889, *to June* 30, 1890.

Salaries or compensation:		
1 engineer of property, 12 months, at $150		1,800.00
1 clerk, 12 months, at $75	$900.00	
1 clerk, 3 months, at $50	150.00	
1 copyist, 12 months, at $55	660.00	
		1,710.00
1 carpenter, foreman, 12 months, at $91	1,092.00	
1 cabinet-maker, 313 days, at $3	939.00	
1 carpenter, 124½ days, at $3	373.50	
1 carpenter, 90 days, at $3	270.00	
1 carpenter, 286 days, at $3	858.00	
1 carpenter, 52 days, at $3	156.00	

Salaries or compensation—Continued.

1 carpenter, 192½ days, at $3	$577.50	
1 carpenter, 150 days, at $3	450.00	
1 carpenter, 311 days at $3	933.00	
1 carpenter, 99½ days, at $3	298.50	
1 carpenter, 47 days, at $3	141.00	
1 carpenter, 13 days, at $3	39.00	
1 carpenter, 37½ days, at $3	112.50	
1 carpenter, 3 days, at $3	9.00	
		$6,249.00
1 painter, 12 months, at $65	780.00	
1 painter, 248 days, at $2	496.00	
		1,276.00
1 skilled laborer, 54 days, at $1.50, $81; 208 days, at $1.75, $364	445.00	
1 skilled laborer, 12 months, at $50	600.00	
1 skilled laborer, 6 months, at $50	300.00	
1 skilled laborer, 4 months and 30 days, at $50, $248.39; 3 days, at $1.50, $3	251.39	
1 skilled laborer, 295 days, at $2	590.00	
1 skilled laborer, 309 days, at $2	618.00	
1 skilled laborer, 104 days, at $2	208.00	
1 skilled laborer, 10 months, at $45	450.00	
		3,462.39
1 laborer, 8 months, at $45, $360; 6 days, at $1.50, $9	369.00	
1 laborer, 1 month 8 days, at $40	50.32	
1 laborer, 3 months, at $40, $120; 1 day, at $1.50, $1.50	121.50	
1 laborer, 230 days, at $1.50	345.00	
1 laborer, 6 months, at $40, $240; 2 days, at $1.50, $3	243.00	
1 laborer, 1 month, at $40	40.00	
1 laborer, 3 months, at $40	120.00	
1 laborer, 1 month, at $30	30.00	
1 cleaner, 3 months, at $30	90.00	
		1,408.82
		15,906.21
Contract repairing elevator		20.00
Total expenditures for salaries or compensation		15,926.21

Materials, etc.:

Exhibition cases	$4,366.77
Designs and drawings for cases	57.00
Drawers, trays, boxes	931.48
Frames, stands, miscellaneous wood work	158.84
Glass	1,875.38
Hardware and interior fittings for cases	1,291.07
Tools	107.37
Cloths, cotton, etc	85.97
Glass jars, etc	395.45
Lumber	1,276.88
Paints, oils, brushes	681.68
Office furniture	605.19
Chairs (for halls)	51.00
Tin, lead	90.98
Brick, plaster	98.00
Rubber goods	40.87
Iron brackets	130.00

Materials, etc.—Continued.

Apparatus	$605.50	
Travelling expenses	31.95	
		$12,861.38
Total expenditure, July 1, 1889, to June 30, 1890, furniture and fixtures		28,807.59
Balance, July 1, 1890, to meet outstanding liabilities		1,192.41

HEATING, LIGHTING, ELECTRIC, AND TELEPHONIC SERVICE FROM JULY 1, 1889, TO JUNE 30, 1890.

Appropriation by Congress for the fiscal year ending June 30, 1890, "for expenses of heating, lighting, and electrical and telephonic service for the National Museum" (Sundry civil act, March 2, 1889. Public 154, p. 16) $12,000.00

Expenditures from July 1, 1889, *to June* 30, 1890.

Salaries or compensation:			
1 engineer, 8 months 23 days, at $120		$1,049.03	
1 fireman, 10 months 58 days, at $50		595.06	
1 fireman, 7 months 112 days, at $50		536.87	
1 fireman, 12 days, at $47.50 per month		18.39	
1 fireman, 12 months, at $50		600.00	
1 fireman, 12 months, at $50		600.00	
1 fireman, 11 months 28 days, at $50		596.67	
1 fireman, 12 days, at $50 per month		19.35	
1 fireman, 2 months, at $40		80.00	
1 telephone clerk, 3 months, at $35, $105; 54 days, at $1.75, $94.50		199.50	
1 telephone clerk, 12 months, at $60		720.00	
1 inspector, job		100.00	
Total expenditures for salaries		5,114.47	
General expenses:			
Coal and wood	$2,058.26		
Gas	1,113.82		
Telephones	601.05		
Electric work	154.40		
Electrical supplies	110.09		
Rental of call-boxes	100.00		
Heating repairs	269.25		
Heating supplies	147.86		
Travel	3.25		
		4,557.98	
Total expenditures, July 1, 1888, to June 30, 1890, heating, lighting, etc			9,672.85
Balance, July 1, 1890, to meet outstanding liabilities			2,327.15

POSTAGE, JULY 1, 1889, TO JUNE 30, 1890.

Appropriation by Congress for the fiscal year ending June 30, 1890, "for postage stamps and foreign postal cards for the National Museum" (Sundry civil act, March 2, 1889. Public 154, p. 16) 1,000.00

Expenditures from July 1, 1889, to June 30, 1890.

For postage stamps and postal cards		$500.00
Balance July 1, 1890		500.00

PRINTING, JULY 1, 1889, TO JUNE 30, 1890.

Appropriation by Congress for the fiscal year ending June 30, 1890, "for printing labels and blanks for the use of the National Museum and for the Bulletins and annual volumes of the proceedings of the Museum" (Sundry civil act, March 2, 1889. Public 154, p. 45)	$10,000.00	
Appropriation in deficiency act approved December 19, 1889 (Public 1, p. 1)	745.16	
		10,745 16

Expenditure from July 1, 1889, to July 30, 1890.

Bulletins Nos. 34, 35, 36, 37	$3,235.94	
Proceedings, vols. XI, XII, XIII	3,137.99	
Extras from Museum Reports	744.43	
Circulars	44.40	
Labels for specimens	2,197.01	
Letter heads, memorandum pads, and envelopes	318.74	
Blanks, time books, order books, etc	832.54	
Catalogue cards	121.56	
Congressional Records	48.00	
Total expenditure July 1, 1889, to June 30, 1890, printing, Museum		10,680.61
Balance July 1, 1890		64.55

OTHER MUSEUM APPROPRIATIONS.

PRESERVATION OF COLLECTIONS, 1887–'88.

Balance July 1, 1889, as per last report		42.69

Expenditures from July 1, 1889, to June 30, 1890.

Books	$3.46	
Travel	37.65	
Services	.21	
		41.32
Balance		1.37

FURNITURE AND FIXTURES, 1887–'88.

Balance July 1, 1889, as per last report	21.96

HEATING, LIGHTING, ETC., 1887–'88.

Balance July 1, 1889, as per last report	3.70

The above balances, $1.37, $21.96, $3.70, were carried under the action of Revised Statutes, section 3090, by the Treasury Department, to the credit of the surplus fund June 30, 1890.

PRESERVATION, 1888-'89.

Balance July 1, 1889, as per last report		$4,198.34

Expenditures from July 1, 1889, *to June* 30, 1890.

Salaries or compensation	$154.99	
Supplies	1,032.82	
Stationery	58.49	
Specimens	2,017.19	
Books	489.43	
Travel	65 64	
Freight	364.60	
Total expenditure		4,183.16
Balance		15.18

FURNITURE AND FIXTURES, 1888-'89.

Balance July 1, 1889, as per last report		2,823.22

Expenditures from July 1, 1889, *to June* 30, 1890.

Exhibition cases	$525.74	
Drawings	65.00	
Drawers, trays, boxes	650.20	
Frames, stands, woodwork	36.10	
Hardware and tools	569.47	
Cloth	69.11	
Glass jars	62.50	
Lumber	186.84	
Paints	4.25	
Office furniture	42.98	
Metal work	431.68	
Slate, brick, plaster	148.50	
Travel	5.45	
Camera	25.00	
		2,822.82
Balance deposited in the U. S. Treasury, May 31, 1890		.40

HEATING, LIGHTING, ETC., 1888-'89.

Balance July 1, 1889		1,089.33

Expenditures from July 1, 1889, *to June* 30, 1890.

Gas	$77.26	
Telephones	200.00	
Electric work	578.00	
Rental of call boxes	10.00	
Heating repairs	220.08	
		1,085.34
Balance		3.99

NATIONAL ZOÖLOGICAL PARK.

Appropriation by Congress "for the organization, improvement, and maintenance of the National Zoölogical Park."

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That the one-half of the following sums named respectively is hereby appropriated out of any money in the Treasury not otherwise appropriated, and the other half out of the revenues of the District of Columbia, for the organization, improvement, and maintenance of the National Zoölogical Park, to be expended under the direction of the Regents of the Smithsonian Institution, and to be drawn on their requisition and disbursed by the disbursing officer of said Institution:

For the shelter of animals	$15,000.00
For shelter barns, cages, fences, and inclosures, and other provisions for the custody of animals	9,000.00
For repairs to the Holt mansion to make the same suitable for occupancy, and for office furniture	2,000.00
For the creation of artificial ponds and other provisions for aquatic animals	2,000.00
For water supply, sewerage, and drainage	7,000.00
For roads, walks, and bridges	15,000.00
For miscellaneous supplies, materials, and sundry incidental expenses not otherwise provided for	5,000.00
For current expenses, including the maintenance of collections, food supplies, salaries of all necessary employés, and the acquisition and transportation of specimens	37,000.00

SEC. 2. That the National Zoölogical Park is hereby placed under the direction of the Regents of the Smithsonian Institution, who are authorized to transfer to it any living specimens, whether of animals or plants, now or hereafter in their charge; to accept gifts for the park, at their discretion, in the name of the United States; to make exchanges of specimens, and to administer the said Zoölogical Park for the advancement of science and the instruction and recreation of the people.

SEC. 3. That the heads of Executive Departments of the Government are hereby authorized and directed to cause to be rendered all necessary and practical aid to the said Regents in the acquisition of collections for the Zoölogical Park.

Approved April 30, 1890.

92,000.00

Expenditures from April 30, 1890, *to June* 30, 1890.

Shelter barns, cages, etc	$43 83	
Miscellaneous supplies	157 57	
Current expenses	717 10	
Total expenditures National Zoölogical Park		918.50
Balance July 1, 1890		91,081.50

RECAPITULATION.

The total amount of the funds administered by the Institution during the year ending June 30, 1890, appears, from the foregoing statements and the account books, to have been as follows:

From balance of last year, July 1, 1889	$11,757.47
From interest on Smithsonian fund for the year	42,180.00
From sales of publications	416.01
From repayments for freights, etc	3,489.50
From special gifts for astrophysical research	10,000.00
Total	67,842.98

Appropriations committed by Congress to the care of the Institution.

International exchanges:			
From balance of last year, July 1, 1889	$21.80		
Appropriation for 1889-'90	15,000.00		
Total		$15,021.80	
North American ethnology:			
From balance of last year, July 1, 1889	$13,491.22		
Appropriation for 1889-'90	40,000.00		
Total		53,491.22	
Preservation of collections:			
From balance of 1887-'88, July 1, 1889	$42.69		
From balance of 1888-'89, July 1, 1889	4,198.34		
From appropriation for 1889-'90	140,000.00		
Total		144,241.03	
Furniture and fixtures:			
From balance of 1887-'88, July 1, 1889	$18.71		
From balance of 1888-'89, July 1, 1889	2,823.22		
From appropriation for 1889-'90	30,000.00		
Total		32,841.93	
Heating, lighting, etc.:			
From balance of 1887-'88, July 1, 1889	$3.70		
From balance of 1888-'89, July 1, 1889	1,089.33		
From appropriation for 1889-'90	12,000.00		
Total		13,093.03	
Postage:			
From appropriation for 1889-'90		1,000.00	
Printing:			
From appropriation for 1889-'90		10,745.16	
National Zoölogical Park:			
From appropriation of April 30, 1890		92,000.00	
			$362,434.17
Grand total			454,277.15

The committee has examined the vouchers for payments made from the Smithsonian income during the year ending June 30, 1890, all of which bear the approval of the Secretary of the Institution, or, in his absence, of the Assistant Secretary as acting Secretary, and a certificate that the materials and services charged were applied to the purposes of the Institution.

The committee has also examined the accounts of the "International Exchanges," and of the "National Museum," and of the "National Zoölogical Park," and finds that the balances above given correspond with the certificates of the disbursing clerk of the Smithsonian Institution, whose appointment as such disbursing officer was accepted and his bonds approved by the Secretary of the Treasury.

The quarterly accounts current, the vouchers, and journals have been examined and found correct.

The abstracts of expenditures and balance sheets under the appropriation for "North American Ethnology" have been exhibited to us; the

vouchers for the expenditures, after approval by the Director of the Bureau of Ethnology, are paid by the disbursing clerk of said Bureau, and, after approval by the Secretary of the Smithsonian Institution, are transmitted to the accounting officers of the Treasury Department for settlement. The disbursing officer of the Bureau is accepted as such, and his bonds approved by the Secretary of the Treasury. The balance available to meet outstanding liabilities on July 1, 1890, as reported by the disburing clerk of the Bureau, is $12,033.08.

Statement of regular income from the Smithsonian Fund to be available for use in the year ending June 30, 1890.

Balance on hand June 30, 1891		$30, 192. 65
Interest due and receivable July 1, 1890	$21, 090. 00	
Interest due and receivable January 1, 1891	21, 090. 00	
		42, 180. 00
Total available for year ending June 30, 1891		72, 372. 65

Respectfully submitted

JAMES C. WELLING,
HENRY COPPÉE,
M. C. MEIGS,
Executive Committee.

WASHINGTON, D. C., *November*, 1890.

ACTS AND RESOLUTIONS OF CONGRESS RELATIVE TO THE SMITHSONIAN INSTITUTION, NATIONAL MUSEUM, ETC.

(In continuation from previous reports.)

[Fifty-first Congress, first session, 1889-'90.]

CHAP. 156.—AN ACT to provide for celebrating the four hundredth anniversary of the discovery of America by Christopher Columbus by holding an international exhibition of arts, industries, manufactures, and the product of the soil, mine, and sea in the city of Chicago, in the State of Illinois.

Whereas, It is fit and appropriate that the four hundredth anniversary of the discovery of America be commemorated by an exhibition of the resources of the United States of America, their development, and of the progress of civilization of the New World; and

Whereas, Such an exhibition should be of a national and international character, so that not only the people of our Union and this continent, but those of all nations as well, can participate, and should therefore have the sanction of the Congress of the United States; Therefore,

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That an exhibition of arts, industries, manufactures, and the products of the soil, mine, and sea shall be inaugurated in the year eighteen hundred and ninety-two, in the city of Chicago, in the State of Illinois, as hereinafter provided.

SEC. 2. That a commission, to consist of two commissioners from each State and Territory of the United States and from the District of Columbia and eight commissioners at large, is hereby constituted to be designated as the World's Columbian Commission.

SEC. 3. That said commissioners, two from each State and Territory, shall be appointed within thirty days from the passage of this act by the President of the United States, on the nomination of the governors of the States and Territories, respectively, and by the President eight commissioners at large and two from the District of Columbia; and in the same manner and within the same time there shall be appointed two alternate commissioners from each State and Territory of the United States and the District of Columbia and eight alternate commissioners at large, who shall assume and perform the duties of such commissioner or commissioners as may be unable to attend the meetings of the said commission; and in such nominations and appointments each of the two leading political parties shall be equally represented. Vacancies in the commission nominated by the governors of the several States and Territories, respectively, and also vacancies in the commission at large and from the District of Columbia may be filled in the same manner and under the same conditions as provided herein for their original appointment.

SEC. 4. That the Secretary of State of the United States shall, immediately after the passage of this act, notify the governors of the several States and Territories, respectively, thereof and request such nominations to be made. The commissioners so appointed shall be called together by the Secretary of State of the United States in the city of Chicago, by notice to the commissioners, as soon as convenient after the appointment of said commissioners, and within thirty days thereafter. The said commissioners, at said first meeting, shall organize by the election of such officers and the appointment of such committees as they may deem expedient, and for this purpose the commissioners present at said meeting shall constitute a quorum.

SEC. 5. That said commission be empowered in its discretion to accept for the purposes of the World's Columbian Exposition such site as may be selected and offered and such plans and specifications of buildings to be erected for such purpose at the expense of and tendered by the corporation organized under the laws of the State of Illinois, known as "The World's Exposition of eighteen hundred and ninety-two:" *Provided*, That said site so tendered, and the buildings proposed to be erected thereon shall be deemed by said commission adequate to the purposes of said exposition: *And provided*, That said commission shall be satisfied that the said corporation has an actual bona fide and valid subscription to its capital stock which will secure the payment of at least five millions of dollars, of which not less than five hundred thousand dollars shall have been paid in, and that the further sum of five million dollars, making in all ten million dollars, will be provided by said corporation in ample time for its needful use during the prosecution of the work for the complete preparation for said exposition.

SEC. 6. That the said commission shall allot space for exhibitors, prepare a classification of exhibits, determine the plan and scope of the exposition, and shall appoint all judges and examiners for the exposition, award all premiums, if any, and generally have charge of all intercourse with the exhibitors and the representatives of foreign nations. And said commission is authorized and required to appoint a board of lady managers of such number and to perform such duties as may be prescribed by said commission. Said board may appoint one or more members of all committees authorized to award prizes for exhibits, which may be produced in whole or in part by female labor.

SEC. 7. That after the plans for said exposition shall be prepared by said corporation and approved by said commission, the rules and regulations of said corporation governing rates for entrance and admission fees, or otherwise affecting the rights, privileges, or interests of the exhibitors or of the public, shall be fixed or established by said corporation, subject, however, to such modification, if any, as may be imposed by a majority of said commissioners.

SEC. 8. That the President is hereby empowered and directed to hold a naval review in New York Harbor, in April, eighteen hundred and ninety-three, and to extend to foreign nations an invitation to send ships of war to join the United States Navy in rendezvous at Hampton Roads and proceed thence to said review.

SEC. 9. That said commission shall provide for the dedication of the buildings of the World's Columbian Exposition in said city of Chicago on the twelfth day of October, eighteen hundred and ninety-two, with appropriate ceremonies, and said exposition shall be open to visitors not later than the first day of May, eighteen hundred and ninety-three,

and shall be closed at such time as the commission may determine, but not later than the thirtieth day of October thereafter.

SEC. 10. That whenever the President of the United States shall be notified by the commission that provision has been made for grounds and buildings for the uses herein provided for and there has also been filed with him by the said corporation, known as "The World's Exposition of eighteen hundred and ninety-two," satisfactory proof that a sum not less than ten million dollars to be used and expended for the purposes of the exposition herein authorized, has in fact been raised or provided for by subscription or other legally binding means, shall be authorized, through the Department of State, to make proclamation of the same, setting forth the time at which the exposition will open and close, and the place at which it will be held; and he shall communicate to the diplomatic representatives of foreign nations copies of the same, together with such regulations as may be adopted by the commission, for publication in their respective countries, and he shall, in behalf of the Government and people, invite foreign nations to take part in the said exposition and appoint representatives thereto.

SEC. 11. That all articles which shall be imported from foreign countries for the sole purpose of exhibition at said exposition, upon which there shall be a tariff or customs duty, shall be admitted free of payment of duty, customs fees, or charges under such regulations as the Secretary of the Treasury shall prescribe; but it shall be lawful at any time during the exhibition to sell for delivery at the close of the exposition any goods or property imported for and actually on exhibition in the exposition buildings or on its grounds, subject to such regulations for the security of the revenue and for the collection of the import duties as the Secretary of the Treasury shall prescribe: *Provided*, That all such articles when sold or withdrawn for consumption in the United States shall be subject to the duty, if any, imposed upon such articles by the revenue laws in force at the date of importation, and all penalties prescribed by law shall be applied and enforced against such articles, and against the persons who may be guilty of any illegal sale or withdrawal.

SEC. 12. That the sum of twenty thousand dollars, or as much thereof as may be necessary, be, and the same is hereby, appropriated, out of any moneys in the Treasury not otherwise appropriated, for the remainder of the present fiscal year and for the fiscal year ending June thirtieth, eighteen hundred and ninety-one, to be expended under the direction of the Secretary of the Treasury for purposes connected with the admission of foreign goods to said exhibition.

SEC. 13. That it shall be the duty of the commission to make report from time to time, to the President of the United States of the progress of the work, and, in a final report, present a full exhibit of the results of the exposition.

SEC. 14. That the commission hereby authorized shall exist no longer than until the first day of January, eighteen hundred and ninety-eight.

SEC. 15. That the United States shall not in any manner, nor under any circumstances, be liable for any of the acts, doings, proceedings or representations of the said corporation organized under the laws of the State of Illinois, its officers, agents, servants, or employes, or any of them, or for the service, salaries, labor, or wages of said officers, agents, servants, or employes, or any of them, or for any subscriptions to the capital stock, or for any certificates of stock, bonds, mortgages, or obligations of any kind issued by said corporation or for any debts,

liabilities, or expenses of any kind whatever attending such corporation or accruing by reason of the same.

SEC. 16. That there shall be exhibited at said exposition by the Government of the United States, from its Executive Departments the Smithsonian Institution, the United States Fish Commission, and the National Museum, such articles and materials as illustrate the function and administrative faculty of the Government in time of peace and its resources as a war power, tending to demonstrate the nature of our institutions and their adaptation to the wants of the people; and to secure a complete and harmonious arrangement of such a Government exhibit, a board shall be created to be charged with the selection, preparation, arrangement, safe-keeping, and exhibition of such articles and materials as the heads of the several Departments and the directors of the Smithsonian Institution and the National Museum may respectively decide shall be embraced in said Government exhibit. The President may also designate additional articles for exhibition. Such board shall be composed of one person to be named by the head of each Executive Department, and one by the directors of the Smithsonian Institution and the National Museum, and one by the Fish Commission, such selections to be approved by the President of the United States. The President shall name the chairman of said board, and the board itself shall select such other officers as it may deem necessary.

That the Secretary of the Treasury is hereby authorized and directed to place on exhibition, upon such grounds as shall be allotted for the purpose, one of the life-saving stations authorized to be constructed on the coast of the United States by existing law, and to cause the same to be fully equipped with all apparatus, furniture, and appliances now in use in all life-saving stations in the United States, said building and apparatus to be removed at the close of the exhibition and re-erected at the place now authorized by law.

SEC. 17. That the Secretary of the Treasury shall cause a suitable building or buildings to be erected on the site selected for the World's Columbian Exposition for the Government exhibits, as provided in this act, and he is hereby authorized and directed to contract therefor, in the same manner and under the same regulations as for other public buildings of the United States; but the contracts for said building or buildings shall not exceed the sum of four hundred thousand dollars, and for the remainder of the fiscal year and for the year ending June thirtieth, eighteen hundred and ninety-one, there is hereby appropriated for said building or buildings, out of any money in the Treasury not otherwise appropriated, the sum of one hundred thousand dollars. The Secretary of the Treasury shall cause the said building or buildings to be constructed as far as possible, of iron, steel, and glass, or of such other material as may be taken out and sold to the best advantage; and he is authorized and required to dispose of such building or buildings, or the material composing the same, at the close of the exposition, giving preference to the city of Chicago, or to the said World's Exposition of eighteen hundred and ninety-two to purchase the same at an appraised value to be ascertained in such manner as he may determine.

SEC. 18. That for the purpose of paying the expenses of transportation, care, and custody of exhibits by the Government and the maintenance of the building or buildings hereinbefore provided for and the safe return of articles belonging to the said Government exhibit, and for the expenses of the commission created by this act, and other contingent expenses, to be approved by the Secretary of the Treasury, upon itemized accounts and vouchers, there is hereby appropriated for the

emainder of this fiscal year and for the fiscal year ending June thirtieth, ighteen hundred and ninety-one, out of any money in the Treasury ıot otherwise appropriated, the sum of two hundred thousand dollars, r so much thereof as may be necessary: *Provided*, That the United States shall not be liable, on account of the erection of buildings, expenses of the commission or any of its officers or employees, or on account f any expenses incident to or growing out of said exposition for a sum xceeding in the aggregate one million five hundred thousand dollars.

SEC. 19. That the commissioners and alternate commissioners appointed under this act shall not be entitled to any compensation for heir services out of the Treasury of the United States, except their ctual expenses for transportation and the sum of six dollars per day or subsistence for each day they are necessarily absent from their omes on the business of said commission. The officers of said commission shall receive such compensation as may be fixed by said commission, subject to the approval of the Secretary of the Treasury, which hall be paid out of the sums appropriated by Congress in aid of such xposition.

SEC. 20. That nothing in this act shall be so construed as to create ny liability of the United States, direct or indirect, for any debt or bligation incurred, nor for any claim for aid or pecuniary assistance rom Congress or the Treasury of the United States in support or liquiation of any debts or obligations created by said commission in excess f appropriations made by Congress therefor.

SEC. 21. That nothing in this act shall be so construed as to override r interfere with the laws of any State, and all contracts made in any State for the purposes of the exhibition shall be subject to the laws hereof.

SEC. 22. That no member of said commission, whether an officer or therwise, shall be personally liable for any debt or obligation which ıay be created or incurred by the said commission.

Approved, April 25, 1890.

HAP. 173.—AN ACT for the organization, improvement, and maintenance of the National Zoological Park.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That the one-half of the following sums named, respectively, is hereby appropriated, out of any money ı the Treasury not otherwise appropriated, and the other half out of he revenues of the District of Columbia, for the organization, improveıent, and maintenance of the National Zoological Park, to be expended nder the direction of the Regents of the Smithsonian Institution, and o be drawn on their requisition and disbursed by the disbursing officer or said Institution:

For the shelter of animals, fifteen thousand dollars.

For shelter-barns, cages, fences, and inclosures, and other provisions or the custody of animals, nine thousand dollars.

For repairs to the Holt mansion, to make the same suitable for occuancy, and for office furniture, two thousand dollars.

For the creation of artificial ponds and other provisions for aquatic nimals, two thousand dollars.

For water supply, sewerage, and drainage, seven thousand dollars.

For roads, walks, and bridges, fifteen thousand dollars.

For miscellaneous supplies, materials, and sundry incidental expenses not otherwise provided for, five thousand dollars.

For current expenses, including the maintenance of collections, food supplies, salaries of all necessary employees, and the acquisition and transportation of specimens, thirty-seven thousand dollars.

SEC. 2. That the National Zoological Park is hereby placed under the directions of the Regents of the Smithsonian Institution, who are authorized to transfer to it any living specimens, whether of animals or plants, now or hereafter in their charge, to accept gifts for the park at their discretion, in the name of the United States, to make exchanges of specimens, and to administer the said Zoological Park for the advancement of science and the instruction and recreation of the people.

SEC. 3. That the heads of executive departments of the Government are hereby authorized and directed to cause to be rendered all necessary and practicable aid to the said regents in the acquisition of collections for the Zoological Park.

Approved, April 30, 1890.

SMITHSONIAN INSTITUTION.

INTERNATIONAL EXCHANGES: For expenses of the system of international exchanges between the United States and foreign countries, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees, seventeen thousand dollars.

NORTH AMERICAN ETHNOLOGY: For continuing ethnological researches among the American Indians, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees, forty thousand dollars.

REPAIRS, SMITHSONIAN BUILDING: For fire-proofing the so-called chapel of the west wing of the Smithsonian Building, and for repairing the roof of the main building and the ceiling and plastering of the main Hall of the building, twenty-five thousand dollars, said work to be done under the supervision of the Architect of the Capitol, with the approval of the Regents of the Smithsonian Institution, and no portion of the appropriation to be used for sky-lights in the roof nor for well-hole in the floor of the main building.

UNDER THE SECRETARY OF THE SMITHSONIAN INSTITUTION AS DIRECTOR OF THE NATIONAL MUSEUM.

HEATING AND LIGHTING: For expense of heating, lighting, electrical, telegraphic, and telephonic service for the National Museum, twelve thousand dollars.

PRESERVATION OF COLLECTIONS OF THE NATIONAL MUSEUM: For continuing the preservation, exhibition, and increase of the collections from the surveying and exploring expeditions of the government, and from other sources, including salaries or compensation of all necessarp employees, one hundred and forty thousand dollars.

FURNITURE AND FIXTURES OF THE NATIONAL MUSEUM: For cases, furniture, fixtures, and appliances required for the exhibition and safe-keeping of the collections of the National Museum, including salaries or compensation of all necessary employees, twenty-five thousand dollars.

POSTAGE FOR THE NATIONAL MUSEUM: For postage stamps and foreign postal cards for the National Museum, five hundred dollars.

PRINTING FOR THE NATIONAL MUSEUM: For the Smithsonian Institution, for printing labels and blanks for the use of the National

Museum and for the "Bulletins" and annual volumes of the "Proceedings" of the National Museum, ten thousand dollars.

EXCHANGES OF THE GEOLOGICAL SURVEY: For the purchase of necessary books for the library, and the payment for the transmission of public documents through the Smithsonian exchange, five thousand dollars.

(Sundry civil appropriation act, approved August 30, 1890.)

MISCELLANEOUS: To re-imburse the Smithsonian Institution for expenses incurred in the exchange of the publications of the Fish Commission for those of foreign countries, being for the service of the fiscal year, eighteen hundred and eighty-nine, two hundred and fifteen dollars and twenty cents.

To enable the Secretary of the Smithsonian Institution to purchase from Frederick S. Perkins, of Wisconsin, his collection of prehistoric copper implements, seven thousand dollars.

Preservation of collections, National Museum: To supply a deficiency in the appropriation for preservation of collections, National Museum, for the fiscal year eighteen hundred and eighty-seven, eleven dollars and forty-five cents.

Claims allowed by the First Comptroller, Treasury Department:

For international exchanges; Smithsonian Institution, one dollar and five cents.

(Deficiency appropriation act, approved September 30, 1890.)

APPOINTMENT OF REGENTS OF THE SMITHSONIAN INSTITUTION.

No. 23.—Joint resolution to fill vacancies in the Board of Regents of the Smithsonian Institution:

Resolved by the Senate and House of Representatives of the United States, etc.—That the vacancies in the Board of Regents of the Smithsonian Institution, of the class other than members of Congress, shall be filled by the appointment of Charles Devens, of Massachusetts, in the place of Noah Porter, of Connecticut, resigned; and by the reappointment of James C. Welling, of Washington City, whose term of office has expired.

Approved May 22, 1890.

REPORT OF S. P. LANGLEY,

SECRETARY OF THE SMITHSONIAN INSTITUTION, FOR THE YEAR ENDING JUNE 30, 1890.

To the Board of Regents of the Smithsonian Institution:

GENTLEMEN: I have the honor to submit herewith the report for the year ending June 30, 1890, of the operations of the Smithsonian Institution, and of the work placed by Congress under its charge in the National Museum, the Bureau of Ethnology, the International Exchanges, and the National Zoological Park.

The National Zoological Park has been formally placed under the care of the Board of Regents during this year,* although its establishment has been under consideration for some time and the preliminary steps connected therewith have been referred to in previous reports.

THE SMITHSONIAN INSTITUTION.

THE ESTABLISHMENT.

By the organizing act of Congress of August 10, 1846, sec. 1,† it was provided that "The President, and Vice-President of the United States, the Secretary of State, the Secretary of the Treasury, the Secretary of War, the Secretary of the Navy, the Postmaster-General, the Attorney-General, the Chief-Justice, and the Commissioner of the Patent Office of the United States, and the Mayor of the city of Washington, during the time for which they shall hold their respective offices, and such other persons as they may elect honorary members, be, and they are hereby constituted an 'establishment' by the name of the 'Smithsonian Institution,'" etc. In the Revised Statutes "the Governor of the District of Columbia" was substituted for the Mayor of the city of Washington, the latter office having become extinct.

Two members having been added to the cabinet of the President since the passage of the act, namely, the Secretary of the Interior, and more recently the Secretary of Agriculture, there appears no good reason why these should not be included in the list of officers of the establishment. This would obviously be consonant with the original intention of the framers of the act, though excluded by the phraseology actually employed. It may be worthy of consideration of the Board of Regents whether it would not be for the interests of the Institution to ask of Congress a re-construction of the section referred to, whereby

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*Act of Congress approved April 30, 1890.

†Title lxxiii, sec. 5579, of the Revised Statutes.

the President, Vice-President, Secretaries of the several Executive Departments, and the Chief Justice of the United States shall constitute the Establishment.

THE BOARD OF REGENTS.

The stated annual meeting of the Board was held on January 8, 1890, at which the resignation of Dr. Noah Porter, presented on account of failing health, was accepted in the following resolution:

Resolved, That the Board having received the resignation of Dr. Noah Porter as a Regent accept it with an expression of their regret, and with assurances of their high personal esteem.

At the same meeting, the appointment by the honorable the Speaker of the House of Representatives on January 6, 1890, of the following members of the House as Regents was announced: the Hon. Benjamin Butterworth, of Ohio, the Hon. Henry Cabot Lodge, of Massachusetts, the Hon. Joseph Wheeler, of Alabama.

The death of the Hon. Samuel S. Cox, for many years a Regent of the Institution, and its earnest friend and supporter, was referred to in my last annual report. By a resolution of the Board of Regents a committee was appointed, of which the Secretary was made chairman, to prepare suitable resolutions on his services and character, and these formal resolutions, with a brief biographical sketch, are given in full in the "necrology" appended.

The institution is indebted to Mrs. Cox for a portrait of her husband to be placed with the collection of portraits of past Regents.

By joint resolution of Congress, approved by the President May 22, 1890, Dr. James C. Welling, whose term as a Regent had expired, was re-elected; and by the same resolution Judge Charles Devens, of Massachusetts, was appointed a member of the Board to succeed Dr. Porter.

I regret to say that Judge Devens has written to me to state that there is a provision in the constitution of Massachusetts in reference to judges of its supreme court, which it has been suggested would prevent any one of them from holding such a position. No action had been taken in the matter at the time of this report.

FINANCES.

The permanent funds of the institution remain as at the time of my last report, namely:

Bequest of Smithson, 1846	$515,169.00
Residuary legacy of Smithson, 1867	26,210.63
Deposits from savings of income, etc., 1867	108,620.37
Bequest of James Hamilton, 1874	1,000.00
Bequest of Simeon Habel, 1880	500.00
Deposit from proceeds of sale of bonds, 1881	51,500.00
Total permanent Smithsonian fund in the Treasury of the United States, bearing interest at 6 per cent. per annum	703,000.00

It seems to me desirable in this connection to direct attention to the exceptional advantages offered in the organization of the Smithsonian Institution for the administration of funds intended for the advancement of science and the increase of knowledge throughout the world. The governing board of the Institution is composed of the highest officers of the United States Government, associated with some of the most distinguished men of learning in the country. The United States Government is itself pledged to the security of the funds of the Institution, guaranteeing an interest of six per cent. annually.

It is safe to say that no institution of learning is better known throughout the world, and I am impressed with the belief that were it also more widely known that the United States, in accepting the gift of Smithson, has signified a willingness to become the custodian of further bequests for the increase and diffusion of knowledge, its permanent endowment would be constantly increased.

The principal facts in relation to Smithson's bequest have been stated in brief in my previous reports and elsewhere at considerable length, and need not be repeated here.

At the beginning of the fiscal year the balance on hand of the income was $11,757.47. Interest on the invested fund, amounting to $42,180, has been received from the Treasurer of the United States, $5,000 have been received from the estate of the late Dr. Jerome H. Kidder, and a like amount from Dr. Alexander Graham Bell for the prosecution of special researches in physics, to which allusion is elsewhere made, and $3,905.51 have been received from miscellaneous sources, making the total receipts $67,842.98.

The total expenditures have been $37,650.33, leaving an unexpended balance on June 30, 1890, of $30,192.65, or, deducting the donations for special researches noted above, amounting to $10,000, the balance available for general expenses on July 1, 1890, was $20,192.65. This sum, which is somewhat larger than usual, is in part held against certain anticipated grants in aid of scientific investigation and the cost of their publication by the Institution.

The Institution has been charged by Congress with the disbursement during the year of the following special appropriations:

For international exchanges	$15,000
For ethnological researches	40,000
For National Museum:	
Preservation of collections	140,000
Furniture and fixtures	30,000
Heating and lighting	12,000
Postage	1,000
Printing	10,000
For National Zoological Park	92,000

The vouchers for the disbursement of these appropriations, with the exception of those for "ethnological researches," have been examined by the Executive Committee, and the various items of expenditure, in-

cluding those of the Bureau of Ethnology, are set forth in a letter addressed to the Speaker of the House of Representatives in accordance with a provision of the sundry civil act of October 2, 1888, while the expenditures from the Smithsonian fund, having likewise been examined and approved by the executive committee, are given in their report.

The estimates for the fiscal year ending June 30, 1891, forwarded to the Secretary of the Treasury under date of October 1, 1889, were as follows:

International exchanges	$27,500.00
North American ethnology	50,000.00
National Museum:	
Preservation of collections	175,000.00
Heating and lighting	15,000.00
Furniture and fixtures	30,000.00
Living animals, in connection with zoological department	50,000.00
Printing and binding	18,500.00
Postage	500.00

BUILDINGS.

I regret that I am unable to report any immediate prospect of relief from the over-crowded condition of the Museum building. The Regents nearly eight years ago, (at their meeting of January 17, 1883,) recommended to Congress the erection of a new Museum building, and the previous steps taken in pursuance of their instruction have already been laid before the Board. Since 1883, the collections of the Museum have enormously increased, so that before a new building can now be completed, the material pressing for display or even for storage, will demand a considerable part of a building as large as the present one.

Sketch-plans for a building that would meet the wants of the Museum for the immediate future were laid before the Board at their meeting in January, 1890. These plans contemplated a building of two stories and a basement, it being indispensable to have rooms for the preparation and study of material apart from the rooms used purely for the purposes of exhibition.

A bill appropriating $500,000 for a building was reported by Senator Morrill on February 19, 1890, from the Senate Committee on Public Buildings and Grounds, and passed the Senate on the 5th of April, 1890. It was referred in the House to its Committee on Public Buildings and Grounds, from which it has not as yet been reported. The following letter in relation to the subject transmitted to the Hon. Leland Stanford, chairman of the Senate Committee on Public Buildings and Grounds, sets forth at some length the urgent need for further accommodation:

[Senate Mis. Doc. No. 116, Fifty-first Congress, first session.]

LETTER OF THE SECRETARY OF THE SMITHSONIAN INSTITUTION IN RELATION TO A BUILDING FOR THE ACCOMMODATION OF THE NATIONAL MUSEUM.

SMITHSONIAN INSTITUTION,
UNITED STATES NATIONAL MUSEUM,
Washington, January 21, 1890.

SIR: I send you herewith a set of sketch-plans intended to show, in a general way, the extent and character of a building such as would seem to be necessary for the accommodation of the Museum collections in the present and immediate future, and respectfully request for them your attention, and a recommendation to Congress of the necessary means for such a building.

These plans and sketches are provisional, but although not presented in detail, they represent the results of studies, extending over many years, of the plans of the best modern museum buildings in Europe and America, nearly all of which have been inspected by officers of the Smithsonian Institution.

The proposed building covers the same area as that finished in 1881. It is intended to consist of two stories and a basement, except in the central portion, which consists of one lofty hall open from the main floor to the roof, the height of which will be 90 feet, galleries being placed on the level of the second floor in other parts of the building. Its interior arrangements are, as you will see, different from those in the actual Museum, all the changes having been planned in the light of the experience of nine years' occupation of the present building. It will afford between two and three times as much available space for exhibition and storage under the same area of roof. The fifteen exhibition halls are completely isolated from each other, and may readily be subdivided, when necessary, into smaller rooms. The lighting will be as good as in the old building, and the ventilation perhaps still better. The sanitary arrangements have been carefully considered.

The necessity for a basement is especially great. In this, place has been provided for many storage rooms and workshops. The existence of a basement will promote the comfort and health of visitors and employés, and by increasing the dryness of the air in the exhibition halls, will secure the better preservation of the collections. These proposed changes in the internal arrangements will not interfere with conformity with the other points of the present Museum building in the essential features of exterior proportion. The total capacity of this present building in available floor space is about 100,000 square feet; that of the new building somewhat exceeds 200,000. The present Museum building contains about 80,000 feet of floor space available for exhibition. That proposed will contain about 103,300 square feet for exhibition. The space devoted to offices and laboratories would not be much more, but the area available for exhibition halls, storage rooms, and workshops far greater. The appropriation for the construction of the present building was $250,000. This sum was supplemented by several special appropriations: $25,000 for steam-heating apparatus; $26,000 for marble floors; $12,500 for water and gas fixtures and electrical apparatus, and $1,900 for special sewer connections, so that the total cost was $315,400. The structure was probably completed for a smaller sum of money than any other similar one of equal capacity in the world, at an expense ralative to capacity which the present prices of material make it certain can not be repeated.

The estimates of cost on this building vary greatly with regard to

details of construction on which I do not here enter, further than to say that the whole should be absolutely fire-proof throughout, and in view of the further great variation of the cost of building materials within the past two years, I am not prepared to state the sum which would be necessary for its completion. It is certain, however, that $500,000, if not sufficient to complete it, would be all that would be required to be expended during the present year, and I would respectfully represent the desirability of an appropriation of this amount for the purpose in question.

Your attention is directed to certain facts in regard to the character of the materials for the accommodation of which this building is desired. The collections of the Smithsonian Institution and of the Government are especially rich in collections of natural history, which may be grouped in three general classes: The zoological collections; the botanical collections, and the geological collections, including not only all the geological and mineralogical material, but the greater portion of that belonging to paleontology, the study of fossil animals and plants forming an essential part of modern geological work.

Besides the natural history collections, there are equally important anthropological collections which illustrate the history of mankind at all periods and in every land, and which serve to explain the development of all human arts and industries. In everything that relates to the primitive inhabitants of North America, Eskimo as well as Indian, these collections are by far the richest in the world, and with the necessary amount of exhibition space, the material on hand will be arranged in a manner which will produce the most impressive and magnificent effect, the educational importance of which can not be over-estimated. Again, there are collections of considerable extent which illustrate the processes and products of the various arts and industries, as well as what are termed the historical collections, which are of especial interest to a very large number of the visitors of the Museum on account of the associations of the objects exhibited with the personal history of representative men, or with important events in the history of America.

The collections illustrating the arts and the art industries are relatively small, and although in themselves of great interest and value, not to be compared in importance with those in natural history and ethnology.

In a letter addressed on June 7, 1888, to the Hon. Justin S. Morrill, and which will be found in a report of June 12 of the same year from the Senate Committee on Public Buildings and Grounds, I made a statement of the rapidity of the recent growth of the Museum, mentioning that in the five years from 1882 to 1887 the number of specimens in the collection had multiplied no less than sixteen times, and endeavored to give an idea, though, perhaps, an inadequate one, of the extent to which the pressure for want of space was felt. The evil has grown rapidly worse, and as I have had occasion to mention, it has been felt in the last year in a partial arrest of the growth of the collections, which emphasizes the demand for more room. The present Museum building is not large enough even for the natural history collections alone, a number of which are without any exhibition space whatever. The proposed building will afford accommodations for the ethnological and technological material already on hand, and for a large part of the natural history material also.

The collections are still increasing, and the number of specimens, as estimated, is now not far from 3,000,000. The appended table (A) shows the annual increase since 1882. The increase during the last year was

comparatively small. This may be accounted for by the fact that our exhibition halls and storage rooms being filled to their utmost capacity, it has seemed necessary to cease in a large degree the customary efforts for the increase of the Museum.

Unless more space is soon provided, the development of the Government collections will of necessity be almost completely arrested.

So long as there was room for storage, collections not immediately required could be received and packed away for future use. This can not longer be done.

The Armory Building, since 1877 assigned to the Museum for storage and workshops, is now entirely occupied by the U. S. Fish Commission, with the exception of four rooms, and by some of the Museum taxidermists, who are now working in very contracted space, and whom it is impossible to accommodate elsewhere.

Increased space in the exhibition halls is needed, the educational value of the collections being seriously diminished by the present crowded system of installation. Still more necessary, however, is room for storage, for re-arranging the great reserve collections, for eliminating duplicate material for distribution to college and school museums, and for the use of the taxidermists and preparators engaged in preparing objects for exhibition. Space is also required for the proper handling of the costly outfit of the Museum cases and appliances for installation, of which there is always a considerable amount temporarily out of use or in process of construction.

The appended table (B) shows the amount of floor space now assigned to the various collections and the amount required for the proper display of material already in hand, making a reasonable allowance for the expansion during the three years which would probably pass before a new building could be completed and provided with necessary cases.

The appended table (C) shows the number of feet of floor space (the average height being 10 feet) required for laboratories, workshops, and for the several departments. This is in addition to storage space under the cases in the exhibition halls, and a considerable portion may be in cellars and attics.

In summarizing what has just been said, it may be stated in general terms that the amount of space already required for exhibition purposes alone, being (table B) 207,500 feet as against 100,675 now occupied, and this being exclusive of the (table C) 108,900 square feet needed for other objects, the accumulations have now reached such a point of congestion that the actual space needs to be doubled, even independently of future increase; and I beg to repeat that, unless more space is provided, the development of the Government collection, which is already partly arrested, will be almost completely stopped.

Your obedient servant.

S. P. LANGLEY,
Secretary.

Hon. LELAND STANFORD,
Chairman Committee on Public Buildings and Grounds,
United States Senate.

TABLE A.—*Annual increase in the collections.*

Name of department.	1882.	1883.	1884.	1885–'86.	1886–'87.	1887–'88.	1888–'89.
NATURAL HISTORY.							
Zoology:							
Mammals	4, 660	4, 920	5, 694	7, 451	7, 811	8, 058	8, 275
Birds	44, 354	47, 246	50, 350	55, 945	54, 987	56, 484	57, 974
Birds' eggs	....	....	40, 072	44, 163	[1]48, 173	50, 055	50, 173
Reptiles and batrachians	....	....	23, 495	25, 344	27, 542	27, 664	28, 405
Fishes	50, 000	65, 000	68, 000	75, 000	100 000	101, 350	107, 350
Mollusks	[2]33, 375	....	400, 000	[3]400, 000	425, 000	455, 000	468, 000
Marine invertebrates (other than mollusks)	[2]11, 781	[2]14. 825	[4]200, 000	[4]350, 000	[4]450, 000	515, 000	515, 300
Insects	1, 000	....	[5]151, 000	500, 000	[4]585, 000	595, 000	603, 000
Comparative anatomy	3, 605	3, 742	7, 214	10, 210	[4]11, 022	11, 558	11, 753
Living animals	....	....	....	....	....	220	491
Botany:							
Recent plants	....	....	....	30, 000	[4]32, 000	38, 000	38, 459
Paleontology:							
Invertebrate:							
Paleozoic	....	20, 000	73, 000	80, 482	84, 491	84, 649	91, 126
Mesozoic	....	....	100, 000	69, 742	70, 775	70, 925	71, 236
Cenozoic (included with mollusks)	....	....	....	....	....	....	....
Plants	....	4, 624	[6]7, 291	[6]7, 429	8, 462	10, 000	10, 178
Geology:							
Minerals	....	14, 550	26, 610	18, 401	18, 601	21, 896	27, 690
Lithology	[7]9, 075	12, 500	18, 000	20, 647	[8]21, 500	22, 500	27, 000
Metallurgy	....	30, 000	40, 009	48, 000	[8]49, 000	51, 412	52, 076
ANTHROPOLOGY.							
Prehistoric archæology	35, 512	40, 491	45, 252	65, 314	101, 659	108, 631	116, 472
Ethnology	....	....	206, 000	[4]500, 000	503, 764	505, 464	506, 324
American aboriginal pottery	....	....	12, 000	25, 000	[4]26, 022	27, 122	28, 222
Oriental antiquities	....	....	....	....	....	....	850
ARTS AND INDUSTRIES.							
Materia medica	....	4, 000	4, 442	4, 850	5, 516	5, 762	5, 942
Foods	....	[9]1, 244	1, 580	[8]822	[10]877	[11]877	911
Textiles	....	....	2, 000	3, 064	3, 144	[11]3, 144	3, 222
Fisheries	....	....	5, 000	[8]9, 870	10, 078	[11]10, 078	[11]10, 078
Animal products	....	....	1, 000	2, 792	2, 822	[11]2, 822	2, 948
Naval architecture	....	....	600	....	....	....	600
Historical relics	....	....	....	1, 002	} 13, 634	14, 640	[11]14, 640
Coins, medals, paper money, etc.	....	....	....	1, 055			
Musical instruments	....	....	....	400	417	427	[6]427
Modern pottery, porcelain, and bronzes	....	....	....	2, 278	2, 238	3, 011	[11]3, 011
Paints and dyes	....	....	....	77	100	[11]100	109
"The Catlin Gallery"	....	....	....	500	500	500	[11]500
Physical apparatus	....	....	....	250	251	[11]251	[11]251
Oils and gums	....	....	....	[8]197	198	[11]198	213
Chemical products	....	....	....	[8]659	661	[11]661	688
Total	193, 362	263, 143	1, 472, 600	2, 420, 944	2, 666, 335	2, 803, 459	2, 863, 894

[1] 2,235 are nests.
[2] Catalogue entries.
[3] Including cenozoic fossils.
[4] Estimated.
[5] Professor Riley's collection numbers 150,000 specimens.
[6] Exclusive of Prof. Ward's collection.
[7] In reserve series.
[8] Duplicates not included.
[9] Including paints, pigments, and oils.
[10] Foods only.
[11] No entries of material received during the year have been made on catalogue.

N. B.—No estimate of increase of collections taken in 1885.

TABLE B.—*Exhibition space.*

Department.	Floor space now occupied.	Amount required.
NATURAL HISTORY COLLECTIONS.		
Zoology:	*Sq. feet.*	*Sq. feet.*
Mammals	6, 500	12, 000
Birds	6, 000	14, 000
Reptiles and batrachians	1, 000	3, 000
Fishes and fisheries	7, 600	14, 000
Mollusks	3, 500	5, 000
Marine invertebrates (other than mollusks)	3, 000	5, 500
Insects	1, 600	4, 000
Comparative anatomy	4, 500	10, 000
Botany:		
Systematic and economic (including forestry)	1, 000	4, 000
Paleontology;		
Invertebrate (including Paleozoic, Mesozoic and Cenozoic)	2, 500	7, 500
NATURAL HISTORY COLLECTIONS—continued.		
Paleontology—continued.	*Sq. feet.*	*Sq. feet.*
Vertebrate	1, 500	10, 000
Mineralogy and geology	12, 000	17, 009
ANTHROPOLOGICAL COLLECTIONS.		
Prehistoric archæology	10, 000	10, 000
General ethnology	10, 400	40, 000
Arts and industries	22, 000	40, 000
History	3, 000	5, 000
Lecture hall	4, 575	6, 500
Totals	100, 675	207, 500

TABLE C.—*Storage, workshops, offices, laboratories, etc.*

Department.	Square feet.
NATURAL HISTORY.	
Zoology:	
Mammals	3, 000
Birds	4, 000
Reptiles and batrachians	2, 500
Fishes	5, 000
Mollusks	4, 000
Marine invertebrates (other than mollusks)	4, 000
Insects	2, 400
Comparative anatomy	3, 000
Botany:	
Herbarium	4, 000
Paleontology:	
Invertebrate:	
Paleozoic	4, 000
Mesozoic	4, 000
Cenozoic	4, 000
Plants (fossil)	2, 000
Vertebrate	6, 000
NATURAL HISTORY—continued.	
Geology:	
Mineralogy and geology (including workshops)	4, 000
Anthropology:	
Prehistoric archæology	2, 000
General ethnology	6, 000
Arts and industries (several divisions)	15, 000
Taxidermists, osteologists, modelers, preparators	10, 000
Mechanics	5, 000
General storage rooms, for cases not in use, duplicates, unelaborated material, etc.	15, 000
Total	108, 900

In compliance with the requirements of the sundry civil bill approved March 2, 1889, an examination was made of the National Museum by the Architect of the Capitol for the purpose of estimating the cost of constructing a basement story under that building. The only portion

of such a basement suitable for workshops and storage would be a cellar running around the outer walls of the building and extending inwards 30 feet, so that the rooms thus obtained might have light and air. Provision was also made to floor with tiles all the rooms under which these basements come. The total expense it is thought would be $57,675, but by reason of the peculiar construction of the present building the Architect has expressed the opinion that the work estimated for would be one of unusual difficulty, and that a site for a storehouse and workshops required might be purchased in the neighborhood of the Museum and a fire-proof building erected thereon for a less sum.

The improvement of the Smithsonian building proper has been the subject of careful consideration, more especially the fire-proofing of the west wing, the urgent need of which has already been brought to the attention of the Regents. A bill was introduced in the Senate on January 15, 1890, by Senator Morrill, providing for an appropriation of $45,000 for fire-proofing the roof of the main hall and that of the so-called chapel in the west wing of the Smithsonian building, putting in a sky-light and well hole for lighting the east wing, and making certain changes which would add greatly to the space available for office rooms in that part of the building, as well as adding to the facility with which the large amount of exchange publications could be handled. This work was to be done under the direction of the Architect of the Capitol with the approval of the Regents. The bill passed the Senate on February 10, 1890, and was favorably reported on in the House March 3, 1890. The matter rested here at the close of the year.

The temporary wooden building for the protection of instruments for astro-physical investigation, which was referred to as contemplated in my last report, was begun on November 30, 1889, and was completed about the 1st of March, 1890. This building is of the most inexpensive character, and is simply intended to protect the instruments temporarily, though it is also arranged so that certain preliminary work can be done here. Its position however immediately south of the main Smithsonian building, is not well suited to refined physical investigations on account of its proximity to city streets and its lack of seclusion. The needs of this department are referred to more at length under the following head of research.

RESEARCH.

I take pleasure in reporting that the Institution has been able to do rather more for the encouragement of original research than it has done for several years past.

Referring to my two previous reports in regard to the project of Professor Baird for securing an astro-physical observatory and laboratory, I am able to say that this object has assumed definite shape in the construction of the temporary shed, which has just been mentioned. In this shed there have been built, as the most expensive part of the

structure, a number of brick piers required for the firm support of the delicate apparatus employed.

In connection with the construction of this building, I desire to express my thanks to Col. O. H. Ernst, U. S. Army, in charge of public buildings and grounds, for the supervision rendered by his office of the work of excavating, etc., for the necessary sewer and water connections.

The principal instrument consists of a siderostat constructed by Sir Howard Grubb, of Dublin, Ireland, for the Smithsonian Institution, to meet my special requirements. This arrived in March, 1890, and has been mounted and put approximately into position for use. Another important and novel instrument, a spectro-bolometer, was made under my directions to meet new and unusual demands, and has also been received and put in place. A third piece of apparatus, a special galvanometer, also designed for the particular class of work in view, has been received; and the only considerable instrument now required to complete the outfit is a resistance box, which has been ordered and is expected from London before the end of the calendar year.

The siderostat is probably the largest and most powerful instrument of its kind ever constructed. The spectro-bolometer is the largest instrument of its kind, and with this improved apparatus it is hoped that interesting investigations begun several years ago, will be continued.

Supplementary to these principal instruments is the Thaw collection of physical apparatus loaned by the executors of the late William Thaw, of Pittsburgh, and there are a few pieces of apparatus, the personal property of the Secretary, so that at the close of the year it might be said that the Institution was in possession of the nucleus of a modern astrophysical laboratory. With this apparatus temporarily mounted, researches have already begun, and one of a scientific and economic character upon "The Cheapest Form of Light" has been the subject of a communication to the National Academy of Sciences. This work is mentioned as indicating my intention to give greater place to one of the chief objects of the Institution, the direct addition to knowledge by original research,—which, at least as regards the physical sciences, has received comparatively little attention since the time of Professor Henry.

The prospects of renewed contributions to physical science by the Institution in the field of original research are happily now better than for many years past. The late Dr. Jerome H. Kidder, formerly an officer of the U. S. Navy, and later attached to the U. S. Fish Commission and to the Smithsonian Institution, had bequeathed to the Institution, in a will made several years ago, the sum of $10,000 to be employed for biological researches. Dr. Kidder, having become especially interested in the proposed astro-physical observatory, had the intention of transferring this bequest, or at least a portion of it, to such an end, and he even ordered that a codicil giving $5,000 to the Institution for an astro-physical observatory should be added to his will, but he was stricken

with so sudden an illness that he was unable to sign it. In view of these circumstances and after careful deliberation upon the matter, the Regents decided to accept as finally and decisively indicative of the wishes of the testator the provisions of this codicil bequeathing $5,000 for the purpose of an astro-physical observatory, and this sum was therefore paid by Dr. Kidder's executor to the Institution.

A further sum of $5,000 was likewise generously presented by Dr. Alexander Graham Bell to the writer individually for the prosecution of the researches in astro-physics, to which he has devoted much of his life, but it has seemed proper to him, under the circumstances, that this sum should be placed to the credit of the Smithsonian Institution upon the same footing as the Kidder bequest, and with the consent of the donor it has been so transferred. I am therefore desirous of here expressing my own personal as well as my official obligation to Dr. Bell for this gift for the increase of knowledge.

The initial step for the establishment of an astro-physical observatory under the National Government thus having been taken by private individuals, it is hoped that Congress will see fit to place it upon a firm footing and to make a small annual provision for its maintenance. And it seems proper to mention that the field of research to which such a department of the Institution would be devoted has been considered of sufficient importance by the legislators of leading foreign nations to justify the erection of costly special observatories and to provide for their maintenance with a staff of astronomers and physicists of wide reputation.

The class of work here specially referred to does not ordinarily involve the use of the telescope, and is quite distinct from that carried on at any observatory in this country. It would in no way conflict with the work of the present U. S. Naval Observatory, being in a field of work that the latter has never entered.

Briefly stated, the work for which the older Government observatories at Greenwich, Paris, Berlin, and Washington were founded, and in which they are for the most part now engaged, is the determination of relative positions of heavenly bodies and of our own place with reference to them. Within the past twenty years, all these Governments but our own have established astro-physical observatories, as they are called, that are engaged in the study of the constitution of the heavenly bodies as distinguished from their positions; in determining, for example, not so much the position of the sun in the sky as the relation that it bears to the earth and to our own daily wants; how it effects terrestrial climate; and how it may best be studied for the purposes of the meteorologist, and so on; and it is an observatory of the latter kind that the donors just mentioned appear to have had prominently in view, and which it is proposed to conduct (though on an extremely modest scale) under the auspices of the Institution.

In connection with this renewed revival in the line of physical re-

search, I may state that steps have been taken to give effect to certain resolutions expressed at a meeting of the American Association for the Advancement of Science several years ago, in regard to the establishment of standard screw threads and standard diameters of tubing for astronomical and physical apparatus. The introduction of such standards in mechanical work of all kinds has proved itself of such great value that its usefulness need not be dwelt upon. As a preliminary step looking to the establishment of this desired uniformity on the part of scientific men, a conference has been had with the Superintendent of the Coast Survey, and it is proposed to invite the co operation of other Government bureaus, and to give effect to their conclusions by ordering and establishing, on behalf of the Institution, recognized standards for the use of scientific instrument makers in all parts of the world.

I have here referred to researches in physical science alone, the work of the Institution and of individual members of its staff and others in natural history being given at some length under the head of the Museum.

EXPLORATIONS.

The work of exploration by the Institution has been carried on through the Bureau of Ethnology and the National Museum, and to the Reports of these departments reference should be made for details.

In my report for last year, mention was made of a trip to Africa by Mr. Talcott Williams, and of the interesting results that had been secured by him. A valuable collection of specimens that he obtained is still unpacked and a complete description of them can not be given until they have been thoroughly examined.

He was fortunate enough to secure five sheets of an extremely rare Berber manuscript, made probably in the thirteenth century; a botanical collection of about three hundred plants, of which all except four or five are phenagamous fossils from a hitherto unexplored region; a valuable collection of ethnographic material from Morocco; villager costumes of men and women, representing both the Berber and mountain villages, and a collection of pottery made with the special design of including all the wares in ordinary use between Tetuan and Fez. Articles illustrating light, fire, and the industry of comb-making and numerous household utensils were also secured.

It may safely be asserted that this collection, taken as a whole, is one of the most interesting of the kind that the Museum has ever received, and the thanks of the Smithsonian Institution are due Mr. Williams for the manner in which he has accomplished his mission.

Mr. W. W. Rockhill, whose explorations in Thibet were also referred to in my last report, has spent a large part of the year in Washington, engaged in preparing an account of his remarkable travels, and he has loaned to the Museum, in addition to his large and almost unique collection of Thibetan material, a most valuable lot of cloisonnés, bronzes, and carved lacquers collected during his residence in Pekin.

I may also mention here collections of unusual interest and value, made by Dr. W. A. Abbott, in the region of Mount Kilémanjaro, and of those by Mr. William Harvey Brown, of the National Museum, while attached to the United States Eclipse Expedition to the west coast of Africa, under the auspices of the Navy Department. Grateful acknowledgments are due Dr. W. H. Rush, U. S. Navy; Mr. J. P. Iddings, U. S. Geological Survey; Mr. E. M. Aaron, of the American Entomological Society; Mr. C. R. Orcutt, of San Diego, Cal., from whom specimens secured in their travels have been received or are expected. Mr. Henry W. Elliott, who is now visiting the Seal Islands of Alaska on United States Government business, is expected to secure for the Museum specimens of fur-seal, fishes, and other zoological material.

In the Bureau of Ethnology I would refer to the mound explorations that have been conducted under the immediate superintendence of Prof. Cyrus Thomas, by Mr. H. L. Reynolds, Mr. J. D. Middleton, and Mr. James Mooney; and to the general field work, chiefly among the Indian tribes, of Mr. W. H. Holmes, Dr. W. J. Hoffman, Mr. Victor Mindeleff, Mr. James Mooney, Mr. Jeremiah Curtin, Mr. J. W. B. Hewitt, and Mrs. T. E. Stevenson.

PUBLICATIONS.

With regard to the character of the works issued by the Institution during the past year, little is to be added to the general statements made in my last report. In each of the three classes of Smithsonian publications, to wit, I, The Contributions to Knowledge; II, The Miscellaneous Collections; and III, The Annual Reports, about the same amount of productiveness has been maintained.

Smithsonian Contributions to Knowledge.—An original memoir by Prof. Alpheus Hyatt on the "Genesis of the Arietidæ," illustrated with numerous plates, has been published during the year, and this has permitted the completion of the long-delayed twenty-sixth volume of the quarto series. Two other memoirs, relating to the solar corona, have been published in the same quarto form, but will not probably be included in the volumes of the "Contributions."

Smithsonian Miscellaneous Collections.—While the number of separate titles under this class has been considerable, many of them are the separate issues of articles contributed at the expense of the Institution to the Annual Reports. It is in contemplation to devote a larger space in the "Collections" than of late to publications connected with the physical sciences; in which direction may be mentioned as one of the more important issues of the year, an "Index to the Literature of Thermodynamics," by Mr. Alfred Tuckerman. The demand for copies of the exhausted fourth edition of Guyot's Meteorological and Physical Tables, published in 1884, has been deemed sufficient to warrant the revision of the work and the issue of a new edition, which has been for several years under consideration. After obtaining the views of prominent me-

georologists the work was placed in the hands of Prof. William Libbey, Jr., of Princeton, New Jersey, with the expectation that the new edition will be ready for the printer during the coming year.

Among the publications of this series mention may be made of the tenth "Toner Lecture," by Dr. Harrison Allen, on "A Clinical Study of the Skull."

A revised catalogue and index of all the Smithsonian publications to the middle of 1886, occupying 383 pages, prepared by Mr. William J. Rhees, the chief clerk, has also been published.

No completed volume of the Miscellaneous Collections has been issued within the year.

Smithsonian Annual Reports.—The annual report of the Regents to Congress for the year ending June 30, 1887, in two parts or volumes, has been received from the Public Printer and has been widely distributed. The annual report for the succeeding year, 1888, although printed, has not yet been received; but is daily expected.

A detailed account of the several publications of the Smithsonian Institution for the year, under each class, will be given in the Appendix.

Other publications.—The publications of the National Museum comprise the "Proceedings of the National Museum" and the "Bulletins of the National Museum," and are maintained by an appropriation annually made by Congress. As stated in my last report, "It has been decided to hereafter omit these publications from the series" of Miscellaneous Collections issued by the Institution.* Of the publications of the Bureau of Ethnology the sixth annual report has been issued during the year.

The edition of Swan's paper on "The Indians of Cape Flattery" having become exhausted, a new edition of 250 copies has been printed.

The Annual Report of the American Historical Association, which by the act of incorporation the Secretary of the Institution is directed to communicate to Congress, has been printed as Senate Miscellaneous Document No. 170.

In October, 1889, final arrangements were made with Prof. Edward D. Cope, whereby it is expected that his important work upon "Reptilia," undertaken several years ago at the request of the Secretary, will be ready for the printer by the end of December, 1890.

Except in the case of the Annual Reports, the publications of the Institution are generally issued with satisfactory promptness. The Annual Reports, which have been for some years so seriously behindhand as to materially affect the value of the reviews upon scientific progress, are, it is hoped, to be brought up to date during the coming year.

To avoid any possible delay on account of lack of legislation, the attention of the chairman of the Committee on Printing of the United States Senate has been called to the desirability of having the bill pro-

*A full account of these productions will be given in the second part of the Annual Report of the Smithsonian Institution for the year 1889-'90.

viding for the printing of the Annual Reports so worded as to allow for the printing of future reports without special legislation each year, at the some time increasing the number of copies to 19,000. An act of Congress in the following terms would probably accomplish all that is desired:

That there be printed of the Reports of the Smithsonian Institution and of the National Museum, for the years ending June thirty, eighteen hundred and eighty-eight, and June thirty, eighteen hundred and eighty-nine, and annually thereafter, in two octavo volumes for each year, nineteen thousand extra copies, of which three thousand shall be for the use of the Senate, six thousand for the House of Representatives, and ten thousand for the Smithsonian Institution.

THE SMITHSONIAN INTERNATIONAL EXCHANGE SERVICE.

At a meeting of the Board of Regents of the Smithsonian Institution on January 8, 1890, it was—

Resolved, That the Regents instruct the Secretary to ask of Congress legislation for the repayment to the Institution of the amount advanced from the Smithsonian fund for Governmental service in carrying on the exchanges.

In connection with this resolution the following outline of the history of the exchanges is important:

Under the act of Congress accepting a donation from James Smithson "for the increase and diffusion of knowedge among men," and giving effect to this trust by the foundation of the Smithsonian Institution, the Board of Regents in 1851 established a system of international exchanges of the transactions of learned societies and like works; but, in addition to such publications, it voluntarily transported between 1851 and 1867 somewhat over 20,000 packages of publications of the bureaus of the National Government at an estimated cost to the private funds of the Institution of about $8,000. This, however, was understood to be a voluntary service, and no request for its re imbursement has been made or is contemplated.

Congress, however, in 1867, by its act of March 2, imposed upon the Institution the duty of exchanging fifty copies of all documents printed by order of either House of Congress, or by the United States Government bureaus, for similar works published in foreign countries, and especially by foreign Governments.

The Institution possessed special facilities and experience for such work, the propriety of its undertaking which, in the interests of the Government, is evident; but it was hardly to have been anticipated that the Government should direct this purely administrative service and make no appropriation for its support. Such, however, was the case, and with the exception of a small (presently to be noted) sum, returned by some bureaus, it was almost entirely maintained during the next thirteen years, or until the first appropriation to the Institution for exchanges in 1881, at the expense of the private fund of James Smithson.

From January 1, 1868, to June 30, 1886, 292,483 packages containing these official Government publications, having little to do with the object to which Congress devoted the Institution's private funds were transported by the Exchange Bureau at a pro rata cost of $92,943.36 of which $29,706.85 accrued between 1881, when the first specific appropriation was made, and 1886. Of this $92,943.36 $19,302.35 was returned from various Departments and bureaus, leaving a balance of $73,641.01 expended in carrying exclusively Governmental publications.

What has preceded refers to the transportation of official documents, and not to that of transactions of learnéd societies and other like works; but it is now necessary to mention that in 1878 the honorable the Secretary of State designated the Smithsonian Institution as the special agent for the United States Government for carrying out the provisions of an international convention at Paris, which made the respective Governments assume the cost, not only of the transportation of official documents, but of scientific and literary publications, between the states interested, and it would seem that Congress itself adopted this view of its responsibility, for from July 1, 1881, to June 30, 1886, while the Congressional and bureaucratic exchange represented a pro rata cost of $29,706.85 and the scientific publications $39,034.90, Congress appropriated directly $35,500, somewhat more than the cost of the Government exchange, but leaving a balance of $3,534.90 for scientific and literary exchanges unpaid. This latter sum, $3,534.90, added to the $73,641.01 mentioned above, makes a total of $77,175.91, for which, in equity, repayment might be requested.

In 1886, on the 15th of March, plenipotentiaries of the United States and various other nationalities signed a convention more formal than that at Paris, by which the respective Governments definitely assumed the exchange of official documents and scientific and literary publications between the states interested.

Adopting, then, the year 1886, rather than the earlier date, 1881 (though, as mentioned in the report, equity would seem to allow the Institution the entire sum expended in exchanges, at least since its official recognition by Congress in 1881 as the Government exchange agent), it appears upon deducting the amount appropriated by Congress, $35,500, from the balance shown in the preceding paragraph, $73,641.01, that we have $38,141.01 as the amount due the private fund of James Smithson from 1868 to 1886.

Considering separately the period from July 1, 1886, to June 30, 1889, we find that the amount expended in these years under the direction of the Smithsonian Institution on account of international exchanges was $47,126.56; of this sum $37,000 was paid by Congressional appropriations, $3,091.75 were paid by Government departments and others, and the balance, $7,034.81, by the Smithsonian Institution.

To recapitulate briefly it appears, then, that the following sums have

been expended from the Smithsonian funds for the support of the international exchange system in the interests and by the authority of the National Government, namely, $38,141.01 in excess of appropriations advanced from January 1, 1868, to June 30, 1886, for the exchange of official Government documents, and $7,034.81 in excess of appropriations from July 1, 1886, to June 30, 1889, advanced for the purpose of carrying out a convention entered into by the United States, or an aggregate of $45,175.82.

A memorandum setting forth the above facts and requesting that steps be taken to procure the return to the Smithsonian fund by Congress of the sum last mentioned ($45,175.82) was transmitted on the 20th of May, 1890, to the Hon. Benjamin Butterworth, of the Board of Regents, to be laid by the latter before Congress in due form.

The exchange work has shown the usual increase, no less than 82,572 packages having been handled during the year, or 6,606 more than during the year immediately preceding. The number of societies and individuals for which exchange accounts are kept is now 16,002.

The actual cost of the exchanges for the fiscal year, taking in account bills rendered and moneys received up to September 21, 1890, for services rendered between July 1, 1889, and June 30, 1890, was $17,401.23. Of this sum $15,000 were appropriated directly by Congress, $1,986.14 were repaid by several Government bureaus to which appropriations had been made for the purpose, $28.40 was received from State institutions and other sources, leaving a deficiency of $386.69, which was paid from the Smithsonian fund.

In my report for last year I had the honor to submit detailed estimates showing the necessity of larger appropriations by Congress if the Exchange Bureau is to be placed upon a satisfactory footing.

The chief increase in outlay would be to secure a more prompt service and to increase the number of exchanges that are received for the Library of Congress, in return for the Government exchanges sent abroad. It is probable that the number of the latter would be largely increased if special efforts were made to that end.

An improvement in the promptness of transmission to Europe has taken place within the last few years, but packages are still unduly delayed by reason of the fact that we are not able to pay for rapid transmission. The exchange boxes go by slow freight and we are in most instances dependent upon the courtesy of the steam-ship companies for free freight. The greater number of the publications now transmitted are for the benefit of the Government and it seems unjust to continue to make use of such privileges originally granted in the interests of of science. The entire sum asked for was $27,500.

Our exchange relations with foreign Governments have undergone no material change on account of the treaty at Brussels proclaimed January 15, 1889, to which allusion has been made in previous reports.

In order to carry out in good faith, as far as our own country is con-

cerned, the convention relating to the immediate exchange of parliamentary journals, a communication was directed to the honorable the Secretary of State under date of December 12, 1889, stating the necessity of procuring from Congress an appropriation of about $2,000 to meet the expenses of transmitting abroad copies of the Congressional Record and other published documents pertaining to the daily routine of Congress; and a joint resolution introduced at the instance of the honorable the Secretary of State was promptly passed by the Senate, appropriating the sum named, $2,000. I regret, however, that at the close of the fiscal year no action had been taken in the matter by the House of Representatives, and in consequence no attempt has been made to give effect to the treaty.

Tables showing in detail the transactions of the year will be found in the report of the curator of exchanges appended hereto.

The progress of work on the new exchange list is mentioned under the head of the library.

LIBRARY.

The accessions to the library have been recorded and cared for as during the last fiscal year.

The following statement shows the number of books, maps, and charts received from July 1, 1889, to June 30, 1890:

	Octavo or smaller.	Quarto or larger.	Total.
Volumes	1,236	527	1,763
Parts of volumes	5,202	8,256	13,458
Pamphlets	3,776	554	4,330
Maps			636
Total			20,187

Of these accessions, 8,695 (namely, 785 volumes, 6,900 parts of volumes, and 1,010 pamphlets) were retained for use at the National Museum library, and 1,059 medical dissertations were deposited in the library of the Surgeon-General, U. S. Army; the remainder were promptly sent to the Library of Congress on the Monday following their receipt.

The reading room is now almost filled with periodicals. There are at present displayed the current volumes of 468 journals. The construction of shelves above the cases in the reading room has rendered it practicable to withdraw from the Smithsonian deposit in the Library of Congress the complete series of the large quarto Transactions or Memoirs of most of the great European academies; the Librarian of Congress kindly giving every facility for this transfer.*

* The publications now deposited in the reading-room are as follows: The "Handlingar" of the Royal Swedish Academy; Transactions of the Royal Society of Edinburgh; Transactions of the Royal Irish Academy; "Skrifter" of the Royal Danish Society of Sciences; "Denkschriften" of the Imperial Academy of Sciences, Vienna; Memoirs of the St. Petersburg Academy; "Atti" of the two Academies of the Lincei at Rome, the royal and the pontifical; Nova Acta Academiæ Cæsareæ Leopoldino-

In my last report, I referred to the commencement of the work of increasing the library by exchanges. This work has now been carried on for a year with fairly promising results.

The labor of assigning the different journals recommended as desirable to the four classes mentioned in my last report—namely, (1) journals which receive no Smithsonian publications, and which are not to be found in the library of the Institution; (2) journals which receive Smithsonian publications, but which make either no return or an inadequate return for these; (3) journals which regularly exchange with the Institution, but of which the files in the library are for any reason defective; (4) journals which regularly exchange with the Institution, and of which the library possesses a complete file—occupied the time until January 18, 1890. The writing of letters asking for exchange or calling attention to deficiencies was then commenced systematically.

Up to the close of the fiscal year, 1,601 such letters had been written. In response to these letters, 201 new exchanges were received and 360 defective series were completed, either wholly or as far as the missing parts were still in print.

A list of the new exchanges is presented in the Appendix (Report of the Librarian) where will also be found a list of the most important accessions outside of the regular serials.

The work of re-organization of the library under the regulations which I had prepared upon my appointment as Assistant Secretary, and described at some length in my report for the years 1887–'88, has been efficiently carried out by the librarian, Mr. Murdoch. I may also mention that a plan is under consideration for the further extension of the usefulness of the library, by establishing as a part of it a collection of books on general literature for the use of the employés of the Institution and its dependencies, although in its present location its growth is impeded for lack of room, owing to the pressing demands of the Government business in the Exchange Bureau.

MISCELLANEOUS.

Statue of Professor Baird.—I desire to call the attention of the Regents to the fact that the bill introduced in the Senate and passed by that body on February 10, 1888, making an appropriation for the erection of a bronze statue in recognition of the distinguished services to the country of the late Professor Baird, has failed to reach final action by Congress. I earnestly hope that steps will be taken to secure for this measure the attention it merits, and I continue to give it my personal care.

Grants in aid of the physical sciences.—In accordance with an early established precedent, though one of late in disuse, some small grants,

Carolinæ Germanicæ Naturæ Curiosorum; "Abhandlungen" of the Berlin Academy; "Nova Acta" of the Academy of Upsala. In addition to these the Philosophical Transactions of the Royal Society, and the "Comptes-Rendus" of the French Academy of Sciences have been deposited in the office of the editor.

from the Smithonian fund, commensurate rather with the abilities of the Institution than with its wishes, have been made this year to aid in physical science in addition to the aid so largely given to bioloigcal and ethnological science through the Museum, Bureau of Ethnology, and Zoological Park.

The subscription of twenty copies of the Astronomical Journal, which are distributed abroad as exchanges of the Institution, has been continued.

To the Lick Observatory, through its director, Professor Holden, a small grant has been made for the purchase of photographic plates and apparatus to be used in securing photographs of the moon, and especially of certain regions on a large scale, the results of the work being available for publication by the Institution.

Aid has also been promised Prof. Albert A. Michelson, of Clark University, Worcester, Mass., in his important investigations for the determination of a standard of length that shall depend upon the length of a wave of light.

A small grant has been made to Mr. F. A. Seely, of the United States Patent Office, for the purchase of certain objects of archæological interest, during the course of a contemplated journey in Spain.

Assignment of rooms for scientific work.—A room in the basement, which is specially suited for delicate physical measurements, on account of its freedom from tremor, has been continued at the disposal of the U. S. Coast and Geodetic Survey for pendulum experiments, and two office rooms have also been assigned to the temporary use of the Zoological Park Commission. The Regents' room, in the south tower, was granted for a meeting of the American members of the committee on the "International Standards for Iron and Steel" on February 19, 1890.

Facilities for study in the Museum have been accorded to a number of students, as stated in describing the Museum work, and under special conditions instruction has been given in taxidermy and photography. The lecture hall in the Museum has been used by authority of the Executive Committee for the meetings of the National Academy and other scientific organizations and for the Saturday lecture courses.

Toner lecture fund.—This fund, which has an estimated value of about $3,000, is in the care of a board of trustees, of which the secretary of the Smithsonian Institution is *ex officio* chairman. No lecture has been delivered this year under the auspices of this fund. The lecture delivered by Dr. Harrison Allen, on May 29, 1889, on the "Clinical Study of the Skull," has been printed.

American Historical Association.—A bill to incorporate the American Historical Association, which provided that the Association should report annually to the Secretary of the Smithsonian Institution and that the Secretary should communicate to Congress the whole of such reports, or such portion thereof as he might see fit, finally became a law on January 4, 1889.

In December, 1889, the annual meeting of the Association took place in Washington, the morning session being held in the lecture hall of the National Museum and the evening session in the Columbian University. The proceedings of this meeting are printed in the annual report of the association, which, in accordance with the provisions cited above, was submitted to me on January 14, 1890, and on June 18 was communicated to Congress and ordered to be printed as Senate Miscellaneous Document No. 170. This report included, in addition to the proceedings of the annual meeting, a number of historical papers of a high order.

The provision by which the Regents are authorized to permit the deposit of the collections, manuscripts, books, pamphlets, and other historical material of the Association, has been met as well as our present accommodations will admit, and in making an estimate for repairs to the Smithsonian buildings arrangements were made for a suitable and safe place in which such valuable records might be stored.

Bureau of fine arts.—The desirability of having in connection with the Government a suitable depository of works of art has presented itself so forcibly to Members of Congress, and without suggestion on the part of the Regents, that a bill was introduced in the Senate by the Hon. Wilkinson Call, on December 4, 1889, providing for the establishment of a bureau of fine arts in the Smithsonian Institution. This was referred to the Committee on the Library, but has not been reported.

The wording of the bill is as follows:

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That there be, and is hereby, created in the Smithsonian Institution a bureau called the Bureau of the Fine Arts, the management of which is entrusted to the Secretary of the Smithsonian Institution.

SEC. 2. That the purpose and duties of this bureau shall be to aid in the development of the fine arts in the several States and Territories of the United States, by the re-production, for the use of art schools and academies, of casts of statuary and other objects used in giving instruction in art; by preparing and distributing plans for the construction of buildings and the adaptation of rooms suitable for use as art schools, with printed plans for the organization of various grades of art academies and classes; by causing to be held annually in Washington, District of Columbia, a public exhibition of works of art, open to all desiring to exhibit, in which the fairest possible opportunity for exposition shall be afforded all contributors; and by the publication of an annual register containing an account of new discoveries, inventions and methods of instruction useful to students of art, together with a report of the progress of the fine arts in the United States.

SEC. 3. That the re-productions and publications of the bureau shall be distributed among institutions of art, under such regulations as the Secretary of the Smithsonian Institution may establish.

SEC. 4. That the Secretary of the Smithsonian Institution shall provide suitable quarters for the holding of the annual art exhibition.

SEC. 5. That for the purpose of carrying on the operations of this bureau there be and is hereby appropriated, for the fiscal year begin-

ing July 1st, eighteen hundred and eighty- , the sum of dollars, to be paid by the Secretary of the Treasury out of any moneys in the Treasury not otherwise appropriated, and expended under the direction of the Secretary of the Smithsonian Institution.

Capron collection of Japanese works of art.—A bill appropriating $14,675, introduced by the Hon. Daniel W. Voorhees on December 4, 1889, was referred to the Committee on the Library, was reported favorably, and passed the Senate on March 29, 1890. It was also, on May 19, 1890, reported favorably by the House Committee, but was not reached on the calendar at the close of the year.

The World's Columbian Exposition, Chicago, 1892.—The act of Congress approved April 25, 1890, which provides for celebrating the four hundredth anniversary of the discovery of America by Christopher Columbus, by holding an international exhibition of arts, industries, manufactures, and the product of the soil, mine, and sea in the city of Chicago, states in section 16:

That there shall be exhibited at said exposition by the Government of the United States, from its Executive Departments, the Smithsonian Institution, the United States Fish Commission, and the National Museum, such articles and materials as illustrate the function and administrative faculty of the Government in time of peace and its resources as a war power, tending to demonstrate the nature of our institutions and their adaptation to the wants of the people; and to secure a complete and harmonious arrangement of such a Government exhibit, a board shall be created to be charged with the selection, preparation, arrangement, safe-keeping, and exhibition of such articles and materials as the heads of the several Departments and the directors of the Smithsonian Institution and National Museum may respectively decide shall be embraced in said Government exhibit. The President may also designate additional articles for exhibition. Such board shall be composed of one person to be named by the head of each Executive Department, and one by the directors of the Smithsonian Institution and National Museum, and one by the Fish Commission, such selection to be approved by the President of the United States. The President shall name the chairman of said board, and the board itself shall select such other officers as it may deem necessary.

Under the authority conveyed by this act I have designated as the representative upon this board of the Smithsonian Institution and National Museum, the assistant secretary of the Institution, Dr. G. Brown Goode, who has already devoted considerable time to the subject of the proposed exposition in addition to his other official duties.

In connection with this requirement that an exhibit shall be made by the National Museum, I beg leave to recur to the fact that it has been the experience in connection with previous expositions on a smaller scale, that the routine work of the Institution is seriously interfered with by thus throwing upon its regular employés the great burden involved in the preparation, packing, and displaying of Museum material without adequate assistance by an increased appropriation during this time of unusual effort. The impairment of specimens by frequent transportation should also be borne in mind, and in justice to our per-

manent exhibits provision should be made for repairing any damage incurred.

Stereotype plates.—All the stereotype plates belonging to the Institution are now stored in the basement of the building, and some progress has been made in examining, re-arranging, and where the boxes have become worn out, in re-packing plates. Owing to the limited amount of time that can be devoted to this work, however, it will be some months before they can be put in a thoroughly satisfactory condition for ready reference.

A request from Messrs. Lee & Shepard, of Boston, for the use of plates from Professor Hyatt's "Genesis of the Arietidæ" has been cheerfully complied with.

Correspondence.—I have given much attention to the improvement of the methods of handling the correspondence of the Institution, which is constantly growing and has already assumed very considerable proportions. A simple but effective means of recording letters, showing at a glance, what letters remain unanswered each week, has been introduced, and as a result few letters remain long without reply.

It should be borne in mind, however, that the character of the correspondence, except such as relates to business routine, is quite different from that of Government bureaus. Constant inquiries are made from all parts of the country for information on almost every conceivable topic, and requests for statistics and for information on the most varying scientific subjects. It is intended that all of these inquiries should receive acknowledgment, and, wherever possible, that the information desired should be sent, though in many cases it requires an amount of time and labor on the part of curators and other officers of the Institution wholly out of proportion to the merits of the case.

As properly coming under the head of "diffusion of knowledge," it does not seem proper to neglect such inquiries, and it is intended to give encouragement and advice wherever possible to all interested in the objects of the Institution.

The course taken by an incoming letter is now as follows: The mail is opened each morning in the chief clerk's office, and all letters addressed to the secretary or the Institution, with the exception of those on printed forms, purely routine matters, and applications for Museum publications, are placed on the secretary's desk at 10 o'clock, together with letters for signature. Having been acted upon, the date stamp of the secretary's office is affixed to each communication and the letter is then returned to the chief clerk's office. Should the secretary have written in his own hand the name of any employé or officer of the Institution upon a letter, such action means that the letter is to be referred to the person named, who is expected to prepare a reply thereto for the secretary's signature.

The one exception to this rule is when the secretary refers a letter to the assistant secretary, who exercises his discretion as to whether the

letter should be answered at all or not, and if so, whether he or the secretary should sign the reply.

In case no comment has been made by the secretary, the disposition of the letters is left to the chief clerk, who assigns them to the officers or clerks having in charge the matter treated of. The letters are then sent to the registry clerk, who affixes the registry number and records the letter in a book suitably ruled with the following columns:

1. Registry number of letter.
2. Name of writer.
3. Address.
4. When written.
5. When received.
6. To whom referred.
7. By whom referred.
8. When referred.
9. Date of answer, or indication no answer required.
10. Synopsis of contents.

A special form is sent with letters referred to the Museum, by means of which an accurate record of the disposition of the letter may be kept, and a similar form is used for letters referred to employès of the Smithsonian Institution proper.

The object of this system is, as above stated, to insure that each letter requiring an answer shall receive it with all attainable promptness, or that a record shall be made of the fact that no answer is required, and, as a rule, it is believed that letters are now being answered on the day after receipt, except in the case of the somewhat numerous class referred to, upon which the report of an expert is first necessary. In the latter case, a limit of six days has been fixed upon from the date of receipt in which to answer ordinary routine letters. A report is rendered each week of the letters that are then unanswered This system, while entailing some additional labor, appears to be fully justified by the results.

Representative relations.—In response to an invitation from Dr. Henry Schliemann, forwarded through the Department of State, to designate a representative of the Smithsonian Institution to participate in an International Conference, held on the ruins of ancient Troy during the latter part of March, 1890, Dr. Charles Waldstein, director of the American School of Classical Studies at Athens, was requested to act as representative of the Institution, and he has most kindly complied with this request, transmitting an interesting report of the proceedings of the Conference.

Prof. H. Carrington Bolton courteously represented the Institution at the installation of Dr. Low as president of Columbia College, New York, on February 3, 1890.

Prof. Otis T. Mason was appointed as the representative of the Institution upon a joint board composed of delegates from different bureaus of the Government interested in the subject to consider and decide questions of geographical orthography and nomenclature. This board met for organization at the office of the Superintendent of the U. S. Coast and Geodetic Survey on the 18th of March, and its work is one

that has already proved to be of great value to the Government and to others interested in geographical matters.

I take occasion to express to the Director of the Mint, the Hon. E. O. Leech, my acknowledgments for his kindness in having prepared an intaglio head of the late Professor Henry for certain official correspondence,—an excellent work of art.

U. S. NATIONAL MUSEUM.

The operations of the National Museum are fully described in the separate Report of the Assistant Secretary, in which are included (1) the report of the Assistant Secretary in charge of the Museum; (2) the reports of the curators of the scientific departments of the Museum; (3) special papers based upon and illustrative of collections in the Museum; (4) bibliography of the publications of the Museum and of papers published by Museum officers and other collaborators; (5) a list of the accessions to the Museum during the year.

Increase of the Museum collections.—A small number of specimens were purchased during the year. The necessity of expending a considerable sum of money in the purchase of new material becomes every year more apparent. The donations of friends of the Museum are to a large extent miscellaneous in character, and they frequently duplicate, rather than enlarge and complete, the various series of objects already in the collections. The Museum has now reached a point where the complete presentation of subjects by means of full suites of specimens is of the highest importance, and this can be accomplished only by purchase.

The increase in the number of accessions during the year has been less than in the preceding year by nearly 200 numbers. This is not surprising, since no special efforts have been made to secure new material, excepting in certain directions, in which the completion of special series of objects was desired, in view of the crowded condition of both the storage and exhibition space. This matter has repeatedly been referred to in the more recent reports of the Institution and of the Museum, and efforts have been made to obtain an appropriation from Congress for the construction of a new Museum building. The Senate has acted favorably in regard to the matter, but its action has not received the support of the House of Representatives.

The contributions during the year, although less in number than in the previous year, are, taken as a whole, equal in importance. Especially is this true in the case of material acquired from foreign countries, and of collections received through the assistance of the Departments and Bureaus of the Government.

The extent and character of the accessions during the year and each year since 1881 is shown in the appended table. The total number of specimens received during the year covered by this report is estimated at 81,992.

Name of department.	1882.	1883.	1884.	[1]1885–'86.	1886–'87.	1887–'88.	1888–'89.	[2]1889–'90.
rts and industries:								
Materia medica		4,000	4,442	4,850	5,516	5,762	5,942	[3]5,915
Foods		1,244	1,580	822	877	877	911	1,111
Textiles			2,000	3,063	3,144	3,144	3,222	3,288
Fisheries			5,000	9,870	10,078	10,078	10,078	10,080
Animal products			1,000	2,792	2,822	2,822	2,948	2,949
Graphic arts								[4]600
Transportation and engineering								[4]1,250
Naval architecture			600				600	[5]600
Historical relics				1,002	13,634	14,640	14,990	20,890
Coins, medals, paper money, etc				1,005				
Musical instruments				400	417	427	427	447
Modern pottery, porcelain, and bronzes				2,278	2,238	3,011	3,011	3,132
Paints and dyes				77	100	100	109	197
"The Catlin Gallery"				500	500	500	500	([6])
Physical apparatus				250	251	251	251	263
Oils and gums				197	198	198	213	1,112
Chemical products				659	661	661	688	
Domestic animals								66
hnology			200,000	500,000	503,764	505,464	506,324	508,830
nerican aboriginal pottery			12,000	25,000	26,022	27,122	28,222	29,269
iental antiquities							850	3,485
ehistoric anthropology	35,512	40,491	45,252	65,314	101,659	108,631	116,472	123,677
ımmals (skins and alcoholics)	4,660	4,920	5,694	7,451	7,811	8,058	8,275	8,836
rds	44,354	47,246	50,350	55,945	54,987	56,484	57,974	60,219
rds' eggs and nests			40,072	44,163	48,173	50,055	50,173	51,241
ptiles and batrachians			23,495	25,344	27,542	27,664	28,405	29,050
shes	50,000	65,000	68,000	75,000	100,000	101,350	107,350	122,575
rtebrate fossils								[7]512
ollusks	33,375		400,000	460,000	425,000	455,000	468,000	471,500
sects	1,000		151,000	500,000	585,000	595,000	603,000	618,000
arine invertebrates	11,781	14,825	200,000	350,000	450,000	515,000	515,300	520,000
omparative anatomy:								
Osteology	3,535	3,640	4,214	10,210	11,022	11,558	11,753	12,326
Anatomy	70	103	3,000					
læozoic fossils		20,000	73,000	80,482	84,491	84,649	91,126	92,355
esozoic fossils			100,000	69,742	70,775	70,925	71,236	71,305

[1] No census of collection taken.

[2] The actual increase in the collections during the year 1889–'90 is much greater than appears from comparison of the totals for 1889 and for 1890. This is explained by the apparent absence of any crease in the Departments of Lithology and Metallurgy, the total for 1890 in both of these depart nts combined showing a decrease of 46,314 specimens, owing to the rejection of worthless material.

[3] Although about two hundred specimens have been received during the year, the total number of ecimens in the collection is now less than that estimated for 1889, owing to the rejection of worth ss material.

[4] The collection now contains between 3,000 and 4,000 specimens.

[5] No estimate of increase made in 1890.

[6] Included in the historical collection.

[7] Only a small portion of the collection represented by this number was received during the year 39–'90.

Name of department.	1882.	1883.	1884.	1885–'86.	1886–'87.	1887–'88.	1888–'89.	1889–'90.
Cenozoic fossils	(Included with mollusks.)							
Fossil plants	....	4, 624	7, 291	7, 429	8, 462	10, 000	10, 178	10, 507
Recent plants[1]	....	......	....	30, 000	32, 000	38, 000	38, 459	39, 654
Minerals	....	14, 550	16, 610	18, 401	18, 601	21, 896	27, 690	37, 101
Lithology and physical geology	9, 075	12, 500	18, 000	20, 647	21, 500	22, 500	27, 000	[2]32, 762
Metallurgy and economic geology	....	30, 000	40, 000	48, 000	49, 000	51, 412	52, 076	
Living animals	....	......	....	....	....	220	[3]491	....
Total	193, 362	263, 143	1, 472, 600	2,420,944	2,666,335	2,803,459	2,864,244	2, 895, 104

[1] These numbers have reference only to specimens received through the Museum, and do not include specimens received for the National Herbarium through the Department of Agriculture.

[2] Collections combined in October, 1889, under Department of Geology. The apparent decrease of more than 50 per cent. of the estimated total for 1889 is accounted for (1) by the rejection of several thousands of specimens from the collection, and (2) by the fact that no estimate of the specimens in the reserve and duplicate series is included. Of the total for 1890, about 16,000 specimens consist chiefly of petrographical material stored away for study and comparison in the drawers of table cases.

[3] Transferred to the National Zoological Park.

Catalogue entries.—The number of entries made in the catalogue of the several departments of the Museum during the year is 28,293.

The number of boxes and packages recorded by the registrar as having been received during the year, and entered upon the transportation record of the Smithsonian Institution, is 52,079. Of this number 827 contained specimens for the Museum. Although the total number of packages received is more than three times as great as that for last year, the number of packages containing specimens for the Museum is only a little more than one-third of the number received during 1889.

Co-operation of the Departments of Government.—The friendly interest displayed in the work of the National Museum by officers of the Departments of the Government has been continued. In no previous year has the Museum had occasion to acknowledge more gratefully the courteous assistance rendered by the Secretaries of the Departments and the chiefs of many of the Bureaus.

Through the medium of the Department of State, several United States ministers and consuls have brought their influence to bear in obtaining for the Museum representations of the fauna and flora of the regions in which they are residing.

The Secretary of the Treasury has extended the usual courtesies in connection with the free entry of specimens. Special facilities have been afforded in connection with the visit of Mr. Henry W. Elliott to the Seal Islands of Alaska, which, it is hoped, will result in the addition of several specimens of fur-seal, fishes, and other natural-history objects to the collections. The Coast and Geodetic Survey, the Revenue Marine Division, the Life-Saving Service, and the Light-House Board have assisted collectors for the Museum in special ways.

Several officers of the U. S. Army have made valuable contributions. The Quartermaster's Department has extended important assistance in connection with the transportation of bulky material for the Museum.

From officers of the U. S. Navy many collections have been received from foreign countries, including the West Indies, Liberia, the Samoan Islands, and Mexico.

Through the courtesy of the Secretary of the Interior, the Museum has received a very valuable collection of ethnological specimens from the Indians of the Tulalip Reservation, Washington. The material transmitted to the Museum by the U. S. Geological Survey is large in extent and quite equal in importance to the collections received from that source in previous years.

From the Divisions of Animal Industry, Entomology, Botany, Forestry, and Ornithology and Mammalogy, in the Department of Agriculture, numerous contributions have been received.

Distribution of Duplicate Specimens.—Collections of ethnological, zoological, botanical, and geological specimens, contained in two hundred and one packages, have been distributed during the year to about one hundred and twenty educational establishments at home and abroad. A large number of duplicate sets of minerals and marine invertebrates were included in these distributions.

Numerous applications for duplicate specimens, chiefly minerals, still remain unfilled. It is hoped that during the next fiscal year it will be possible to send out bird-skins and rocks also.

Museum Publications.—This department of the Museum work has been unusually active during the year.

The Museum Reports for 1886 and 1887 have been published. Each of these volumes contains several papers based upon collections in the Museum by Museum officers and other collaborators.

Volume XI of the Proceedings of the National Museum, for 1888, has been issued. This contains xi+703 pages, 60 plates, and 122 text figures. It includes eighty-five papers by forty-three authors, nineteen of whom are officers of the Museum. The papers composing Volume XII of Proceedings of the National Museum, for 1889, are twenty-nine in number (Nos. 761–789); and were all published as separates during the year, although the bound volume has not yet been issued. Commencing with this volume the system of issuing sixteen pages at a time—forming a signature—as soon as sufficient manuscript had accumulated, has been discontinued. Each paper is now printed separately, in advance of the bound volume, and is immediately distributed to specialists.

Five numbers of the Bulletin have been published (Nos. 34–38, inclusive). Bulletin 34 relates to "The Batrachia of North America," by Prof. E. D. Cope. Bulletin 35 contains a "Bibliographical Catalogue of the Described Transformations of North American Lepidoptera," by

Mr. Henry Edwards. Bulletin 36 is entitled "Contributions to t Natural History of the Cetaceans, A Review of the Family Delphinida by Mr. Frederick W. True. Bulletin 38 has the title: "Contributi toward a Monograph of the Insects of the Lepidopterous family Nc tuidæ of Temperate North America," and is a revision of the speci of the genus *Agrotis*. This Bulletin, by Mr. John B. Smith, of Rutge College, New Jersey, was not actually published until after the close the fiscal year, although it was put in type during the year covered this report. The manuscript for other Bulletins relating to deep-s fishes, by Drs. G. Brown Goode and Tarleton H. Bean, and to a descri tion of the metallurgical collection in the Museum, by Mr. Fred P. Dewe has been transmitted to the Government Printing Office.

A large number of papers upon scientific subjects have been pu lished by officers of the Museum and other specialists. They are ferred to in the bibliography of Museum publications, constituting Se tion IV of the separate report of the Assistant Secretary.

Assistance to students.—The usual facilities have been granted to st dents in the various branches of natural history, and several collectio have been lent to specialists for comparison and study. Dr. R. W. Sh feldt, U. S. Army, requested permission to study bird-skeletons. M Bashford Dean, of the College of the City of New York, received fis for study; a collection of bats from the British Museum was furnish to Dr. Harrison Allen, of Philadelphia, for comparison and study; a pa of the Museum collection of Coleoptera was sent for a similar purpose Capt. T. L. Casey, of New York City. Several persons have receiv instruction in taxidermy and photography.

Special researches.—Several of the curators in the Museum are p paring for publication in the Museum Report for 1890 papers which a the result of special investigation and research. Among these may mentioned a hand-book of the geological collections, by Mr. George Merrill; a descriptive paper relating to the collection of humming-bir in the Museum, by Mr. Robert Ridgway; papers relating to Japane religion and Japanese burials, by Mr. Romyn Hitchcock. Other ge tlemen, not officially connected with the Museum, have also prepar papers for publication in the same volume.

The Museum Report each year contains a number of descripti papers of the kind alluded to, and the interest which they have excit among all classes of people has been very great. During this year se eral hundred copies of papers of this character, printed in the more cently published reports of the Museum, have been distributed free cost. Among these may be especially noted the "Hand-Book and Ca logue of the Building and Ornamental Stones in the National Museu by Mr. George P. Merrill,* and the paper entitled "The Exterminati of the American Bison," by Mr. William T. Hornaday."†

* Printed in the report for 1886 and also separately.

† Printed in the report for 1887 and also separately.

Museum library.—The number of publications added to the Library during the year is 12,437, of which 1,479 are volumes of more than 100 pages, 2,250 pamphlets, 8,672 parts of regular serials, and 36 charts. With the exception of the charts these numbers are more than double the receipts of last year. The most notable gift was a nearly complete set of Kiener's "Iconographie des Coquilles Vivantes," illustrated with very beautifully colored plates. This was presented by the Wagner Free Institute of Science, in Philadelphia.

Museum labels.—During the year 3,920 forms of labels have been printed (twenty-four copies of each form) for use in connection with labeling the collections of ethnology, geology, mammals, comparative anatomy, porcelains, oriental antiquities, graphic arts, foods, textiles, and materia medica.

Meetings and lectures.—The use of the Lecture Hall has been granted for lectures and meetings of scientific societies, as The Association of American Agricultural Colleges and Experiment Stations, November 12–15, 1889, inclusive; the American Historical Association, December 28–31; the American Institute of Mining Engineers, February 18, 1890; Memorial Meeting of the Academy of Sciences, March 27; the Geological Society of America, April 17; the National Academy of Sciences, April 15–18, inclusive; Meeting of the Committee on Arrangements of the Geological Congress, April 18; The National Geographic Society, May 2.

The course of Saturday lectures, ten in number, beginning February 1, and ending April 3, was delivered under the direction of the joint committee of the scientific societies of Washington. A course of four lectures relating to the anthropological exhibits at the Paris Exposition in 1889 was given in May by Mr. Thomas Wilson, curator of archæology. A lecture, under the auspices of the National Geographic Society, was delivered on April 11 by Ensign J. B. Bernadou on the subject of "Corea and the Coreans."

Visitors.—The number of visitors to the Museum building during the year ending June 30, 1890, was 274,324. The number of visitors to the Smithsonian building during the same period was 120,894. These figures are considerably less than during 1889, when, on account of the inauguration of President Harrison, immense numbers of people visited the Museum. On March 5, it may be remembered, more than 56,000 people visited the Museum and Smithsonian buildings. The total number of visitors since 1881 to the Museum building is 2,111,949, and to the Smithsonian building, 970,012.

Extension of hours for visiting the Museum.—On December 20 a bill was introduced in the House of Representatives by the Hon. W. H. Crain, having for its object the opening of the Smithsonian and Museum buildings during extra hours. Mr. Crain also introduced a bill later in

the session to provide an electric plant for lighting the building. Neither of these bills has been reported from the committees to whic they were referred.

Museum personnel.—Mr. George P. Merrill has been appointed Curat of the Department of Geology, which combines the functions of t previously existing departments of Lithology and Physical Geolog and of Metallurgy. This change in the administration of these depa ments was made upon the resignation of Mr. Fred P. Dewey, who f several years had been in charge of the metallurgical collections.

Mr. William C. Winlock, of the Smithsonian Institution, was appoint Honorary Curator of the Section of Physical Apparatus in the Nation Museum.

Mr. William T. Hornaday, perhaps the first taxidermist in the cou try, through his extensive knowledge of the habits and natural at tudes of animals, in a very wide range of travel as a field naturali has elevated the standard of his art by the fidelity of his groupings a his skill in the representation of life-like aspects in the plastic for He had rendered valuable service to the National Museum as its c taxidermist, and subsequently as Honorary Curator of the Departm of Living Animals, which led to his appointment as Acting Superin dent of the National Zoological Park. From this position he resig on the 15th of June last.

Dr. Frank Baker was, in June, appointed Honorary Curator of Department of Comparative Anatomy in the Museum, though as has been found necessary to assign Dr. Baker to temporary duty Acting Manager of the National Zoological Park, Mr. F. W. True co tinues to fill the position of acting curator of that department.

A detailed statement relating to the work of the administrative o cers of the Museum will be found in the volume containing the rep of the Assistant Secretary.

Explorations.—In connection with the expedition sent by the Uni States Government to the West Coast of Africa to take observations the eclipse of the sun, the National Museum obtained the privilege sending a naturalist for the purpose of making collections of ethnologi and zoological objects. Mr. William Harvey Brown, of the Natio Museum, was detailed to accompany the expedition. Early in Ju 1890, the first collections were received as the result of his exploratio They included mammals, fishes, insects, plants, reptiles, birds, she rocks, and ethnological objects. Additional collections will doubtl soon be received; and will be referred to in the next report. As outcome of Mr. Brown's exploration work, collections have been ceived from Rev. G. H. R. Fisk, Mr. J. H. Brady, Mr. P. MacOw director of the Botanical Garden at Cape Town, Mr. Frye, of Cape To and others. The thanks of the Smithsonian Institution are especi

lue to several of the officers and sailors of the U. S. S. *Pensacola* for assistance rendered Mr. Brown in his work.

Dr. W. H. Rush, U. S. Navy, has kindly offered to collect marine invertebrates during his expedition to the Azores, Madeira, and the English Channel.

Mr. J. P. Iddings, of the U. S. Geological Survey, has expressed his willingness to bear in mind the requests of the Museum during his expedition to the volcanic regions of Europe.

Mr. E. M. Aaron, of the American Entomological Society, has kindly offered to be of service to the Museum in collecting entomological material during his visit to Jamaica.

Mr. C. R. Orcutt, of San Diego, California, has announced his intention to visit the Colorado desert and the Gulf of California, and to allow the Museum to share the results of his expedition.

Mr. Henry W. Elliott, formerly of the Alaska Commercial Company, is visiting the Seal Islands of Alaska on business connected with the United States Government, and hopes to be able to secure for the Museum some fine specimens of walrus, fur-seal, fishes, and other zoological material.

Department of living animals.—Upon the passage of the bill placing the National Zoological Park under the care of the Board of Regents, the department of living animals of the Museum was merged in the new park and the necessary transfers were made from the Museum rolls. For convenience, therefore, the report in regard to the principal accessions to this department have been included in the report of the acting manager of the Park.

The animals are retained for the present in their sheds in the Smithsonian Grounds for the reason that during the fitting up of the Park they can there be cared for at a much less expense; for instance, two watchmen are now required instead of twenty that would probably be needed at the Park, where each group of animals will be placed in a center from which to grow, a plan that involves the necessity at first of spreading the collection over a considerable area.

The interest in this small collection has constantly increased, and has been manifested by numerous offers of valuable gifts, most of which it has been impossible, through lack of space and immediate accommodations, to accept.

NATIONAL ZOOLOGICAL PARK.

In the early part of this century a naturalist traveling in Sibe stood by the mutilated body of a mammoth still undecayed, which t melting of the frozen gravel had revealed, and to the skeleton of whi large portions of flesh, skin, and hair still clung. The remains we excavated and transported many hundred miles across the froz waste, and at last reached the Imperial Museum at St. Petersbu where, through all these years, the mounted skeleton has justly be regarded as the greatest treasure of that magnificent collection.

Scientific memoirs, popular books, theological works, poems— short, a whole literature—has come into existence with this disco ery as its text. No other event in all the history of such subjects h excited a greater or more permanent interest outside of purely sci tific circles; for the resurrection of this relic of a geologic time in condition analogous to that in which the bodies of contemporaneo animals are daily seen brings home to the mind of the least curi observer the reality of a long extinct race with a vividness which fossils or petrifactions of the ordinary sort can possibly equal.

Now, I am assured by most competent naturalists that few, if a of those not particularly devoted to the study of American anim realize that changes have already occurred or are on the point of tak place in our own characteristic fauna compared with which the dis pearance from it of the mammoth was insignificant. That animal w common to all northern lands in its day. The practical domesticati of the elephant gives to every one the opportunity of observing gigantic creature closely allied to the mammoth, and from which he m gain an approximately correct idea of it. But no such example is hand in the case of the bison, the prong-horn antelope, the elk, the Roc Mountain goat, and many more of our vanishing races.

The student of even the most modern text-books learns that t characteristic larger animals of the United States are those just me tioned, with the moose, the grizzly bear, the beaver, and if we inclu marine forms and arctic American animals we may add the northe fur-seal, the Pacific walrus, the Californian sea-elephant, the manat and still others.

With one or two exceptions out of this long list, men now living c remember when each of these animals was reasonably abundant with its natural territory. It is within the bounds of moderation to affi that unless Congress places some check on the present rate of destru tion there are men now living who will see the time when the anima enumerated will be practically extinct, or exterminated within the li its of the United States. Already the census of some of them can expressed in three figures.

The fate of the bison, or American buffalo, is typical of them "Whether we consider this noble animal," says Audubon, "as an c

ject of the chase or as an article of food for man, it is decidedly the most important of all our American contemporary quadrupeds."

At the middle of the last century this animal pastured in Pennsylvania and Virginia, and even at the close of the century ranged over the whole Mississippi Valley and further west wherever pasturage was to be found. At the present time a few hundred survivors represent the millions of the last century, and we should not have even these few hundred within our territory had it not been for the wise action of Congress in providing for them a safe home in the Yellowstone Park.

Now, for several reasons it has been comparatively easy to trace the decline of the buffalo population. The size of the animal, its preference for open country, the sportsman's interest in it, and its relations to the food-supply of the Western Indians, all led to the observation and record of changes; and accordingly I have made special mention of this animal in representing the advantages of a national zoological park where it might be preserved; but this is by no means the only characteristic creature now threatened with speedy extinction.

The moose is known to be at the present time a rare animal in the United States, but is in less immediate danger than some others. The elk is vigorously hunted and is no longer easily obtained, even in its most favored haunts. The grizzly bear is believed to be rapidly approaching extinction outside of the Yellowstone Park, where, owing to the assiduous care of those in charge, both it and the elk are still preserved. The mountain sheep and goat, which inhabit less accessible regions, are becoming more and more rare, while the beaver has retreated from a vast former area to such secluded haunts that it may possibly survive longer than the other species which I have just enumerated, and which are but a portion of those in imminent danger of extinction.

Among the marine forms the manatee still exists, but, although not exterminated, it is in immediate danger of becomimg so, like the Californian sea-elephant, a gigantic creature, often of greater bulk than the elephant, which has suffered the fate of complete extinction within a few past years; at least it is uncertain whether a single individual actually survives. The Pacific walrus, upon which a large native population has always in great part depended for food and hides, is rapidly following the sea-elephant, and so on with other species.

This appalling destruction is not confined to mammals. Disregarding the birds of song and plumage, to which the fashions of the milliner have brought disaster, nearly all the larger and more characteristic American birds have suffered in the same way as their four-footed contemporaries. The fate of the great Auk is familiar to all naturalists; but it is not so well known that the great Californian vulture and several of the beautiful sea-fowl of our coasts have met the same fate, and that the wild pigeon, whose astonishing flocks were dwelt upon by Audubon and others in such remarkable descriptions and which were long

the wonder of American travelers, with the less known, but magnific ivory-billed woodpecker, and the pretty Carolina parrakeet, have become, if not extinct, among the rarest of birds.

Apart from the commercial value of its skins, the tax upon wh has paid for the cost of our vast Alaskan territory, the singular hab and teeming millions of the northern fur-seal have excited general terest even among those who are not interested in natural history. 1849 these animals abounded from Lower California to the lon Alaskan Isles, and it has been supposed that the precautions taken the Government for their protection on the breeding-grounds of t Pribilov Islands would preserve permanently the still considera remnant which existed after the purchase of Alaska and the destructi of the southern rookeries. But it is becoming too evident that t greed of the hunters and the devastation caused by the general ad tion of the method of pursuing them in the open sea, destroying ind criminately mothers and offspring, is going to bring these hopes naught.

For most of these animals, therefore, it may be regarded as cert that, unless some small remnant be preserved in a semi-domestica state, a few years will bring utter extinction. The American of t next generation, when questioned about the animals once characteri of his country, will then be forced to confess that with the exceptio a few insignificant creatures, ranking as vermin, this broad contin possesses none of those species which once covered it, since the prese generation will have completed the destruction of them all.

The Yellowstone Park is doing excellent work under the prese management, and too much can not be said in praise of the acti which has given it to the country. It is, however, also desirable a necessary that, if these vanishing forms are to be preserved, th should be some zoological preserve or garden nearer the Capital, whe representatives of all these races, not only of the land, but of the wat also, may be preserved under the care of those permanently interest in their protection, in the charge, that is, of men who not only ha special professional knowledge of their habits and needs, but who ma be considered as having an unselfish interest in looking to their prese vation, and who may act as scientific advisers, whenever such advi is deemed desirable by Congress or by the heads of Departments.

Is it realized that nearly all the principal animals indigenous to t United States are either substantially extinct or in danger of beco ing so and is it sufficiently realized that, once extinct, no expenditu of treasure can restore what can even to-day be preserved by prom action of a very simple and definite kind?

It is such considerations as these that have induced me to ask t earnest attention of the Regents, of Congress, and of the country to t immediate necessity for action. The trust is unquestionably for the a vancement of science as well as for the instruction and recreation

the people, and thus becomes a fitting object for the care of the Smithsonian Institution.

In my Report for last year the preliminary steps for the establishment of a Zoological Park in the District of Columbia were detailed. The District of Columbia bill, which received the approval of the President on March 2, 1889, contained an appropriation of $200,000 for the purchase of the land and established a Commission, composed of the Secretary of the Interior, the President of the Board of Commissioners of the District of Columbia, and the Secretary of the Smithsonian Institution, for the purpose of selecting and acquiring a suitable site upon Rock Creek.

The utmost care was exercised to keep within the limits of this appropriation, and the Commission is even able to turn into the Treasury a small balance upon the completion of its work. To accomplish this, however, it was necessary to leave out a strip of land of about 8 acres on the east side of the creek, which it seemed to the Commission very desirable to secure, and I venture to express the hope that Congress will see fit to make special provision for the purchase of the property at an early day.

From a commercial point of view the enterprise has already proved a most successful one, the land having risen in value since its condemnation from 200 to 300 per cent.

At the beginning of the fiscal year the ground had yet to be acquired. A careful consideration of the property in the neighborhood of Rock Creek, described in the act of March 2, 1889, had been made and an area of 166.48 acres selected.* The difficulty of establishing the boundaries of certain tracts described in the older deeds caused a long delay, but the survey was finally completed on the 21st of November, 1889.

* The following list shows the tracts in detail and the amount eventually to be paid for each:

Land for the National Zoological Park.

Owner.	Acres.	Amount paid.	How obtained.
Miss A. E. J. Evans	94.050	$94,860.00	By agreement.
H. D. Walbridge	14.450	14,450.00	Do.
Woodley Park Syndicate	7.453	5,875.00	Do.
Henry C. Holt	13.360	40,000.00	Do.
Mrs. M. Caney	1.440	3,000.00	Do
Mrs. E. L. Dunn *et al*	.392	170.76	Do.
Pacificus Ord	24.570	16,836.48	By condemnation.
J. P. Klingle	6.180	9,270.00	Do.
Union Benevolent Association	1.700	3,000.00	Do.
E. E. Hayden	.670	1,897.00	Do.
McPherson & Finley	.315	1,372.00	Do.
James Kervand	1.060	233.10	Do.
United States (part Quarry road)	.846	......	Do.
Total	166.486	190,964.34	

A map of the park, showing the location and quantity of each lot, was filed in the public records of the District of Columbia. On examination of the list it will be seen that for 131.14 acres an agreement was effected with the owners as to the sum to be paid. For 34.49 acres no such agreement could be made, and the Commission therefore took the course prescribed by the act of March 2, 1889, for this contingency, and petitioned the Supreme Court of the District to assess the value of the land. This was done by three appraisers appointed by the Court, and the finding of the appraisers was approved by the President of the United States. At the close of the year title deeds had already passed for the greater portion of the property.

The site thus selected is, it is believed, admirably suited for the purpose for which it is designed. Situated at a convenient distance from the city in a region of remarkable natural beauty, it has a surface of great variety, offering unusual advantages of varied exposure for animals requiring different treatment. While some portions still retain the original forest, others are cleared or covered by a dense second growth of pine, excellent for cover and producing conditions similar to those of the natural haunts of many of the animals it is proposed to preserve. An abundant supply of water is furnished to the lower portions by Rock Creek, a small perennial stream that during freshets swells to considerable size, and at intervals of years, to rare but destructive floods. A number of small runlets or "branches" fall into the creek giving an effective drainage to all parts of the park. The system of water ways has for the most part been cut by erosion, so that the hill-sides and valleys usually present smooth, rounded slopes, practicable for roads and walks; yet this is agreeably varied at several places by an outcropping of the underlying rock, giving a somewhat bolder character.

In the Appendix will be found a map showing the situation of the Zoological Park with reference to the city of Washington, and following it a second map giving, on a somewhat larger scale, the outline of the park and its principal topographical features.

Having obtained the site it became necessary to procure means for the organization and maintenance of the park. The Commission accordingly, under date of January 16, 1890, addressed a letter to Congress, concluding with the following words:

Before the expiration of the present fiscal year the Zoological Park Commission will have completed the duties with which it was charged by the act of Congress which called it into existence, and the title to the lands it has purchased will be vested in the United States. Pending the completion of the condemnation proceedings now in progress, and the submission of a final report, it is extremely desirable that Congress should enact further legislation in regard to the park. The Commission has no authority to put up fences and lay out roads or grounds, or to erect buildings, nor is it even certain that it has the right to accept donations. The park is declared by Congress to be "for the advancement of science and the instruction and recreation of the people." In the construction of ponds and lakes, and the erection of inclosures

and buildings for the purposes of zoological science, a stage will soon be reached where scientific direction seems obviously desirable; and it is respectfully represented to Congress that any means for laying out and improving the grounds can be most advantageously used in view of the purpose of Congress as to the ultimate disposition of the park now when the foundations of its future usefulness are being laid. If the very considerable collection of living animals now in the custody of the Smithsonian Institution is to form the nucleus of the zoological park collection its transfer should be effected by legislative enactment and suitable measures taken for its maintenance. The Commission is of the opinion that the collection referred to should, with the consent of the Regents of the Institution, be transferred to the Zoological Park as soon as possible after the Government takes full possession of the site.

JOHN W. NOBLE,
Secretary of the Interior,
J. W. DOUGLASS,
Prest. Board Com. Dis. Col.,
S. P. LANGLEY,
Secretary Smithsonian Institution,
Commissioners for the establishment of a Zoological Park in the District of Columbia.

After thorough consideration the following act was passed placing the park under the direction of the Regents of the Smithsonian Institution, and transferring to it the collection formerly under the charge of the United States National Museum:

AN ACT for the organization, improvement, and maintenance of the National Zoological Park.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That the one-half of the following sums named, respectively, is hereby appropriated out of any money in the Treasury not otherwise appropriated, and the other half out of the revenues of the District of Columbia, for the organization, improvement, and maintenance of the National Zoological Park, to be expended under the direction of the Regents of the Smithsonian Institution, and to be drawn on their requisition and disbursed by the disbursing officer for said Institution:

For the shelter of animals, fifteen thousand dollars.

For shelter-barns, cages, fences, and inclosures, and other provisions for the custody of animals, nine thousand dollars.

For repairs to the Holt mansion, to make the same suitable for occupancy, and for office furniture, two thousand dollars.

For the creation of artificial ponds and other provisions for aquatic animals, two thousand dollars.

For water supply, sewerage, and drainage, seven thousand dollars.

For roads, walks, and bridges, fifteen thousand dollars.

For miscellaneous supplies, materials, and sundry incidental expenses not otherwise provided for, five thousand dollars.

For current expenses, including the maintenance of collections, food supplies, salaries of all necessary employees, and the acquisition and transportation of specimens, thirty-seven thousand dollars.

SEC. 2. That the National Zoological Park is hereby placed under the directions of the Regents of the Smithsonian Institution, who are authorized to transfer to it any living specimen, whether of animals or plants, now or hereafter in their charge, to accept gifts for the park at their

discretion, in the name of the United States, to make exchanges of specimens, and to administer the said Zoological Park for the advancement of science and the instruction and recreation of the people.

SEC. 3. That the heads of Executive Departments of the Government are hereby authorized and directed to cause to be rendered all necessary and practicable aid to the said Regents in the acquisition of collections for the Zoological Park.

Approved, April 30, 1890.

As it seemed desirable to have at once expert advice on the subject of laying out and improving the park, Mr. Frederick Law Olmsted, a distinguished landscape gardener, was requested to make a preliminary inspection of the ground and to express an opinion as to what, under the conditions imposed by the primary objects of the law, would be the best general disposition to make of it. It soon became evident that a further survey was necessary in order to fix the boundaries of the maximum rise to be expected from Rock Creek. This stream, ordinarily small, drains a water-shed having an area of some 83 square miles, with a slope so considerable that after copious rains the water rapidly rises far beyond its usual limits and becomes destructive to any buildings or other fixtures situated along its course. A remarkable inundation of this character occurred in June, 1889, the extent of which was noted at several points along the creek. It would be evidently impracticable to place any buildings of importance within the area subject to these heavy floods, and the suitable locations and plans for the bridges to be constructed could not be prepared until their height and span were determined with reference to the maximum rise of water. The survey of the creek was not completed at the close of the year, but it has since been finished as shown in the map previously referred to.

Having once secured the picturesque features of the land from obliteration by the rapid encroachment of the city, it has been the policy to proceed slowly with improvements and to utilize the natural advantages of the location, interfering as little as possible with its original aspects. Even with these economical principles the cost of converting the tract to the uses of a park is far beyond what would ordinarily be imagined, for it should be remembered that the cost of improving Central Park, New York, has already been not less than $14,000 per acre, and that of Prospect Park, Brooklyn $9,000 per acre, while that of the large Franklin Park, Boston, is estimated at $2,900 per acre.

In following this policy and keeping within the limits of the appropriations, no immediate provision has been made for the considerable expense involved in opening at once to the public the entire area of 166 acres. The complete establishment of the park in a manner befitting its national character will be a work of considerable time, and for the present it has been deemed advisable to set aside nearly 40 acres, selected on account of accessibility and moderate elevation, as well as on account of its being adapted to the purposes of the park without great expense, while a further tract of some 15 acres will be so arranged that

it can be opened to the public, though it may not have a strictly park-like cultivation. There will thus be free to the public, it is hoped by next year, between 50 and 60 acres, an area larger than that of the Zoological Gardens in the Regents Park of London, or the Jardin des Plantes of Paris.

A distinct area of some 10 or 15 acres will be reserved in another portion of the park for administrative and other purposes requiring seclusion, and will contain a lodge for the resident superintendent, offices, stable, infirmary for animals, and a proposed laboratory.

It should be remembered that a most important feature of this undertaking is that it is not only a place for public resort and amusement, but it is also intended to furnish secluded places for the breeding and restoration of the various animals indigenous to this country.

At London and Paris the zoological gardens are chiefly for the amusement of the people by the exhibition of curious and foreign animals, and for the benefit of the naturalist; our paramount interest is to preserve the indigenous animals, and then to provide, in the words of the act, for the instruction and amusement of the people.

Though anticipating the report for the coming year it does not seem out of place in the present connection to allude to the fact that the Secretary, in his private capacity, has been appointed by the President one of the commissioners of the more extensive national park upon Rock Creek, contiguous to the Zoological Park, a charge which he has accepted with some reluctance on account of the pressure of present official duties, but with a feeling that by reason of the necessary intimate connection between the two national parks the public interests will be subserved by this action.

I can not close the report in relation to this new undertaking of the Institution without reference to the loss we have sustained in the death of Senator Beck, who, though not upon the Board of Regents, took a lively interest in the Institution, and a special interest in establishing and placing under its care the preservation of the natural scenery in the neighborhood of the Capital.

I regret, also, to report that near the close of the year, the Institution was reluctantly obliged to accept the resignation of Mr. W. T. Hornaday, curator of living animals in the National Museum, who, having been assigned to the duty of superintendent of the park under the Commission, it was hoped would be able to accept the position of superintendent of the park upon its transfer to the Board of Regents. His efforts assisted the Commission greatly in the selection of the land, and did much to insure the success of the measure before Congress.

Dr. Frank Baker honorary curator of the Department of Comparative Anatomy in the Museum, was appointed on June 1, 1890, acting manager of the Zoological Park.

BUREAU OF ETHNOLOGY.

Ethnologic researches among the North American Indians were continued by the Smithsonian Institution, in compliance with acts of Congress, during the year 1889–'90, under the direction of Maj. J. W. Powell, Director of the U. S. Geological Survey.

The work of the Bureau of Ethnology during the year has proceeded along accustomed lines. Investigations in relation to the Sign Language and Pictography of the American Indian, preliminary reports of which subjects have appeared in annual reports of the Bureau, have been discontinued and the final results of this study will soon appear. Investigations of the Mounds of the eastern United States have also been practically brought to an end and the final discussion of the subject will speedily be published.

The archæologic researches which have been inaugurated in the vicinity of Washington have already been fruitful of results of more than local interest. Excavations into the quarry sites and workshops of the district have shown that the class of archæologic objects from this vicinity, which have hitherto been assumed to be palæolithic and to represent the rude implements of primitive man, are in fact nothing but the "rejects" of much more recent times; and that however far back in point of time some of them may date, they are not separable from the rejects of the historic Indian.

As usual, considerable attention has been paid to the collection of linguistic material, both because it is thought that languages form the only safe basis for classifying peoples, and because no material relating to our Indians is vanishing with such rapidity. The latter reason has also impelled the collection of Indian mythology. Myths are hardly more enduring than the languages in which they are preserved. Though they may persist to some extent after a language decays and falls into partial disuse, it is only in a degraded and emasculated form that deprives them of their chief value, as embodying the religious ideas and the philosophy of primitive peoples.

The medicine practices of the Indian have also received much attention and a large number of the plants used in the Indian *Materia Medica* have been collected, preserved, and their Indian and botanical names obtained. In addition, the formulas and secret practices attending their use have been carefully recorded. As was to be expected, it has been found that so intimately interwoven are the Indian systems of religion and medicine that it is practically impossible to say where the one ends and the other begins. It has also been demonstrated that contrary to popular belief, the chief and almost sole effacacy possessed by so-called Indian medicine lies, not in the inherent virtue of the

plants used, but in the mystic properties imparted to them by the sorcerers or professional "Medicine men."

During the year one of the Bureau assistants visited Casa Grande, in Arizona, with a view to determining the best method to give effect to the act passed by Congress for preserving the ancient ruin. The preservation from the hand of the vandal and the effects of time and exposure of the more important Indian mounds and ruins which are situated within the national domain, is one that may well receive attention. The land upon which many of them are situated is of little value for economic purposes, and the comparatively small outlay required for their restoration, when such is necessary, and for their preservation, is small when contrasted with their historical and archæological value and their popular interest.

No phase of tribal life and society presents a more curious and interesting study than that exhibited by the Pueblo Indians, who, in many respects, were far in advance of less sedentary tribes. Study of one of them, Sia, was begun during the year, and other Pueblos will be visited and studied in succession.

Further details respecting the work of the Bureau will be found in the report of its director, Major J. W. Powell, given in full in Appendix I.

NECROLOGY.

SAMUEL SULLIVAN COX.

I am called upon to record here the death of one of the most public-spirited and versatile members of Congress that have served upon the Board of Regents, the Hon. Samuel Sullivan Cox, a member of the House of Representatives, who was born at Zanesville, Ohio, September 30, 1824, and first elected a Regent on December 19, 1861. He died at his home in New York on the 10th of September, 1889.

At a meeting of the Board, held on the 8th of January, 1890, a committee was appointed to prepare resolutions on the services and character of Mr. Cox, consisting of the Secretary, Hon. Joseph Wheeler, Dr. Welling, and Hon. Mr. Lodge, and they subsequently reported as follows:

To the Board of Regents:

Your committee report that the Hon. S. S. Cox was first appointed a Regent of the Smithsonian Institution December 19, 1861, and that he filled that office, except for intervals caused by public duties, to the time of his death.

While he was not a regular attendant at all the meetings of the Board, he was ever ready to advance the interests of the Institution and of science, either as a Regent or as a member of Congress; and although such men as Hamlin, Fessenden, Colfax, Chase, Garfield, Sherman, Gray, and Waite, in a list comprising Presidents, Vice-Presidents, Chief-Justices, and Senators of the United States, were his associates,

there were none whose service was longer or more gratefully to be remembered, nor perhaps any to whom the Institution owes more than to Mr. Cox.

The regard in which his brother Regents held Mr. Cox's accuracy of characterization and his instinctive recognition of all that is worthiest of honor in other men may be inferred from the eulogies which he was requested by them to deliver among which may be particularly mentioned the one at the commemoration in honor of Professor Henry in the House of Representatives. But though these only illustrate a very small part of his services as a Regent, your committee are led by their consideration to recall that his first act upon your Board was the preparation and delivery of an address at the request of the Regents on their late colleague, Stephen A. Douglas, and that on this occasion he used words which your committee permit themselves to adopt, as being in their view singularly characteristic of Mr. Cox himself:

"It was not merely as one of its Regents that he showed himself the true and enlightened friend of objects kindred to those of this establishment; he ever advocated measures which served to advance knowledge and promote the progress of humanity. The encouragement of the fine arts, the rewarding of discoverers and inventors, the organization of exploring expeditions, as well as the general diffusion of education, were all objects of his special regard."

In view of these facts it is—

Resolved, That in the death of Hon. Samuel Sullivan Cox the Smithsonian Institution has suffered the irreparable loss of a long-tried friend, the Board of Regents of a most valued associate and active member during fifteen years of service, and the country of one of its most distinguished citizens.

Resolved, That the Board of Regents desire to express their deep sympathy with the bereaved family of the deceased, and that a copy of these resolutions be transmitted to the widow of their late associate.

Mr. Cox was descended from a long line of distinguished ancestors. His father, Hon. Ezekiel Taylor Cox, who moved from New Jersey to Zanesville early in the century, held the position of State senator and clerk of the supreme court of Ohio; his grandfather, General James Cox, was an officer in the Revolution, speaker of the New Jersey assembly, and member of Congress at the time of his death; his great-grandfather, Judge Joseph Cox, was a distinguished man of his time, as were his great-great-grandfather, James Cox, and his great great-great-grandfather, Thomas Cox, one of the original proprietors of the province of East New Jersey.

Upon the completion of a classical course Mr. Cox studied law, and at the age of twenty-five, turning his attention to journalism, was the editor of the Columbia Statesman; at twenty-nine he was the chairman of the committee of the Democratic party of Ohio. When scarcely more than thirty he was offered an appointment as secretary of legation to Great Britain, but declined the honor, though he afterwards accepted a similar position and represented the United States at Peru. At thirty-two he was elected to Congress and continued as a member of that body, almost without interruption, for a period of over thirty years. He was elected Speaker *pro tempore* of the House of Representatives

in 1876, and was minister to Turkey during the first part of President Cleveland's administration, receiving from the Sultan shortly after this mission the degree of the order of the Mejidieh.

Of Mr. Cox's political career it is unnecessary to speak. The unanimity with which his fellow-Congressmen hastened to pay tribute to his memory, in terms most glowing and affectionate, attests his esteem in the House of Representatives. No one upon the floor of the House of Representatives in late years has appreciated more fully or has championed to such an extent the cause of science. To him the scientific departments of the Government looked for assistance and appreciation; as a member of the Board of Regents he was a firm supporter of the liberal policy laid down for the Institution by Professor Henry.

Respectfully submitted.

S. P. LANGLEY,

Secretary of the Smithsonian Institution.

APPENDICES TO SECRETARY'S REPORT.

APPENDIX I.

REPORT OF THE DIRECTOR OF THE BUREAU OF ETHNOLOGY.

SIR: I have the honor to present the following report upon the work of the year, dividing it for convenience into two general heads, viz, field work and office work.

FIELD WORK.

The field work of the year is divided into (1) mound explorations and (2) general field studies, the latter having been directed during the year chiefly to archæology, language, religious practices, and pictography.

Mound explorations.—The work of exploring the mounds of the eastern United States was, as in former years, under the superintendence of Prof. Cyrus Thomas. During this year he discontinued explorations in person, being engaged almost the entire time upon the preparation of the second volume of his report and of an additional bulletin, with accompanying maps of the archæologic localities.

Mr. Henry L. Reynolds, however, was employed during the summer in exploring the works in Manitoba and the two Dakotas with special reference to their types and distribution. The results of this investigation proved very satisfactory, as the types within this area are found to be unusually well defined, according to physical structure and contents. While thus employed other archæological remains were noted and examined, such as the bowlder outlines of circles and animals and the ancient village sites on the Missouri River. A full report concerning these investigations will appear in the forthcoming report of Professor Thomas. Mr. Reynolds also made a visit to certain earthworks in Iowa and Indiana for the purpose of ascertaining their types. In the autumn he was employed in South Carolina and Georgia exploring the mounds of that section, about which little was known. Two mounds—a large one on the Wateree River, below Camden, South Carolina, and one on the Savannah River, Georgia—proved of special interest. The contents of the latter consisted of as fine specimens of every class of primitive art as have been found in mounds.

Mr. James D. Middleton, who had acted as a regular assistant from the organization of the division, was engaged during the month of July, 1889, in surveying and making plats of certain ancient works of Michigan and Ohio. At the end of the month he resigned his position in the Bureau.

Mr. James Mooney, although engaged in another line of research, obtained important information for the Mound Division, in reference to the location, distribution, and character of the ancient works of the Cherokee in western North Carolina and adjoining sections.

General Field Studies.—In the autumn of 1889 Mr. W. H. Holmes was directed to take charge of the archæologic field-work of the Bureau. In September he began excavations in the ancient bowlder quarries upon Piney Branch of Rock Creek, near Washington. A trench was carried across the principal quarry, which had a width of more than 50 feet and a depth in places of 10 feet. The ancient methods of quarrying and working the bowlders were studied and several thousands of specimens were col-

lected. Work was resumed in the next spring and five additional trenches were opened across widely separated portions of the ancient quarries. Much additional information was collected, and many specimens were added to the collection. In June work was commenced on another group of ancient quarries, situated north of the new Observatory, on the west side of Rock Creek. Very extensive quarrying and implement-making had been carried on in this place. The conditions and phenomena were almost identical with those of the Piney Branch site. Subsequently an ancient soapstone quarry near Tenallytown was examined. The ancient pitting corresponded quite closely with that of the bowlder quarries and the condition of the pits indicated equal age.

Dr. W. J. Hoffman proceeded early in July to White Earth Reservation, Minnesota, to continue the collection and study of mnemonic and other records relating to the Midē′wiwin or "Grand Medicine Society" of the Ojibwa Indians. He had already spent two seasons with this tribe, and having been satisfactorily prepared, was initiated into the mysteries of the four several degrees of the society, by which means he was enabled to record the ceremonials of initiation, which was desired by the Indians, so that a complete exposition of the traditions of the Ojibwa cosmogony and of the Midē′ Society could be preserved for the information of their descendants. Through intimate acquaintance with, and recognition by, the Midē′ priests, Dr. Hoffmann secured all the important texts employed in the ceremony—much of which is in an archaic form of speech—as well as the musical notation of songs sung to him for that purpose; also the birch-bark records of the society, and the mnemonic songs on birch-bark, employed by the Midē′ priests, as well as those of the Jĕ′ssakki′d and the Wâbĕnō′, which represent two other grades of Shamans.

The so-called cosmogony charts, four versions of which were secured, had not previously been exhibited to a white man, nor to Indians until after the necessary fees had been paid for such service, preparatory to admission into the society.

He also secured, as having connection with the general subject, a list of plants and other substances constituting the *materia medica* of the above-named locality, the method of their preparation, administration, and reputed action, the whole being connected with incantation and exorcism.

Mr. Victor Mindeleff made a short trip (from December 7 to January 20) to the ruin of Casa Grande, in Arizona, visiting also the sites of Mr. F. H. Cushing's work while in charge of the Hemenway expedition. Plans and photographs were secured on this trip, and fragments of typical pottery were collected from the principal ruin visited. Casa Grande was found to be almost identical in character with the many ruins scattered over the valleys of both the Gila and Salado.

On July 3 Mr. James Mooney started on a third trip to the Cherokee reservation in North Carolina, returning November 17. During this time he devoted his attention chiefly to the translation and study of the sacred formulas used by the Shamans, obtained by him during a previous visit. In this work he employed the service of the most prominent medicine men, among them being the writers of some of the original formulas, and obtained detailed explanations of the accompanying ceremonies and the theories upon which they were based, together with descriptions of the mode of preparing the medicine and the various articles used in the same connection. He was also permitted to witness a number of these ceremonies, notedly the solemn rite known as "going to water." About three hundred specimens of plants used in the medicine practice were also collected, with their Indian names and uses, in addition to about five hundred previously obtained. These plants were sent to the botanists of the Smithsonian Institution for identification under their scientific names. The study of these Cherokee plant names, in connection with the medical formulas, will throw much light upon Indian botanic classification and therapeutics. The study of the botany is a work of peculiar difficulty, owing to the absence of any uniform system among the various practitioners. Attention was also given to the ball play, and several photographs of different stages of the ball dance were secured. One of the

oldest men of the tribe was also employed to prepare the feather wands used in the eagle dance, the pipe dance of the prairie tribes, and the calumet dance spoken of by the early Jesuit writers, which has now been discontinued among the Cherokees for about thirty years. These wands were deposited in the National Museum as a part of the Cherokee collection, obtained on various visits to the reservation.

A considerable amount of miscellaneous information in regard to myths, dances, etc., was obtained, and a special study was made of their geographic nomenclature for the purpose of preparing an aboriginal map of the old Cherokee country. With this object a visit was made to the outlying Indian settlements, especially that on Cheowah River, in Graham County, North Carolina, and individuals originally from widely-separated districts were interviewed. The maps of the Geological Survey, on a scale of 2 miles to an inch, were used in the work, and the result is a collection of probably more than one thousand Cherokee names of localities within the former territory of the tribe, given in the correct form, with the meaning of the names and whatever local legends are connected. In North Carolina practically every local name now known to the Cherokees has been obtained, every prominent peak or rock, and every cove and noted bend in a stream having a distinctive name. For Georgia and a portion of Tennessee the names must be obtained chiefly from old Indians now living in the Indian Territory. It may be noted here that as a rule the Cherokees and some other tribes have no names for rivers or settlements. The name belongs to the district and is applied alike to the stream, town, or mountain located in it. When the people of a settlement remove, the old name remains behind, and the town in its new location takes the name attached to the new district. Each district along a river has a distinct name, while the river as a whole has none, the whole tendency in Indian languages being to specialize. The last six weeks of this field season were spent by Mr. Mooney in visiting various points in North and South Carolina, Georgia, Tennessee, and Alabama, within the former limits of the Cherokees, for the purpose of locating mounds, graves, and other antiquities for an archæologic map of their territory, and collecting from former traders and old residents materials for a historic sketch of the tribe.

Mr. Jeremiah Curtin spent July, and until August 28, 1889, at various points on the Klamath River, from Orleans Bar to Martin's Ferry, Humboldt County, California, in collecting myths and reviewing vocabularies of the Weitspekan and Ehnikan languages. From August 30 to September 10 he was at Blue Lake and Arcata, Humboldt County, California, engaged in taking down a Wishoshkan vocabulary and collecting information concerning the Indians of the region thereabout. Arriving in Round Valley, Mendocino County, California, September 16, he remained there till October 16, and took vocabularies of the Yuki and Palaihnihan language. From Round Valley he went to Niles, Alameda County, California, where he obtained partial vocabularies of three languages formerly spoken in that region. Of these one was spoken at Suisun, another was kindred to the Mariposan, a third was Costanoan. On October 27 he arrived in Redding, Shasta County, California, where he obtained a considerable addition to his material previously collected in the form of myths and additions to the Palaihnihan vocabulary. During this work he visited also Round Mountain. On January 10, 1890, he returned to office work.

From July 10 to November 9, 1889, Mr. J. N. B. Hewitt was engaged in field work. Until September 7 he was on the Onondaga reservation, near Syracuse, New York, where legends, tales, and myths were collected and recorded in the vernacular; also accounts of the religious ceremonies and funeral rites were obtained, the terms forming the Onondagan scheme of relationships of affinity and consanguinity were recorded, and valuable matter pertaining to the league and its wampum record was also collected.

From the last mentioned date to the 9th of November he was engaged on the Grand River reservation in Canada, where he successfully made special effort to obtain the chants and speeches used in the condolence council of the league. The religious doc-

trines and beliefs of the pagan Iroquois were recorded; plant and animal names were collected; many religious and gentile songs were secured, and accounts of the principal Iroquoian "medicines" in the vernacular were obtained. A Wyandot vocabulary was also recorded.

Mrs. T. E. Stevenson left Washington in March, 1890, to study the Sia, Jemez, and Zuñi Indians. She made Sia her first point of investigation, and found so much of ethnologic interest in this Pueblo that she continued her work there to the end of the fiscal year engaged in making a vocabulary and studying the habits, customs, mythology, and medicine practices of these people. She has been admitted to the ceremonials of the secret societies and has made detailed accounts of them, the altars being photographed by Miss M. S. Clark, who accompanied her. Her investigations so far have resulted in a clear exposition of the religion of the people.

OFFICE WORK.

The Director was engaged during the year, when his other duties would permit, in the preparation of a work on the characteristics of Indian languages.

Col. Garrick Mallery, U. S. Army, was occupied in continued study of sign language and pictography with the collection and collation of additional material obtained by personal investigation, by correspondence, and by the examination of authorities. This work was performed with special reference to the preparation for early publication of a monograph on each of those subjects, that on pictography to be first presented. The re-arrangement and revision of material already published in the preliminary papers on the sign language and on the pictographs of the North American Indians which respectively appeared in the first and fourth annual reports of this Bureau, and the insertion of matter obtained later by exploration and research, have been conjoined with discussion and comparison. By this treatment it is hoped that the monographs on sign language and pictography, having as their text the attainments of the North American Indians in those directions, may contribute to the understanding of similar exhibitions of evanescent and durable thought-writing, whether still employed in other parts of the world or now only found in records of material remains.

During the fiscal year Mr. H. W. Henshaw was engaged, in addition to his administrative duties, in assisting the Director in the final preparation of the linguistic map of North America north of Mexico, and the accompanying report, which is now completed and in the hands of the printer. He also began the final revision for the printer of his dictionary of Indian tribal names.

Rev. J. Owen Dorsey completed his editorial work in connection with the publication of Riggs' Dakota-English Dictionary. He wrote articles on the following subjects: Measures and valuing; The Dha-du-ghe Society of the Ponka tribe; Omaha dwellings, furniture, and implements; Omaha clothing and personal ornaments; Ponka and Omaha songs; The places of gentes in Siouan camping circles; Winnebago folklore notes; Teton folklore; Omaha folklore; The gentile system of the Siletz tribes; and a Dakota's account of the sun-dance. He revised some of his Omaha and Ponka genealogical tables and began the arrangement of Kansa tables of a similar character. He continued the elaboration of his monograph on Indian personal names, and completed the following lists in which the Indian names precede their English meanings: Winnebago, 383 names; Iowa, Oto, and Missouri, 520; Kwapa, 15; and Kansa, 604.

Dr. Dorsey finished the preparation of his texts for Contributions to North American Ethnology, Vol. 6, The Çegiha Language. Part II. Additional myths, stories, and letters, and corrected proof for the volume as far as page 651. He prepared a manuscript of other Omaha and Ponka letters, to be published as a bulletin. He began an article entitled "A study of Siouan cults," for which over forty colored illustrations were prepared by Indians, under his direction; and of this article he

completed four chapters, treating of the cults of the Omaha, Ponka, Kansa, Osage, Iowa, Oto, Missouri, and Winnebago tribes, and half of a fifth chapter that describes the cults of the Dakota and Assiniboin. When not otherwise engaged, he was occupied in making entries on slips for the Ȼegiha-English Dictionary. From September to December, 1889, he obtained from George Miller, an Omaha, who came to Washington to aid him, additional myths, legends, letters, folklore, and sociologic material, grammatical notes and corrections of dictionary entries, besides genealogical tables arranged according to the subgentes as well as the gentes of the Omaha tribe.

During the year Mr. Albert S. Gatschet was wholly engaged in office work. He finished his last draught of the "Klamath Grammar," a language of southwestern Oregon, making numerous additions, also appendices, as follows: Idioms and dialectic differences in the language; colloquial form of the language; syntactic examples; complex synonymous terms; roots with their derivatives. The typographic work on the grammar was terminated, the proofs and revises having all been read by the author. The last portion of the entire work, being the "ethnographic sketch of the Klamath people," was then re-written from earlier notes while consulting the best topographic and historical materials obtainable. Mr. Gatschet also drew a map of "the headwaters of the Klamath River," the home of the tribes, being on a scale of 15 miles to the inch, which will appear as the frontispiece in Part I. The "ethnographic sketch" is now in the hands of the printer.

Mr. Jeremiah Curtin was engaged from January 10 to June 30, 1890, in arranging the myth material collected by him in the field and in copying vocabularies. The Hupa, Ehnikan, and Wishoshkan vocabularies were finished and the Yana partly done on June 30, 1890.

The office work of Dr. W. J. Hoffman consisted in arranging the material gathered by him during the preceding three field seasons and in preparing the manuscript for publication, which has been completed. During the first three months of the year 1890 a delegation of Menomoni Indians were at Washington, District of Columbia, on business connected with their tribe, and during that period Dr. Hoffman obtained from them a collection of facts relating to mythology, social organization and government, the gentile system and division of gens into phratries, together with many facts relating to the Mitä'wit, or "Grand Medicine Society" as they term it. These are interesting and valuable, as some portions of the ritual explain doubtful parts of the Ojibwa phraseology, and *vice versa*, although the two societies differ greatly in the dramatized portion of the forms of initiation.

On his return from the field in November Mr. James Mooney devoted his attention to the elaboration of the sacred formulas already obtained. Two hundred of these formulas, being about one-third of the whole number, have now been translated. In each case the translation from the original manuscript in Cherokee characters is given first, then a translation following the idiom and spirit of the original as closely as possible, and finally an explanation of the medicine and ceremonies used and the underlying theory. About one-half of the whole number relate to medicine. The others deal with love, war, self-protection, the ball play, agriculture, and life-conjuring. A preliminary paper with a number of specimen formulas will appear in the seventh annual report of the Bureau. The whole collection will constitute a unique and interesting contribution to the aboriginal literature of America. All the words occurring in the formulas thus far translated have been glossarized, with grammatic notes and references from the original texts, making a glossary of about two thousand words, a great part of which are in the archaic or sacred language. Several weeks were also given to the preparation of an archæologic map of the old Cherokee country from materials collected in the field and from other information in possession of the Bureau.

During the year Mr. W. H. Holmes has been chiefly engaged in the preparation of papers on the Arts of the Mound Builders, to form a part of the monograph upon the Mound Builders, by Prof. Cyrus Thomas. Four papers are contemplated; one upon

Pottery, a second upon Art in Shell and Bone, a third upon Textile Fabrics, and a fourth upon Pipes. Three of these papers are well advanced towards completion. In addition to this work he has prepared papers relating to his field explorations. These include a report upon excavations in the ancient quartzite bowlder workshops and the soapstone quarries of the District of Columbia, and a rock shelter in West Virginia. Portions of these papers have been published in the American Anthropologist.

Mr. James C. Pilling has continued to devote such time as he could command for the purpose to the preparation of bibliographies of the languages of North America. At the close of the fiscal year 1888–'89 the proof-reading of the Bibliography of the Muskhogean Languages was completed, but the edition was not ready for delivery. It was delivered August 8, 1889.

After the Muskhogean Bibliography had been finished, work was at once begun on the Algonquian, by far the largest of those yet undertaken. Much of the material for this was already in hand, the collection having been gradually pursued during several years preceding, and the greater part of the work remaining consisted in assembling, arranging, revising, and verifying that material. August 16–22 were profitably spent by Mr. Pilling in the Lenox, Astor, and New York Historical Society libraries, at New York City, and the Massachusetts Historical Society, Boston Athenæum, and Boston Public libraries, at Boston, chiefly in verifying and revising the material in hand. The first portion of the manuscript was transmitted to the Public Printer November 15, 1889. At the close of the fiscal year final proof had progressed to the two hundred and fifty-eighth page, carrying the work approximately half way to completion.

From the 1st to the 10th of July, 18·9, Mr. J. N. B. Hewitt was engaged in collating and recording Iroquoian proper names, both of persons and places, as they occur in the narratives of the early explorers and historians of the pristine habitat of the Iroquoian peoples. Afterwards, to the 9th of November, he was employed in field work.

Upon his return to the office and until the end of the fiscal year he was engaged in translating and annotating the myths, legends, tales, and all of the other matter which he had previously collected in the field; and in translating and recording for easy reference, for the purpose of verification and exposition of the matter so collected, the mythologic, ethnographic, and other anthropologic data found in the early French narratives of the New World, and especially that which is found in the works of Champlain, Lafitau, Charlevoix, and in the Jesuit Relations. Much linguistic material has been obtained from the translations of the matter which Mr. Hewitt personally collected while engaging in field work.

Prof. Cyrus Thomas was personally engaged during the entire year on the preparation of his report on the field work and collections of the preceding seven years. A bulletin giving the archæologic localities within the mound area, together with a series of accompanying maps, was completed for publication. It will form a closely printed octavo of about two hundred and fifty pages. His report, which requires much comparison and reference as well as study of the works explored and objects obtained, is progressing as rapidly as is consistent with proper care and due regard for details, and will be completed and presented for publication during the next fiscal year.

Mr. Henry L. Reynolds, on his return from field duty, assisted Professor Thomas in the preparation of that part of his report and bulletin relating to those archæologic districts the works of which he had visited. He then resumed the preparation of his paper on the aboriginal use of metal. In May he made an examination of the metal specimens in the private and public archæological collections of New York City, and in June visited Providence and Boston in search of certain rare historic data relating to the early life and customs of the Indians, both in respect to the use of metal and to other matters. He was engaged in the office upon this work at the close of the fiscal year.

During the year Mr. Victor Mindeleff was engaged upon a report on the architecture of Tusayan and Cibola. This work was interrupted by a short field trip to the ruin of Casa Grande, as mentioned under the head of field work, and was resumed on his return from that trip. The report, together with the data for its illustrations, has been finished for publication. A report was also prepared on the repairs and protection of the ruin of Casa Grande, on the Gila River, in Arizona. This report was accompanied by diagram, plans, and a series of photographs. He also was occupied in an architectural discussion on this ruin, together with one on the ruins on the Rio Salado, excavated by the Hemenway expedition, which were visited by him.

During the first four months of the fiscal year Mr. Cosmos Mindeleff was engaged in revising manuscript and otherwise assisting Mr. Victor Mindeleff in the preparation of a report on Pueblo Architecture, his own portion of the report having been previously finished. The report was handed in for publication in December, 1889. He then commenced the preparation of a series of maps, upon which the location of all known ruins in the ancient Pueblo country will be plotted, in order to show their distribution. The maps were partly done and the plotting of the ruins was commenced. When completed the maps will show the location of all ruins mentioned in literature or known to explorers and will be accompanied by a card catalogue containing a description of each ruin and reference to the literature relating to it, the whole forming a valuable record. It is intended that a résumé of this shall be published.

During the year the work of the modelling room was continued under the direction of Mr. Cosmos Mindeleff, and was confined almost entirely to the enlargement of the "duplicate series," referred to in previous reports. The large model of Peñasco Blanco, one of the Chaco ruins, reported last year as commenced, was completed, cut into sections for convenience of shipment, and boxed. A duplicate of a model of the Pueblo of Tewa, the original of which was made in 1883, was finished and exchanged for the original in the National Museum. The original was condemned and destroyed and another duplicate was made for the duplicate series. A duplicate was also made of a model of Schumepovi, and the original was put in order and added to the series. A duplicate of a model of the Pueblo of Shipaulovi was also finished and added to the same series. The original model of Casa Blanca cliff ruin was withdrawn from the Museum, and a number of duplicate casts were made, one of which was finished and re-deposited in the Museum. Duplicates were also made of models of Great Elephant Mound, Great Etowah Mound, and two others. In the latter half of the fiscal year work was commenced on the duplication of two very large models, one of Walpi and the First Mesa, the other of Mummy Cave cliff ruin. The original models had been very hurriedly made for the New Orleans Exposition, and, being cast in plaster of paris, had suffered considerably in transportation. An attempt was made to cast the models in paper, and in both cases the attempt was very successful. The first duplicate of the Walpi model was completed and deposited in the National Museum, to replace the original which was destroyed. The finished model weighed about 500 pounds, instead of 2,500 pounds, the weight of the original. The model of the Mummy Cave was cast, but was not finished at the close of the year. A second duplicate of Walpi, for the duplicate series, was cast, but not finished, at the close of the year. It will be divided into sections for convenience of shipment. Toward the close of the year work was commenced on two new models which will be used to illustrate a report of Mr. Holmes, upon his work of the Archæology of the District of Columbia.

But one demand was made during the year upon the duplicate series. This was for a number of transparencies to be exhibited as a part of the display of the United States at the Paris Exposition. Sixty of these large photographs on glass were sent and two grand prizes were awarded them. Upon the conclusion of the exposition the transparencies were returned, and some damage suffered in transportation was made good by the United States Commission.

During the year nine models, ranging in size from 2 feet square to 14 by 5 feet, were finished; twelve models, including duplicate casts, were finished but not painted; and four models were commenced and not finished.

Mr. De Lancey W. Gill during the year succeeded Mr. Holmes in the charge of preparing and editing the illustrations for the publications for the Bureau. The following list shows the number of drawings that have been prepared under his supervision for actual publication during the year:

Architectural drawings, drawings of mounds, earthworks, ancient ruins, etc	102
Maps, diagrams, and sections	64
Objects of stone, wood, shell, bone, etc	377
Total	543

These drawings were prepared from field surveys and sketches, from photographs, and from the objects themselves. No field work has been done by Mr. Gill's division during the year although many valuable drawings and photographs were procured in Arizona by Mr. Victor Mindeleff and in the District of Columbia by Mr. W. H. Holmes.

The photographic work remains under the able management of Mr. J. K. Hillers. The following statement shows the amount of work done in the laboratory:

Negatives.		Prints.	
Size.	Number.	Size.	Number.
28 by 34	12	28 by 34	36
20 by 24	6	20 by 24	26
14 by 17	2	14 by 17	6
11 by 14	20	11 by 14	128
8 by 8	90	8 by 10	529
5 by 8	14	5 by 8	66

Photographs were obtained of Indians from sittings as follows:

Tribe.	Number.
Dakota	32
Sac and Fox	5
Otoe	4
Moki	5
Umatilla	5

During the year the Sixth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution was issued. It contains the introductory report of the Director, J. W. Powell, 35 pages, with accompanying papers, as follows: Ancient Art of the Province of Chiriqui, Colombia, by William H. Holmes; a Study of the Textile Art in its relation to the Development of Form and Ornament, by William H. Holmes; Aids to the Study of The Maya Codices, by Prof. Cyrus Thomas; Osage Traditions, by Rev. J. Owen Dorsey; the Central Eskimo, by Dr. Franz Boas. The work forms a royal octavo volume of lviii + 657 pages, including a general index, and is illustrated by 546 figures in the text, 10 plates, and 2 maps in pocket.

Very respectfully,

J. W. POWELL,
Director.

Prof. S. P. LANGLEY,
Secretary of the Smithsonian Institution.

Appendix II.

REPORT OF THE CURATOR OF EXCHANGES FOR THE YEAR ENDING JUNE 30, 1890.

SIR: I have the honor to submit the following report of the operations of the Bureau of International Exchanges for the fiscal year ending June 30, 1890.

This report has been prepared in a form somewhat similar to the reports of previous years, being for the sake of convenience divided into the following headings:

Tabular statement of the transactions of the office and comparison with the work of previous years.

Expense.

Number of correspondents.

International exchange of official documents, etc.

Efficiency of the service.

List of transportation companies.

TRANSACTIONS OF THE BUREAU OF INTERNATIONAL EXCHANGE DURING THE FISCAL YEAR 1889-'90.

	July, 1889.	Aug., 1889.	Sept., 1889.	Oct., 1889.	Nov., 1889.	Dec., 1889.	Jan., 1890.	Feb., 1890.	Mar., 1890.	Apr., 1890.	May., 1890.	June, 1890.
Number of packages received	3, 711	4, 565	8, 049	2, 029	10, 940	3, 895	6, 692	2, 299	13, 745	5, 505	4, 304	17, 338
Weight of packages received	9, 655	13, 289	14, 331	6, 365	29, 409	8, 624	12, 458	10, 480	35, 521	13, 802	13, 909	34, 814
Entries made:												
Foreign	4, 893	2, 887	1, 015	2, 694	5, 549	4, 762	6, 742	3, 730	8, 325	6, 689	4, 612	8, 220
Domestic	1, 208	3, 112	724	1, 016	1, 214	838	2, 126	462	1, 582	1, 882	1, 138	1, 050
Ledger accounts:												
Foreign societies	4, 466	......	......	......	......	......	......	......	......	......	......	5, 131
Domestic societies	1, 355	......	......	......	......	......	......	......	......	......	......	1, 431
Foreign individuals	4, 699	......	......	......	......	......	......	......	......	......	......	6, 340
Domestic individuals	2, 610	......	......	......	......	......	......	......	......	......	......	3, 100
Domestic packages sent	1, 193	2, 036	573	605	1, 084	686	1, 760	287	1, 946	1, 611	635	800
Invoices written	871	528	451	427	1, 443	1, 563	608	2, 921	1, 962	1, 513	1, 006	3, 655
Cases shipped abroad	33	16	61	14	115	46	31	107	125	40	66	219
Acknowledgments recorded:												
Foreign	810	793	428	760	860	210	222	686	799	477	1, 453	900
Domestic	226	1, 031	423	600	540	......	......	......	......	......	......	*6, 206
Letters received	119	90	84	103	87	91	110	125	174	149	195	182
Letters written	96	41	164	67	171	82	108	192	217	102	118	267

Recapitulation.

	Total.	Increase over 1888–'89.		Total.	Increase over 1888–'89.
Number of packages received	82, 572	6, 606	Domestic individuals	3, 100	490
Weight of packages received	202, 657	22, 729	Domestic packages sent	13, 216	†4, 002
Entries made:			Invoices written	16, 948	2, 850
Foreign	60, 118	13, 976	Cases shipped abroad	873	180
Domestic	16, 352	†1, 924	Acknowledgments recorded:		
Ledger accounts:			Foreign	8, 398	956
Foreign societies	5, 131	665	Domestic	9, 026	2, 144
Domestic societies	1, 431	76	Letters received	1, 509	295
Foreign individuals	6, 340	1, 641	Letters written	1, 625	†425

* From December to June inclusive. † Decrease.

An idea of the growth of the service since 1886 is conveyed by the annexed summary:

Comparative statement.

Packages.	1886–'87.	1887–'88.	1888–'89.	1889–'90.
Received	52, 218	75, 107	75, 966	82, 572
Shipped:				
Domestic	10, 294	12, 301	17, 218	13, 216
Foreign	41, 424	62, 306	58, 035	69, 036

EXPENSE.

The expenses of the Exchange Bureau are met in part by a direct appropriation made by Congress in the following terms:

"For expenses of the system of international exchanges between the United States and foreign countries, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees, fifteen thousand dollars."

This is supplemented by appropriations to several Government Bureaus by which they are enabled to pay a portion of the cost of the exchange of their documents at a rate of 5 cents per pound weight as established by the Board of Regents. Smaller sums have been received from State institutions desiring to make use of the service, and the deficiency is paid from the Smithsonian fund.*

The receipts and disbursements by the accounting officer of the Smithsonian Institution on account of the international exchanges, as shown in his statement for the fiscal year, dated July 1, 1890, were as follows:

Receipts.

Direct appropriation by Congress		$15, 000. 00
Repayment to Smithsonian Institution:		
United States Government Departments	1, 771. 53	
Societies and other sources	18. 45	
		1, 789. 98

*The actual cost of the exchanges from July 1, 1889, to June 30, 1890, compiled from the accounting officer's books and including the receipts and disbursements for the fiscal year, entered up to September 24, 1890, was $17,407.30.

Fifteen thousand dollars of this sum ($17,407.30) were appropriated by Congress directly to the Smithsonian Institution, $2,009.34 were repaid to the Institution by Government Bureaus, $28.40 by State institutions and the deficiency, $369.56, was met by the Smithsonian fund.

Disbursements.

	From Congressional appropriations.	Repayments.
Salaries and compensation of employés	$11,638.49	$142.00
Salaries of foreign agents	1,500.00	....
Freight	998.67	1,113.06
Packing boxes	443.41	222.50
Printing, stationery, postage, etc	407.44	316.53
	14,988.01	1,794.09

Bills for the transportation of exchanges have been rendered to all Government Bureaus receiving or sending publications during the year, except in a few instances where the amount was trifling. The total received from such sources was $1,771.53, as mentioned above.

It may not be superfluous to repeat the statement made in previous years, that this method of meeting the expenses of the Exchange Bureau is extremely unsatisfactory both to the Smithsonian Institution and to the Government Bureaus that have occasion to make use of the service, and I again recommend that a sufficient appropriation be procured to cover the entire cost of the exchanges, thereby enabling it to understand at a glance the exact amount appropriated for such purposes. At present the appropriation is distributed through all the principal appropriation bills of the Government.

In order to effect the desired change, that is, to collect in a single item the entire appropriation for international exchanges and at the same time to make allowances for the payment of ocean freight, the sum of $27,500 was asked for, for the fiscal year 1889–'90 based upon the detailed statements submitted in my last report. The amount finally appropriated was $15,000, the same as that for the year preceding.

CORRESPONDENTS.

The number of correspondents now upon our books is 16,002, divided into societies and institutions, individuals, foreign and domestic, as follows:

	Foreign.	Domestic.
Societies and institutions	5,131	1,431
Individuals	6,340	3,100
Total	11,471	4,531

A comparison with similar figures for last year shows a net increase of 2,872.

INTERNATIONAL EXCHANGE OF OFFICIAL DOCUMENTS.

The exchange of official documents between the Government of the United States and that of foreign countries has been carried on through the intermediary of the Smithsonian Institution, though this exchange has only been placed upon a definite diplomatic footing since January 15, 1889, the date upon which the convention signed at Brussels on March 15, 1886, was proclaimed by the President of the United States. This convention, the text of which was given in full in Dr. Kidder's report on exchanges for the year 1887–'88, provided that there should be established in each of

the contracting countries a bureau for the special transmission of the publications of its Government, the transactions of its learned societies, etc., to foreign governments and individuals, and for the receipt from the similar bureaus of other countries of the publications of their government and scientific and literary societies. This involves, as will be seen but little or no modification of the present long-established Smithsonian Institution exchange system, and it is hoped that the official recognition of the value of such a service by so many governments will result in extending the scheme that has been in operation here for the past forty years, the expense of which has been borne largely by the funds of James Smithson.

In accordance with a provision made in the Brussels Convention the Governments of the Argentine Republic and of Paraguay have signified their adhesion to the convention, the former on September 3, 1889, and the latter on December 10, 1889. The countries therefore included in the international agreement are:—The United States of America, the Argentine Republic, Belgium, Brazil, Italy, Paraguay, Portugal, Servia, Spain, Switzerland, Uruguay.

While neither England nor Germany appear in the above list, both of these countries have addressed inquiries to this institution through diplomatic channels with regard to exchanges with our Government, and it is most gratifying to report that the British Government, through Her Majesty's Stationery Office, has presented to the Government of the United States, for deposit in the Library of Congress, an important collection of the publications of the parliamentary and executive offices from the years 1882 to 1889, constituting a most valuable series of documents and forming a partial return for the series of publications issued by our own Government since 1868 and sent regularly to the British Museum. Moreover we have the assurance that this valuable series will be continued in annual shipments.

The Government of Germany has also expressed its appreciation of the international exchange service in such a way as to lead us to expect that it will in due time make fitting acknowledgment of the series of United States Government publications presented to the Royal Public Library, and to the Library of the Imperial German Parliament at Berlin.

A second convention made at Brussels, and also proclaimed by the President on the 15th of January, 1889, provided for the immediate exchange of Parliamentary journals and the like, but it had not at the close of the fiscal year been set in satisfactory operation. An effort was made by a letter addressed to the Department of State on December 12, 1889, to carry out the stipulations of this treaty as far as the United States Government was concerned, and upon the recommendation of the Secretary of State a joint resolution appropriating $2,000 for the purpose was passed by the Senate on January 22, 1890, but it has not yet been acted upon by the House.

EFFICIENCY OF THE SERVICE.

An inspection of the tables presented at the beginning of this report bears sufficient evidence that the Bureau has not decreased in efficiency during the past year, especially when it is considered that the increased number (6,606) of packages was handled and accounted for with a decrease in the clerical force during eleven months. At the close of the year there were but 321 packages on hand and the record work was tolerably well up to date.

The distribution to foreign countries was made in 873 cases, representing 385 transmissions, as follows:

	Cases.
Argentine Republic	12
Austria*	4
Baden*	4
Bavaria*	4
Belgium	12
Bolivia	1
Canada†	8
China	3
Chili	8
Colombia	6
Costa Rica	2
Cuba	3
Denmark	9
Dutch Guiana	1
Ecuador	2
Egypt	2
France	23
Germany	25
Great Britain	30
Greece	6
Guatemala	2
Hayti	6
Hungary*	4
India	5
Italy	18
Japan	10
Liberia	2
Mexico†	7
Netherlands	12
New South Wales	9
New Zealand	8
Nicaragua	2
Norway	8
Paraguay	1
Peru	6
Polynesia	2
Portugal	7
Prussia*	4
Queensland	9
Russia	18
San Salvador	1
Saxony*	4
South Australia	8
Spain	8
Sweden	9
Switzerland	15
Tasmania	6
Turkey	6
Uruguay	2
Venezuela	7
Victoria	8
West Indies	(‡)
Würtemberg*	4

* Miscellaneous exchanges included in transmissions to Germany.

† In addition to a large amount sent by mail.

‡ By mail.

The entire number of publications sent abroad during the year under the provisions of the act of Congress of March 2, 1867, has been 27,300, and there have been received in return but 1,820 packages or volumes. The United States Government Departments have forwarded to their correspondents abroad through the Bureau 16,496 packages or volumes, and have received in return 8,886. The total, then, of the exchanges for the enrichment of the Government libraries has been 10,706 packages received and 43,796 packages sent abroad, a total of 54,502 packages, or 66 per cent. of the total number of packages handled.

Statement of Governmental exchanges distributed during the year 1889–'90.

	Packages.			Packages.	
	Received for.	Sent by.		Received for.	Sent by.
American Ephemeris	1	....	Library of Congress	1, 820	....
Army Medical Museum	2	....	Light-House Board	2	
Botanical Gardens	1	1	Marine Hospital	....	[illegible]
Bureau of Education	68	....	Nautical Almanac	18	[illegible]
Bureau of Engineers, U. S. Army	43	272	National Academy	276	1, 55
Bureau of Ethnology	9	2, 669	National Board of Health	2	....
Bureau of the Mint	2	....	National Museum	106	2, 20
Bureau of Statistics	16	2	Navy Department	7	
Census Bureau	9	....	Naval Observatory	113	81
Coast Survey	72	18	Office of Indian Affairs	3	....
Commissioners of the District of Columbia	1	....	Ordnance Bureau, U. S. Army	5	
Comptroller of the Currency	1	....	Patent Office	212	49
Department of Agriculture	95	896	Smithsonian Institution	1, 795	3, 65
Department of the Interior	23	102	Smithsonian Institution (by mail)	5, 050	.....
Department of Labor	6	99	Smithsonian Institution (returned to Document Division)	22	....
Department of State	15	....	Signal Office	74	[illegible]
Entomological Commission	8	....	Surgeon-General	136	[illegible]
Exchange Bureau	8	....	Treasury Department	11	
Fish Commission	91	414	War Department	17	1[illegible]
General Land Office	4	....		10, 695	16, 49
Geological Survey	413	2, 685	Public Printer		27, 30
House of Representatives	1	....		10, 695	43, 79
Hydrographic Office	48	....			

Total Governent exchanges	54, 43
Miscellaneous exchanges	28, 08
Total exchanges	82, 57

Of the 82,572 parcels received by the Exchange Bureau, 69,356 were for foreign and 13,216 for domestic distribution.

While it is thus shown that more work has been done and with less force than in the preceding years, I strongly recommend that a slight increase in the office force be made in order that it may be possible to handle more rapidly the large and constantly increasing amount of exchange material. An additional assistant in the shipping room will, I am confident, prevent any reasonable complaints of delays in the office proper. Delays that occur by reason of slow ocean transportation will be obviated when sufficient appropriation is made to pay for freight; the delays that occur in the foreign exchange bureaus or agents, except those in the pay of the Smithsonian Institution, lie of course beyond the control of the Institution.

The foreign agents of the Institution, Dr. Felix Flügel, Leipzig, and Messrs. William Wesley & Son, London, have given the same careful attention to the interests of the Institution as in former years and are entitled, as well as the immediate employés of the Bureau, to my warmest thanks. Grateful acknowledgments are also due to the following transportation companies and firms for their continued liberality in granting free freight or otherwise assisting in the transmission of exchange parcels and boxes, while to others we are indebted for reduced rates in consideration of the disinterested services of the Institution in the diffusion of knowledge among men.

Allan Steamship Company (A. Schumacher & Co., agents), Baltimore.
d'Almeirim, Baron, Royal Portuguese consul-general, New York.
American Board of Commissioners for Foreign Missions, Boston.
American Colonization Society, Washington, District of Columbia.
Anchor Steamship Line (Henderson & Bro., agents), New York.
Atlas Steamship Company (Pim, Forwood & Co.), New York.
Bailey, H. B., & Co., New York.
Barber & Co., New York.
Bixby, Thomas E., & Co., Boston.
Borland, B. R., New York.
Bors, C., consul-general for Sweden and Norway, New York.
Botassi, D. W., consul-general for Greece, New York.
Boulton, Bliss & Dallett, New York.
Calderon, Climaco, consul-general for Colombia, New York.
Caldo, A. G., consul-general for Argentine Republic, New York.
Cameron, R. W. & Co., New York.
Baltazzi, X, consul-general for Turkey, New York.
Compagnie, Générale Transatlantique (A. Forget, agent), New York.
Cunard Royal Mail Steam-ship Company (Vernon H. Brown & Co., agent), New York.
Dennison, Thomas, New York.
Espriella, Justo R. de la, consul-general for Chili, New York.
Florio Rubattino Line—Navigazione Generale Italiano (Phelps Bros. & Co.), New York.
Grace, W. R., & Co., New York.
Hamburg American Packet Company (R. J. Cortis, manager), New York.
Hensel, Bruckmann & Lorbacher, New York.
Inman Steam-ship Company (Henderson & Bro., agents), New York.
Mantez, José, consul-general for Uruguay, New York.
Merchant, S. L. Co., New York.
Muñoz y Espriella, New York.
Murray, Ferris & Co., New York.
Navarro, J. N., consul-general for Mexico, New York.
Netherlands American Steam Navigation Company (W. H. Vanden Toorn, agent) New York.
New York and Brazil Mail Steam-ship Company, New York.
New York and Mexico Steam-ship Company, New York.
North German Lloyd (agents: Oelrichs & Co., New York; A. Schumacher & Co., Baltimore).
Obarrio, Melchor, consul-general for Bolivia, New York.
Pacific Mail Steam-ship Company (H. J. Bullay, superintendent), New York.
Panama Railroad Company, New York.
Pioneer Line (R. W. Cameron & Co.), New York.
Perry, Ed., & Co., New York.
Pomares, Mariano, consul-general for Salvador, New York.
Red Star Line (Peter Wright & Sons, agents, New York and Philadelphia).
Royal Danish consul, New York.
Royal Spanish consul, New York.
Ruiz, Domingo L., consul-general for Ecuador, New York.
Stewart, Alexander, consul-general for Paraguay, Washington, District of Columbia.
Toriello, Enrique, consul-general for Guatemala, New York.
Vatable, H. A., & Co., New York.
White Cross Line of Antwerp (Funch, Edye & Co.), New York.
Wilson & Asmus, New York.

Upon January 1, 1890, a new system of recording the correspondence was adopted having been submitted to a preliminary trial of several months to test its applicability to the special wants of the office. Every letter or invoice received is assigned a current number, and is entered at once in a book for the purpose, a card index facilitating reference to the letters filed chronologically. All out-going letters are entered in a similar book.

The collection of scientific and other directories, Government year-books, and lists of members of learned societies, has received a number of valuable additions, and it is hoped that with increased funds at command and a diminution of more pressing needs this collection will be made an important feature of the Exchange Bureau.

Perhaps the most serviceable information to those having occasion to make use of the service is that contained in Exhibit A, appended, showing the number of shipments each month to the various countries with which we are in correspondence.

Very respectfully yours,

WILLIAM C. WINLOCK,
Curator of Exchanges.

Mr. S. P. LANGLEY,
Secretary of the Smithsonian Institution.

EXHIBIT A.

Transmission of exchanges to foreign countries.

Countries.	Date of transmission, etc.
Argentine Republic	September 13, 1889; January 2, February 15, June 23, 1890.
Austria-Hungary	Included in transmission to Germany.
Belgium	August 29, November 23, 25, 1889; February 19, March 6, May 7, June 3, 16, 1890.
Bolivia	February 17, 1890.
British Colonies	Included in transmission for England.
China	December 30, 1889; February 21, May 10, 1890.
Chili	September 13, 1889; January 2, February 17, June 23, 1890.
Colombia	February 17, June 23, 1890.
Costa Rica	February 14, June 23, 1890.
Cuba	October 16, 1889; February 20, June 23, 1890.
Denmark	September 14, November 25, 1889; February 20, April 21, June 16, 1890.
Dutch Guiana	February 17, 1890.
East India	February 21, June 21, 1890. Also included in transmissions to England.
Ecuador	February 17, June 23, 1890.
Egypt	Februray 21, June 24, 1890.
France and Colonies	July 6, August 12, September 7, October 22, November 14, 23, December 7, 1889; January 5, 27, February 4, 11, 12, March 4, 21, 29, May 1, June 6, 19, 24, 1890.
Germany	July 3, 20, 25, August 14, September 3, October 12, November 7, 11, 23, December 13, 1889; January 4, 27, February 3, 12, March 6, 23, 29, April 25, May 16, 29, June 5, 18, 1890.
Great Britain, etc	July 10, August 8, September 11, October 18, November 8, 16, 23, December 7, 28, 1889; January 2, 28, February 7, 11, 13, 20, 21, March 8, 22, April 2, 30, May 5, 13, June 4, 14, 24, 30, 1890.
Greece	February 20, June 16, 1890.
Guatemala	February 14, June 23, 1890.
Hayti	February 20, June 23, 1890.
Italy	July 15, August 30, October 26, November 15, 23, December 27, 1889; January 27, February 14, 20, March 6, 31, April 5, June 6, 30, 1890.

Transmission of exchanges to foreign countries—Continued.

Countries.	Date of transmission, etc.
Japan	September 19, December 30, 1889; February 21, May 10, 23, June 21, 1890.
Liberia	February 11, June 24, 1890.
Mexico	September 24, 1889; February 15, March 20, 1890. The majority of Mexican exchanges are sent by registered mail.
New South Wales	September 27, December 23, 1889; February 21, May 13, June 21, 1890.
Netherlands and colonies	September 12, November 15, 23, 1889; January 31, February 15, March 6, June 3, 13, 1890.
New Zealand	December 23, 1889; February 24, May 13, June 21, 1890.
Nicaragua	February 14, June 23, 1890.
Norway	October 25, November 25, 1889; April 24, June 16, 1890.
Paraguay	February 17, 1890.
Peru	February 17, June 23, 1890.
Polynesia	February 24, June 21, 1890.
Portugal	December 31, 1889; February 24, June 14, 1890.
Queensland	November 16, December 23, 1889; January 18, May 13, June 21, 1890.
Roumania	Included in Germany.
Russia	July 11, 25, September 20, November 15, 23, December 30, 1889; February 10, 13, March 6, 31, April 7, May 14, June 6, 20, 1890.
Servia	Included in Germany.
San Salvador	February 17, 1890.
South Australia	December 26, 1889; February 24, May 13, June 21, 1890.
Spain	December 31, 1889; February 24, May 24, June 16, 1890.
Sweden	September 12, 1889; January 3, April 3, May 31, June 20, 1890.
Switzerland	July 12, 29, September 19, November 23, December 27, 1889; February 14, 24, March 6, May 14, June 6, 20, 1890.
Tasmania	February 21, June 21, 1890.
Turkey	February 24, June 16, 1890.
Uruguay	February 17, June 23, 1890.
Venezuela	September 13, 1889; February 17, June 23, 1890.
Victoria	September 27, 1889; February 21, May 13, June 21, 1890.

In addition to the above, shipments of United States Congressional publications were made on September 7, November 30, 1889; March 17, June 28, 1890, to the governments of the following-named countries:

Argentine Republic.
Austria.
Baden.
Bavaria.
Belgium.
Buenos Ayres.
Brazil.
Canada (Ottawa).
Canada (Toronto).
Chili.
Colombia.
Denmark.
France.
Germany.
England.
Greece.
Hayti.
Hungary.
India.
Italy.
Japan.
Mexico.
Netherlands.
New South Wales.
New Zealand.
Norway.
Peru.
Portugal.
Prussia.
Queensland.
Russia.
Saxony.
South Australia.
Spain.
Sweden.
Switzerland.
Tasmania.
Turkey.
Venezuela.
Victoria.
Wurtemberg.

APPENDIX III.

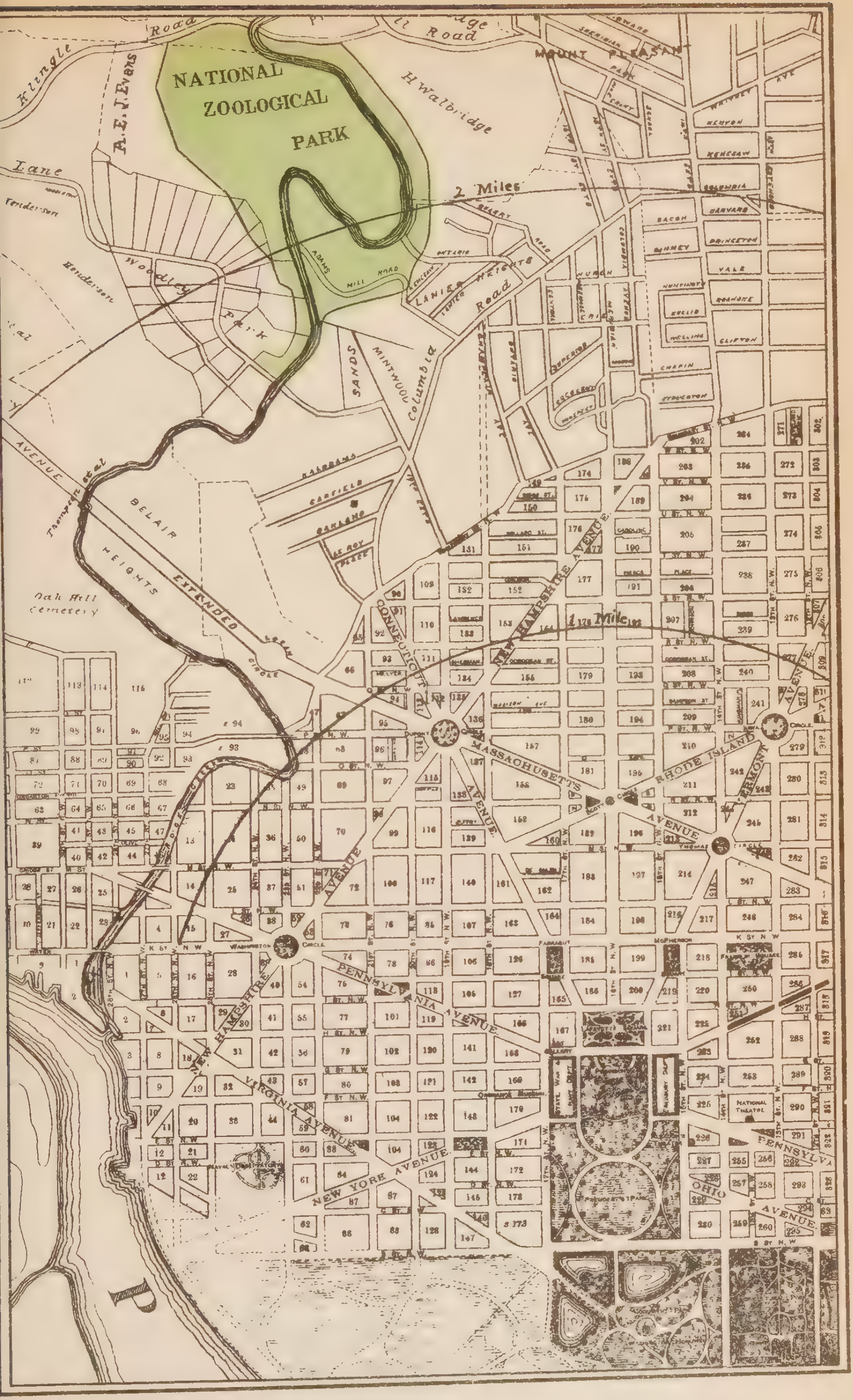
REPORT OF THE ACTING MANAGER OF THE NATIONAL ZOOLOGICAL PARK.

On June 15, 1890, the animals exhibited for some years past at the National Museum, and forming the nucleus of a collection for the National Zoological Park, were turned over to the acting manager of the park.

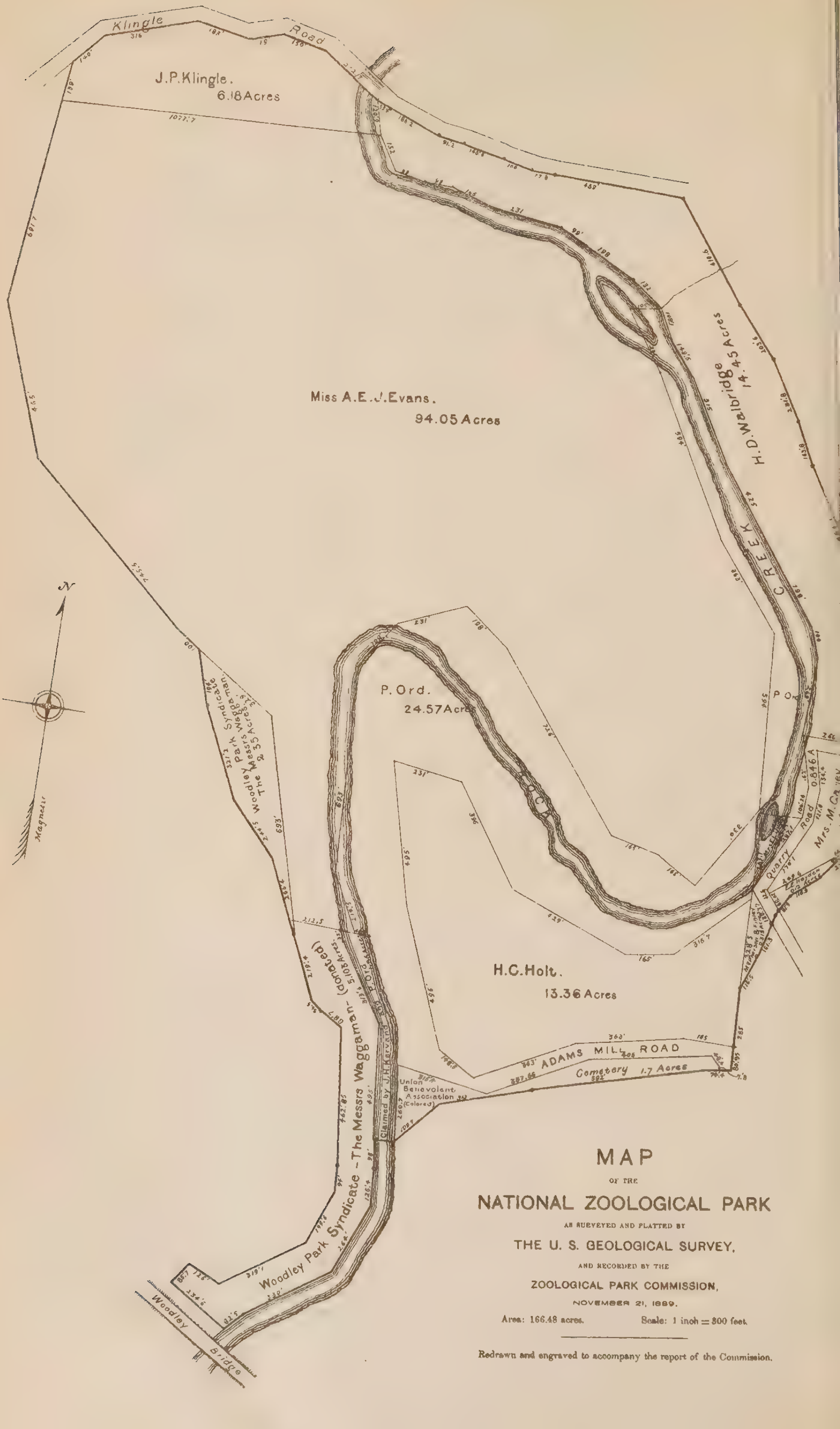
They are shown in detail in the following list:

Name.	Specimen.
MAMMALS.	
Opossum (*Didelphys virginiana*, Kerr)	8
Peccary (*Dicotyles tajaçu*, L.)	1
Mule Deer (*Cariacus macrotis*, Say)	1
Columbian Blacktailed Deer (*Cariacus columbianus*, Rich.)	1
Virginia Deer (*Cariacus virginianus*, Bodd.)	1
American Elk or Wapiti (*Cervus canadensis*, Erxl.)	4
American Bison (*Bison americanus*, Gmel.)	6
Rocky Mountain Sheep (*Ovis montana*, Cuv.)	1
Angora Goat (*Capra hircus angorensis*)	6
Woodchuck (*Arctomys monax*, L.)	5
Prairie Dog (*Cynomys ludovicianus*, Ord.)	3
Striped Gopher (*Spermophilus tredecimlineatus*, Mitchill)	11
Red Squirrel (*Sciurus hudsonius*, Pallas)	2
Gray Squirrel (*Sciurus carolinensis carolinensis*, Gmel.)	1
Flying Squirrel (*Sciuropterus volucella volucella*, Pallas.)	2
Canada Porcupine (*Erethizon dorsatus dorsatus*, L.)	3
Guinea Pig (*Cavia aperea*)	4
Black Bear (*Ursus americanus*, Pallas)	3
"Cinnamon" Bear (*Ursus americanus*	
Silver-tip Grizzly Bear (*Ursus horribilis*,	1
Ord.)	1
Raccoon (*Procyon lotor*, L.)	5
Ferret (*Putorius furo*, L.)	2
Gray Fox (*Urocyon virginianus*, Schreber)	4
Swift Fox (*Vulpes velox*, Say)	2
Red Fox (*Vulpes fulvus fulvus*, Desmarest)	9
Panther (*Felis concolor*, L.)	1
White-throated Capuchin Monkey (*Cebus hypoleucus*, Humboldt)	1
Brown Capuchin Monkey (*Cebus fatuellus*)	1
Grivet Monkey (*Cercopithecus callitrichus*)	1
Macaque Monkey (*Macacus cynomolgus*)	1

Name.	Specimen.
BIRDS.	
Golden Eagle (*Aquila chrysaëtus*, L)	1
White-headed Eagle (*Haliæetus leucocephalus*, L.)	2
Cooper's Hawk (*Accipiter cooperi*, Bonap.)	1
Red-shouldered Hawk (*Buteo lineatus*, Gmel.)	3
Sparrow Hawk (*Falco sparverius*, L.)	2
Great Horned Owl (*Bubo virginianus*, Gmel.)	4
Barn Owl (*Strix pratincola*, Bonap.)	11
Barred Owl (*Syrnium nebulosum*, Forst.)	1
Red and blue Macaw (*Ara chloroptera*)	1
Red and blue and yellow Macaw (*Ara macao*)	1
Yellow and blue Macaw (*Ara araraunea*)	2
Sulphur-crested Cockatoo (*Cacatua galerita*)	3
Clarke's Nut-cracker (*Picicorvus columbianus*, Wils.)	6
Long-crested Jay (*Cyanocitta stelleri macrolopha*, Baird)	2
Carolina Paroquet (*Conurus carolinensis*, L.)	1
Houdan Chickens (*Gallus bankiva*)	2
Frizzled Chickens (*Gallus bankiva*)	2
Bronze Turkeys (*Meleagris gallopavo*, L.)	2
White Turkeys (*Meleagris gallopavo*, L.)	3
Canada Goose (*Branta canadensis*, L.)	2
Night Heron (*Nycticorax nycticorax nævius*, Bodd.)	3
Turtle Dove (*Zenaidura macroura*, L.)	6
REPTILES.	
Black Snake (*Bascanion constrictor*, L.)	1
Hog-nosed Adder (*Heterodon platyrhinus*, Latreille)	4
Elephant Tortoise (*Testudo elephantopus*)	2
Galapagos Tortoise (*Testudo nigrita*)	1
Alligator (*Alligator mississippiensis*, Daudin)	17
Bull-frog (*Rana catesbiana*, Shaw)	1
Water Turtles	
Total	185



Map showing the Location of the National Zoological Park.



MAP

OF THE

NATIONAL ZOOLOGICAL PARK

AS SURVEYED AND PLATTED BY

THE U. S. GEOLOGICAL SURVEY,

AND RECORDED BY THE

ZOOLOGICAL PARK COMMISSION,

NOVEMBER 21, 1889.

Area: 166.48 acres. Scale: 1 inch = 800 feet.

Redrawn and engraved to accompany the report of the Commission.

It has hitherto been impossible to give suitable housing to these animals, most of which are gifts to the Government, and many of them are kept in a long, low shed, imperfectly lighted and heated, wherein animals accustomed to the most diverse climates are of necessity indiscriminately placed, the common Virginia opossum receiving the same heat and treatment that serves for the parrots and cockatoos. In an annex to this shed the monkeys are placed, and it has been possible to give them somewhat more suitable protection. The larger animals are confined either in separate out-door cages or in shelter-barns and pens, but these constructions are unsuitable and insufficient even for the small number of such animals kept. Happily, this condition is not a permanent one, as Congress has provided for the care and maintenance of the collection in the National Zoological Park.

No zoological collection has ever been placed in a site so satisfactory. It is ample in extent, being about four times larger than any zoological garden in this country and from ten to fifteen times the size of most of the gardens of Europe. It is within a short distance of the city, being but little over one-half mile from its limits (see map No. 1) easily accessible by excellent roads; yet it has all the quiet and seclusion of a remote country district. Within its bounds every variety of slope exposure is found, from the north sides of hills covered with dense growth, suitable for animals requiring coolness and shade, to the sunny southern aspects for tropical and subtropical species. The natural variety of surface is also great. Rocks form natural cliffs where wild sheep and goats can jump and climb ; densely wooded portions form an excellent cover for shy animals, and a large open field along the creek affords an opportunity for excellent grazing grounds. In the creek itself aquatic animals and birds may be suitably reared.

That the picturesqueness of the region is notable is shown by the names given to different parts of it in the grants and early deeds of the eighteenth century. There it is found that a considerable part of the park was known as "Pretty Prospect," also as "The Rock of Dunbarton," while other parts are from the tracts of "Mount Pleasant" and "Pleasant Plains." The actual owners from whom the site was immediately derived are shown on the accompanying map No. 2. A portion of it was once owned by John Quincy Adams, who built upon the creek the "Columbia Mill," for many years past known as "Adams's Mill." Fragmentary ruins of some of the mill buildings still remain.

The only habitable building found within the limits defining the park was that known as the "Holt House." This mansion is one of the few remaining in the District dating back to near the beginning of the century, it having been built in 1805. Though in a very dilapidated condition, it is thought desirable to repair it, preserving as far as possible its characteristic features, and it will be used for the offices of the Park.

The original forest covering this land was doubtless mainly oaks, hickories, and tulip trees. A portion of this was cleared away, and the land was probably cultivated for many years. Being then allowed to lie fallow, there sprang up upon it a thick second growth of scrub pines and cedars. A large variety of trees of natural growth is found. A list of those already noted that may be classed as indigenous follows:

Popular name.	Scientific name.
Tulip tree	*Liriodendron Tulipifera*, L.
American holly	*Ilex opaca*, Ait.
White or silver maple	*Acer dasycarpum*, Ehrh.
Red or swamp maple	*Acer rubrum*, L.
Box elder	*Negundo aceroides*, Moench.
Common locust	*Robinia Pseudacacia*, L.
Honey locust	*Gleditschia triacanthos*, L.
Red bud or Judas tree	*Cercis Canadensis*, L.

Popular name.	Scientific name.
Wild black cherry	*Prunus serotina*, Ehrh..
Witch-hazel	*Hamamelis Virginiana*, L.
Flowering dogwood	*Cornus Florida*, L.
Black gum	*Nyssa sylvatica*, Marsh.
Persimmon	*Diospyros Virginiana*, L.
Red ash	*Fraxinus pubescens*, Lam.
Sassafras	*Sassafras officinale*, Nees.
Slippery elm	*Ulmus fulva*, Michx.
American or white elm	*Ulmus Americana*, L.
Blackberry	*Celtis occidentalis*, L.
Red mulberry	*Morus rubra*, L.
Buttonwood or plane tree	*Platanus occidentalis*, L.
White heart hickory	*Carya tomentosa*, Nutt.
Pignut hickory	*Carya porcina*, Nutt.
Swamp hickory	*Carya amara*, Nutt.
Black walnut	*Juglans nigra*, L.
Butternut	*Juglans cinerea*, L.
River or red birch	*Betula nigra*, L.
Hornbeam or water beech	*Carpinus Caroliniana*, Walt.
Hop hornbeam	*Ostrya Virginica*, Willd.
White oak	*Quercus alba*, L.
Post oak	*Quercus stellata*, Wang.
Chestnut oak	*Quercus prinus*, L.
Red oak	*Quercus rubra*, L.
Scarlet oak	*Quercus coccinea*, Wang.
Yellow barked or black oak	*Quercus coccinea*, Wang, var. *tinctoria*, Gray.
Pin or swamp Spanish oak	*Quercus palustris*, Du Roi.
Spanish oak	*Quercus falcata*, Michx.
Black jack or barren oak	*Quercus nigra*, L.
Willow oak	*Quercus Phellos*, L.
Chestnut	*Castanea sativa*, Mill, var. *Americana*, Gray.
American beech	*Fagus ferruginea*, Ait.
Black willow	*Salix nigra*, Marshall.
Red cedar	*Juniperus Virginiana*, L.
Pitch pine	*Pinus rigida*, Miller.
Jersey or scrub pine	*Pinus inops*, Ait.
Yellow pine	*Pinus mitis*, Michx.
White pine	*Pinus strobus*, L.

ORNITHOLOGY OF THE ZOOLOGICAL PARK.

This region has long been known to be, because of its seclusion and natural a vantages, one of the favorite nesting grounds for the birds that visit the District Columbia. At my request Mr. H. W. Henshaw, a well-known authority in orn thology, has made the special report which follows:

"For many reasons the situation of the site of the National Zoological Park is se to be a wise one, and from no point of view do its advantages appear greater than a haunt of our native birds. A section which has long been known to be the chos home of birds and animals in a state of nature would seem to be a peculiarly fitti abode for them in a state of captivity. It is certain that neither within the Distri nor in the region immediately about it is there a spot which is resorted to by su numbers of birds, nor one where the rarer migratory species can so certainly be foun The park region has long been familiar to every bird collector who has ever ma Washington his headquarters, and probably no area of equal size has furnished

iany specimens to the collection of the National Museum and of private collectors s this.

"To appreciate its advantages as a haunt in which to study the habits of our irds one must visit it in early morning about the middle of May. At this time housands of birds are eagerly winging their way to their northern homes, and the ittle groves of pines and the outlying deciduous thickets are filled with hundreds of varblers, flycatchers, and sparrows. Among others one may be pretty sure to find, mid throngs of commoner species, numbers of Bay-breasted and Blackburnian Varblers. Should the visitor carefully scan the low thickets a Mourning or Conecticut Warbler, rare birds indeed in this latitude, may, perchance, reward his earch.

"The Worm-eating and Kentucky Warblers are always present about that date, hough in small numbers. So, too, are the Yellow-bellied and Least Flycatchers; while he Traills Flycatcher is an occasional visitant. There is a thicket on the west bank of the creek which Lincoln's Finch, long unnoticed in the district, visits each spring, nd I have seen seven or eight of a morning. The Scarlet Tanager, whose bright olors arrest the eye of even the most careless, finds here a favorite resort, and the Rose-breasted Grosbeak, always a prize to the collector, is a regular and common vistor to the tall oaks that cover the eastern slopes. The northeast corner of the park s the only spot known to me where the song of the Summer Tanager may be heard vith anything like certainty, for it is one of our rarest summer visitors. Not so the Olive-backed Thrushes. Several of the five species are common elsewhere, but nowhere do they all occur so abundantly as here, even the Gray-cheeked being numerous. The above are but a few of many species that throng the tree-tops and brushiles at this time of year.

"To explain just why this spot of all others in the District should be the favorite esort for our birds would be difficult. Rock Creek is elsewhere as well wooded as it s here. Elsewhere its banks furnish far more picturesque places, and if we can uppose that birds are influenced in their choice of a resort by the æsthetic sense, vhy are not such places equally favored with their presence?

"I am inclined to believe that the answer is to be found in the somewhat prosaic eason that the gentle slopes of the creek at this point invite the early sunshine, and hat the succession of woods, thickets, and open spots favor the presence of insects nd seed-producing plants. In other words, that here the birds find the exact kind of shelter they require and food in abundance.

"A list of the birds that are known to have nested within the limits of the park, mall though this area is, would include almost all the land birds credited to the District. A catalogue of the birds of the District was prepared by Doctors Coues nd Prentiss several years since (1883), and published by the National Museum under he title of 'Avifauna-Columbiana.'

"As, however, having a more intimate relation to the Park, I subjoin a list of the irds which are known to have nested within the Park area within recent years. Many of them, it is to be hoped, will refuse to recognize as valid the exclusive itle of possession conferred by Congress, and will continue to occupy their old iomes as theirs by squatters' rights. Others doubtless, let us hope a small minority, will prefer to yield their ancient titles and move to more secluded spots in the adoining territory.

"But ninety-one species of land birds are known to breed within the limits of the District, and the following list shows that of this number sixty-one species, or nore than 76 per cent., breed regularly or occasionally within the Park. The uperior advantages it offers to bird life will therefore be readily appreciated."

"*List of birds nesting within the National Zoological Park.*

Popular name.	Scientific name.	Popular name.	Scientific name.
Woodcock	*Philohela minor.*	Scarlet tanager	*Piranga erythomelas.*
Bob White	*Colinus virginianus.*	Summer tanager	*Piranga rubra.*
Turtle dove	*Zenaidura macroura.*	Rough-winged swallow.	*Stelgidopteryx serripennis.*
Broad-winged hawk	*Buteo latissimus.*	Cedar waxwing	*Ampelis cedrorum.*
Screech owl	*Megascops asio.*	Red-eyed vireo	*Vireo olivaceous.*
Yellow-billed cuckoo	*Coccyzus americanus.*	Warbling vireo	*Vireo gilvus.*
Black-billed cuckoo	*Coccyzus erythrophthalmus.*	Yellow-throated vireo	*Vireo flavifrons.*
Kingfisher	*Ceryle alcyon.*	White-eyed vireo	*Vireo noveboracensis.*
Downy woodpecker	*Dryobates pubescens.*	Black and white warbler.	*Mniotilta varia.*
Red-headed woodpecker	*Melanerpes erythrocephalus.*	Worm-eating warbler	*Helmitherus vermivorus.*
Flicker	*Colaptes auratus.*	Yellow warbler	*Dendroica æstiva.*
Ruby-throated humming-bird.	*Trochilus colubris.*	Prairie warbler	*Dendroica discolor.*
King bird	*Tyrannus tyrannus.*	Oven-bird	*Seiurus aurocapillus.*
Crested flycatcher	*Myiarchus crinitus.*	Louisiana water-thrush.	*Seiurus motacilla.*
Phœbe	*Sayornis phœbe.*	Kentucky warbler	*Geothlypis formosa.*
Wood pewee	*Contopus virens.*	Maryland yellow-throat.	*Geothlypis trichas-*
Acadian flycatcher	*Empidonax acadicus.*	Yellow-breasted chat	*Icteria virens.*
American crow	*Corvus americanus.*	American redstart	*Setophaga ruticilla.*
Fish crow	*Corvus ossifragus.*	Mockingbird	*Mimus polyglottus.*
Cow bird	*Molothrus ater.*	Catbird	*Galeoscoptes carolinensis*
Orchard oriole	*Icterus spurius.*	Brown thrasher	*Harporhynchus rufus.*
Baltimore oriole	*Icterus galbula.*	Carolina wren	*Thryothorus ludovicianus*
European house sparrow.	*Passer domesticus.*	House wren	*Troglodytes aëdon.*
American goldfinch	*Spinus tristis.*	White-breasted nuthatch.	*Sitta carolinensis.*
Grasshopper sparrow	*Ammodramus s. passerinus.*	Tufted titmouse	*Parus bicolor.*
Chipping sparrow	*Spizella socialis.*	Carolina chickadee	*Parus carolinensis.*
Field sparrow	*Spizella pusilla.*	Blue-gray gnatcatcher	*Polioptila cærulea.*
Song sparrow	*Melospiza fasciata.*	Wood thrush	*Turdus mustelinus.*
Towhee	*Pipilo erythrophthalmus.*	American robin	*Merula migratoria.*
Cardinal	*Cardinalis cardinalis.*	Bluebird	*Sialia sialis.*
Indigo bunting	*Passerina cyanea.*		

Many other creatures likewise find a natural home within these limits, and thoug no systematic collection has yet been made, there has been observed during the sea son in the Park or its immediate vicinity the common woodchuck, the cotton-ta rabbit, the Virginia opossum, and the flying squirrel.

BOTANY OF THE ZOOLOGICAL PARK.

An examination of the flora has been made by Mr. W. Hunter, an employé of th Park and a competent botanist, who received advice and assistance from Prof. Lest F. Ward and Prof. W. H. Knowlton. The list of plants is necessarily incomplet owing to the fact that the observations did not commence until late in the seaso Excluding the trees a list of which has already been given, the following were note

lematus Virginiana, L.
nemonella thalictroides, Spach.
nemone Virginiana, L.
[epatica triloba, Chaix.
anunculus repens, L.
anunculus repens, L., var. hispidus, Torr. and Gray.
conitum uncinatum, L.
imicifuga racemosa, Nutt.
simina triloba, Dunal.
[enispermum Canadense, L.
odophyllum peltatum, L.
anguinaria Canadensis, L.
asturtium palustre, D. C.
asturtium Armoracia, Fries.
arbarea vulgaris, R. Br.
rabis Canadensis, L.
entaria laciniata, Muhl.
raba verna, L.
apsella Bursa-pastoris, Moench.
epidium Virginicum, L.
echea minor, Walt.
iola cucullata, Ait.
iola sagittata, Ait.
iola pedata, L. In bloom October 21, 1890.
iola pedata, L., var. bicolor, Pursh. In bloom October 9, 1890.
olygala verticillata, L.
ianthus Armeria, L.
aponaria officinalis, L.
ilene stellata, Ait.
erastium viscosum, L.
tellaria media, Smith.
tellaria pubera, Michx.
nychia dichotoma, Michx.
ortulaca oleracea, L.
laytonia Virginica, L.
scyrum Crux-Andreæ, L.
[ypericum perforatum, L.
ypericum maculatum, Walt.
[ypericum mutilum, L.
[ypericum nudicaule, Walt.
[alva rotundifolia, L.
ida spinosa, L.
butilon Avicennæ, Gaertn.
inum Virginianum, L.
eranium maculatum, L.
xalis violacea, L.
xalis corniculata, L., var. stricta, Sav.
npatiens pallida, Nutt.
npatiens fulva, Nutt.
uonymus Americanus, L.
elastrus scandens, L.
eanothus Americanus, L.
itis Labrusca, L.
Vitis æstivalis, Michx.
Vitis cordifolia, Lam.
Ampelopsis quinquefolia, Michx.
Staphylea trifolia, L.
Rhus typhina, L.
Rhus glabra, L.
Rhus copallina, L.
Rhus Toxicodendron, L.
Baptisia tinctoria, R. Br.
Trifolium arvense, L.
Trifolium pratense, L.
Trifolium repens, L.
Tephrosia Virginiana, Pers.
Stylosanthes elatior, Swartz.
Desmodium nudiflorum, D. C.
Desmodium paniculatum, D. C.
Lespedeza reticulata, Pers.
Vicia Caroliniana, Walt.
Phaseolus perennis, Walt.
Strophostyles peduncularis, Ell.
Cassia Chamæcrista, L.
Cassia nictitans, L.
Spiræa Aruncus, L.
Rubus occidentalis, L.
Rubus villosus, Ait.
Rubus Canadensis, L.
Geum album, Gmel.
Fragaria Virginiana, Duchesne.
Potentilla Norvegica, L.
Potentilla Canadensis, L.
Agrimonia Eupatoria, L.
Agrimonia parviflora, Hook.
Rosa lucida, Ehrh.
Rosa Carolina, L.
Saxifraga Virginiensis, Michx.
Heuchera Americana, L.
Hydrangea arborescens, L.
Penthorum sedoides, L.
Cuphea viscosissima, Jacq.
Epilobium coloratum, Muhl.
Ludwigia alternifolia, L.
Ludwigia palustris, Ell.
Œnothera biennis, L.
Œnothera fruticosa, L.
Gaura biennis, L.
Circæa Lutetiana, L.
Passiflora lutea, L.
Sanicula Canadensis, L.
Cicuta maculata, L.
Cryptotænia Canadensis, D. C.
Thaspium barbinode, Nutt.
Angelica hirsuta, Muhl.
Daucus carota, L.
Aralia nudicaulis, L.
Cornus stolonifera, Michx.
Sambucus Canadensis, L.

Viburnum prunifolium, L.
Viburnum dentatum, L.
Lonicera sempervirens, Ait.
Cephalanthus occidentalis, L.
Houstonia purpurea, L.
Houstonia cærulea, L.
Mitchella repens, L.
Diodia teres, Walt.
Galium Aparine, L.
Galium triflorum, Michx.
Galium pilosum, Ait.
Vernonia Noveboracensis, Willd.
Elephantopus Carolinianus, Willd.
Eupatorium purpureum.
Eupatorium perfoliatum, L.
Eupatorium ageratoides, L.
Eupatorium cœlestinum, L.
Chrysopsis Mariana, Nutt.
Solidago bicolor, L.
Solidago bicolor, L., var. concolor, Gray.
Solidago cæsia, L.
Solidago ulmifolia, Muhl.
Solidago nemoralis, Ait.
Solidago lanceolata, L.
Solidago Canadensis, L.
Sericocarpus conyzoides, Nees.
Aster corymbosus, Ait.
Aster patens, Ait.
Aster undulatus, L.
Aster ericoides, L.
Aster paniculatus, Lam.
Aster puniceus, L.
Aster linariifolius, L.
Erigeron Canadensis, L.
Erigeron bellidifolius, Muhl.
Erigeron annuus, Pers.
Erigeron strigosus, Muhl.
Antennaria plantaginifolia, Hook.
Gnaphalium polycephalum, Michx.
Polymnia Canadensis, L.
Silphium trifoliatum, L.
Chrysogonum Virginianum, L.
Ambrosia trifida, L.
Ambrosia trifida, L., var. integrifolia, Gray.
Ambrosia artemisæfolia, L.
Xanthium strumarium, L.
Eclipta procumbens, Michx.
Rudbeckia fulgida, Ait.
Rudbeckia laciniata, L.
Helianthus divaricatus, L.
Helianthus doronicoides, Lam.
Actinomeris squarrosa, Nutt.
Coreopsis verticillata, L.
Bidens, frondosa, L.
Bidens chrysanthemoides, Michx.
Bidens bipinnata, L.
Helenium autumnale, L.
Achillea Millefolium, L.
Anthemis arvensis, L.
Chrysanthemum Leucanthemum, L.
Arnica nudicaulis, L.
Erecthites hieracifolia, Raf.
Arctium lappa, L.
Cnicus lanceolatus, Hoffm.
Cnicus altissimus, Willd, var. discolo[r], Gray.
Cnicus altissimus, Willd.
Hieracium venosum, L.
Taraxacum officinale, Weber. In bloo[m] Oct. 9, 1890.
Chondrilla juncea, L. Opposite upp[er] quarry.
Lactuca Canadensis, L.
Lactuca Canadensis, L., var. integrifoli[a], Torr. & Gray.
Lactuca leucophæa, Gray.
Prenanthes serpentaria, Pursh.
Lobelia syphilitica, L.
Lobelia spicata, Lam.
Lobelia inflata, L.
Specularia perfoliata, A. D. C.
Gaylussacia resinosa, Torr & Gray.
Vaccinium vacillans, Solander.
Epigæa repens, L.
Gaultheria procumbens, L.
Leucothoë racemosa, Gray.
Kalmia latifolia, L.
Rhododendron nudiflorum, Torr.
Chimaphila umbellata, Nutt.
Chimaphila maculata, Pursh.
Monotropa uniflora, L.
Steironema ciliatum, Raf.
Chionanthus Virginica, L.
Apocynum cannabinum, L.
Asclepias tuberosa, L.
Sabbatia angularis, Pursh.
Phlox maculata, L.
Polemonium reptans. L.
Ellisia Nyctelea, L.
Cynoglossum Virginicum, L.
Echinospermum Virginicum, Lehm.
Echium vulgare, L.
Ipomœa hederacea, Jacq.
Ipomœa purpurea, Lam.
Ipomœa lacunosa, L.
Convolvulus spithamæus, L.
Solanum nigrum, L.
Solanum Carolinense, L.
Physalis pubescens, L.
Datura stramonium, L.
Datura tatula, L.

Verbascum Thapsus, L.
Verbascum Blattaria, L.
Linaria vulgaris, Mill.
Scrophularia nodosa, L., var. Marylandica, Gray.
Chelone glabra, L.
Mimulus ringens, L.
Ilysanthes riparia, Raf.
Veronica officinalis, L.
Gerardia pedicularia. L.
Gerardia flava, L.
Gerardia tenuifolia, Vahl.
Pedicularis Canadensis, L.
Epiphegus Virginiana, Bart.
Tecoma radicans, Juss.
Ruellia ciliosa, Pursh.
Dianthera Americana, L.
Phryma Leptostachya, L.
Verbena urticæfolia, L.
Trichostema dichotomum, L.
Collinsonia Canadensis, L.
Mentha Canadensis, L.
Lycopus Virginicus, L.
Lycopus sinuatus, Ell.
Cunila Mariana, L.
Pycnanthemum incanum, Michx.
Calamintha Nepeta, Link.
Calamintha Clinopodium, Benth.
Hedeoma pulegioides, Pers.
Salvia lyrata, L.
Monarda fistulosa, L.
Lophanthus nepetoides, Benth.
Nepeta Glechoma, Benth.
Scutellaria lateriflora, L.
Scutellaria serrata, Andrews.
Scutellaria pilosa, Michx.
Brunella vulgaris, L.
Lamium amplexicaule, L.
Plantago major, L.
Plantago Rugelii, Decsne.
Plantago lanceolata, L.
Amarantus paniculatus.
Amarantus retroflexus, L.
Amarantus spinosus, L.
Chenopodium album, L.
Chenopodium, ambrosioides, L.
Phytolacca decandra, L.
Polygonum orientale, L.
Polygonum Pennsylvanicum, L.
Polygonum Virginianum, L.
Polygonum aviculare, L.
Polygonum erectum, L.
Polygonum sagittatum, L.
Polygonum dumetorum, L., var. scandens, Gray.
Rumex crispus, L.
Rumex obtusifolious, L.
Rumex Acetosella, L.
Asarum Canadense, L.
Aristolochia Serpentaria, L.
Lindera Benzoin, Meisner.
Euphorbia maculata, L.
Euphorbia hypercifolia, L.
Euphorbia corollata, L.
Acalypha Virginica, L.
Laportea Canadensis, Gaudichaud.
Pilea pumila, Gray.
Bœhmeria cylindrica, Willd.
Alnus serrulata, Ait.
Corylus Americana, Walt.
Salix humilis, Marshall. Above lower quarry.
Arisæma triphyllum, Torr.
Symplocarpus fœtidus, Salisb.
Orchis spectabilis, L.
Goodyera pubescens, R. Br.
Corallorhiza odontorhiza, Nutt.
Hypoxys erecta, L.
Dioscorea villosa, L.
Smilax rotundifolia, L.
Smilax glauca, Walt.
Polygonatum biflorum, Ell.
Smilacina racemosa, Desf.
Erythronium Americanum, Smith.
Uvularia perfoliata, L.
Medeola Virginica, L.
Luzula campestris, DC.
Juncus tenuis, Willd.
Tradescantia Virginica, L.
Cyperus strigosus, L.
Cyperus ovularis, Tore.
Rhynchospora glomerata, Vahl.
Carex platyphylla, Carey.
Leersia oryzoides, Swartz.
Phleum pratense, L.
Cynodon Dactylon, Ters.
Brachyelytrum, aristatum, Beauv.
Eleusine Indica, Gærtn.
Muhlenbergia Mexicana, Trin.
Muhlenbergia diffusa, Schreb.
Dactylis glomerata, L.
Poa annua, L.
Poa compressa, L.
Poa pratensis, L.
Poa brevifolia, Muhl.
Eragrostis major, Host.
Eragrostis pectinacea, Gray.
Bromus secalinus, L.
Elymus Virginicus, L.
Elymus striatus, Willd.
Paspalum setaceum, Michx
Panicum sanguinale, L.

Panicum latifolium, L.
Panicum microcarpon, Muhl.
Panicum dichotomum, L.
Panicum Crus-galli, L.
Setaria glauca, Beauv.
Setaria viridis, Beauv.
Erianthus saccharoides, Michx.
Andropogon fuscatus.
Andropogon Virginicus, L.
Equisetum hyemale, L.
Polypodium vulgare, L.
Pteris aquilina, L.
Adiantum pedatum, L.
Asplenium Trichomanes L.
Asplenium ebeneum, Ait.
Asplenium thelypteroides, Michx
Asplenium Filix-fœmina, Bernh.
Phegopteris hexagonoptera, Fee.
Aspidium Novaboracense, Swartz.
Aspidium Filix-mas, Swartz.
Aspidium acrostichoides, Swartz.
Cystopteris fragilis, Bernh.
Onoclea sensibili.
Dicksonia pilosiuscula, Walld.
Botrychium ternatum, Swartz, var. obliquum, Milde.
Botrychium ternatum, Swartz, var. dissectum, Milde.
Botrychium Virginianum, Swartz.
Lycopodium complanatum, L.

GEOLOGY OF THE ZOOLOGICAL PARK.

A special report upon the geology of the Park has been kindly furnished by Mr. W. J. McGee, geologist to the Geological Survey.

"There is transmitted herewith a geologically colored map of the National Zoological Park.

"Except that the prevailing rock formation is complex in structure and of age not yet definitely determined, the geology of the Park is exceedingly simple. The formations are:

Recent	Alluvium.
Pleistocene	Columbia loam and gravel.
Cretaceous (?)	Potomac gravel.
Archean (?)	Piedmont gneiss. Vein quartz. Steatite.

"In addition to these well characterized formations there is a limited variety of residua left on decomposition of rock in place, of torrential or overplacement deposits formed by wash adown slopes, etc.

"The recent alluvium is confined to the channel and flood plain of Rock Creek. It consists of loam, sand, and gravel partly derived from the older formations within the Park, but mainly brought in by Rock Creek from beyond the limits of that reservation. These materials are sometimes irregularly stratified, but again assorted into sheets, sand-banks, gravel-bars, and more extended stretches of loam. It should be observed that the alluvium area, together with the channel meandering through it, are coterminous with the flood plain of Rock Creek, and hence are subject to overflow during great freshets.

"The Columbia formation is a deposit of loam, gravel, bowlders, etc., formed during the first ice invasion of the glacial period. Its age is therefore early Pleistocene. About rivers the formation commonly consists of two members, the upper a homogeneous loam commonly red or brown in color, and the lower a bed of sand, gravel, cobble-stones, and bowlders commonly stained brown by ferric oxide, sometimes stratified, and here and there displaying a peculiar black stain which is mainly ferruginous, but has been found to contain a trace of cobalt. Along the rivers of the Middle Atlantic slope the formation is sometimes fashioned into terraces; and some of its best developments in the District of Columbia (from which the name is taken) are terraciform. In the Park the deposit displays the usual division into a superior loam and an inferior bed of coarse materials; and the usual topographic form is assumed since the deposit is practically confined to the pine-clad terrace or bench north and west of Rock Creek, in the central part of the reservation. The formation is indeed confined to these terraces, save that an ill-defined and perhaps scarcely continu-

ous spur extends into the little valley of the branch that forms the principal affluent of Rock Creek, and that another spur (from which the loam has been washed, laying bare the coarse materials of the inferior member) extends southeastward beyond the terrace-scarp toward the upper angle in the course of Rock Creek. The red loam of the upper member was derived mainly from the Piedmont gneiss of the upper reaches of Rock Creek; while the lower member consists of sand and some loam from the same source, well-rounded pebbles and cobble-stones from the Potomac formation, angular or slightly water-worn fragments of quartz from the veins of that material cutting the gneiss both within the Park and beyond its limits, bowlders of gneiss, etc.

"The Potomac formation is a series of sands, clays, and gravels extending from the Roanoke to the Delaware, but best developed along the Potomac River, in honor of which the formation was christened. The age, determined through paleo-botany by Professor Fontaine, is early Cretaceous; determined from vertebrate paleontology by Professor Marsh, is Jurassic; and as determined by physical geology the formation represents the beginning of the Cretaceous. Along its westernmost margin the formation is usually represented by outlying patches of gravel commonly crowning eminences; and this is the character displayed in the Park. Five small areas only occur in the reservation: There is a remnant retaining the original structure crowning the second greatest eminence in the northwestern part; there are two small remnants, one certainly displaying the original structure upon the eminence occupied by the Holt mansion in the southeastern corner of the reservation; there is a fourth remnant, which may be in place, but is probably a residuum let down and disturbed by the decay of the subjacent gneiss, mid-length of the southwestern boundary; and there is another small area, which is certainly residual in the northeastern portion. These remnants and others of like character beyond the limits of the Park are of especial interest in that their cobble-stones were extensively used by aboriginal men for the manufacture of rude implements. Modern man also utilizes the cobble-stones extensively for road-making and other purposes.

"The Piedmont gneiss is a vast complex of crystalline rocks extending from Alabama to New Jersey. Many rock varieties are recognized within the complex; but they have not yet been systematically differentiated throughout any considerable part of the terrane. Within the Park the prevailing rocks are schists varying in composition from place to place, and varying also in dip and strike. In general the dip is high, sometimes nearly vertical, and the prevailing strike is northerly and southerly. The gneiss is the prevailing formation of the Park. It is overlain in part by alluvium and by the Columbia formation, as well as by the isolated remnants of the Potomac formation; and elsewhere it has been decomposed to a considerable depth so that it is concealed by a mantle of materials derived from its own destruction either in place or carried down slopes by gravity and the wash of storm waters. This mantle of decomposed rock may be 20, 50, or even more feet in thickness, and probably averages no less than 15 or 20 feet over the entire reservation. So profound has been this decomposition of the crystalline rocks that exposures occur only in the steeper bluffs where Rock Creek has corroded rapidly during the later Neocene, Pleistocene, and recent times. The rocks of the Piedmont belt are seldom sufficiently firm, tough, and durable to yield valuable building stones, and within the Park they give little promise in this direction. At three points only is the promise even fair: In the extreme northwestern corner of the reservation, toward the northern end of the old quarry mid-length of the eastern side, and in the old quarry opposite Adams's Mill.

"Within the Park, as beyond its limits, the Piedmont gneisses are frequently intersected by veins of quartz. These range from sheets but a fraction of an inch thick to great masses many yards across. Some of the more conspicuous examples have been mapped. No law governing the trend or inclination of these veins is indicated by these exposures, and no such law has thus far been formulated; but although the relation of the quartz veins to the gneisses is not apparent, there is an obvious rela-

tion between these obdurate rock masses and the topography. Many of them appear in eminences or in the extremities of salients jutting streamward from the general upland; and even where they have not been observed their existence may be suspected in all the more sharply-cut salients.

"The Piedmont gneiss varies from place to place in mineral composition as well as in structure, and now and then sheets or masses of steatite—the soapstone of the aborigines and early white settlers—may be found. This is true within the Park as well as beyond its limits, and at two points quarries have been opened for the extraction of these materials for industrial purposes.

"The topographic configuration of the Park is well shown upon the map. The gracefully curved hills and steep ravines characteristic of the country about the National Capital here represent the work of Rock Creek during ages of erosion, and from hills, valleys, and ravines the systematic geologist reads a record of erosion upon lines first determined by rock structure, afterward modified by the superposition of an extensive formation—the Potomac—and finally developed under the influence of these conditions affected albeit by the structure of the rocks reached by the stream in the latter stages of its cutting. It is by reason of the varied conditions represented in this complicated history that, while the configuration is commonly adjusted to the hard quartz veins, there are cases in which quartz and topography are manifestly independent in their distribution.

"The Park is watered as well as drained by Rock Creek and a few spring-born streamlets. Within the reservation there are two walled springs, two others that have received some attention, and a number of minor seeps; but the yield of these springs is trifling, none now giving permanent streams and all threatening to diminish as the surface is further deforested or trampled. Wells of small yield may doubtless be found by excavating in nearly any part of the Park; but the Potomac and Columbia areas are too small to afford reservoirs; the dips of gneiss are too steep to give strong subterranean streams, and the structure of the prevailing formation is too complex to permit determination of such small subterranean water-ways as may exist; moreover, wells east of Rock Creek will inevitably be contaminated within a few years, if not at present, in consequence of the recent spread of population over the adjacent uplands; and there is prospective danger of like contamination west of the water-way. Accordingly the Park must look either to Rock Creek or beyond its own limits for permanent water supply."

Unmistakable signs of Indian occupation have been found. Professor Holmes, the archæologist of the Geological Survey, made a careful examination of the bowlder-beds of the Potomac formation, and found many chipped implements, showing that here, as elsewhere in the Rock Creek region the quartzite pebbles are shaped into weapons While most of those found were the imperfectly formed and rejected stones, some portions of finished blades were discovered. It is not improbable that an Indian village once existed within the Park limits, near the soapstone quarry on the eastern side of the creek.

At the close of the fiscal year, the development and adaptation of this beautiful region to the purposes of a zoological park were already commenced; competent professional advice was procured, and plans were under consideration for accommodating the animals now in the collection and those that will shortly be added.

Respectfully submitted.

FRANK BAKER,
Acting Manager.

MR. S. P. LANGLEY,
Secretary of Smithsonian Institution.

APPENDIX IV.

REPORT OF THE LIBRARIAN.

SIR: I have the honor respectfully to submit my report on the work of the library during the year from July 1, 1889, to June 30, 1890.

The work of recording and caring for the accessions has been carried on as during the preceding year, the entry numbers on the accession book running from 193,431 to 207,175.

The following condensed statement shows the character and number of these accessions:

Publications received between July 1, 1889, and June 30, 1890:

	Octavo or smaller.	Quarto or larger.	Total.
Volumes	1,236	527	1,763
Parts of volumes	5,202	8,256	13,458
Pamphlets	3,776	554	4,330
Maps			636
Total			20,187

Of these publications 8,695 (namely, 785 volumes, 6,900 parts of volumes, and 1,010 pamphlets) were retained for use in the National Museum, and 1,059 medical dissertations were deposited in the library of the Surgeon-General, U. S. Army. The remainder were promptly sent to the Library of Congress on the Monday following their receipt.

Among the most important additions to the list of serials during the year may be mentioned the following publications:

Advance.
American Agriculturist.
American Apiculturist.
American Architect.
American Artisan.
American Art Printer.
American Athlete.
American Cabinetmaker and Upholsterer.
American Carpet and Upholstery Trade.
American Chemical Review.
American Cultivator.
American Dairyman.
American Druggist.
American Engineer.
American Garden.
American Journal of Railway Appliances.
American Lithographer and Printer.
American Machinist.
American Miller.
American Silk Journal.
American Teacher.
L'Ami de l'Enfance.
Annales de l'Académie d'Archéologie d'Anvers.
Annales de l'Extrême Orient et de l'Afrique.
Annual Report of the Metropolitan Museum, New York.
Annual Report of the New York State Forest Commission.
Annual Report of the Pennsylvania Academy of Fine Arts.
Annual Report of the Providence Public Library.
Anthony's Photographic Bulletin.
L'Anthropologie.

Architecture and Building.
Archives de Physiologie.
Arizona Weekly Journal-Miner.
Astronomische Arbeiten (K. K. Gradmessungsbureau, Wien).
Beacon (Photographic).
Bibliotheca Sacra.
Boletin del Ministerio de Industria, Chile.
Builder and Wood Worker.
Building Budget.
Bulletin de l'Académie d'Archéologie d'Anvers.
Bulletin du Comité des Forges de France.
Bulletin of the Geographical Society of Bucharest.
Bulletin of the Public Library of Cincinnati.
Bulletin de la Société Belge d'Électriciens.
Bulletin de la Société Bretonne de Géographie.
Bulletin de la Société de Géographie de Marseille.
Bulletin de la Société de Géographie de Toulouse.
Brickmaker.
California Architect.
Carpet and Upholstery Trade.
Carriage Monthly.
Central School Journal, Keokuk, Iowa.
Circulars of the Engineers' Club of Kansas City.
Chicago Journal of Commerce.
Colorado School Journal.
Common School Education.
Connoisseur.
Contributions of the Old Residents' Historical Association, Lowell, Mass.
L'Economiste Français.
Edinburgh Circulars.
Education.
Educational Current.
Educational Journal, Toronto.
Educational Monthly.
Educational Record.
Electrical Engineer.
Electrical Review.
Electrical World.
Entomological News.
Farmers' Review.
Freeman.
Gleanings in Bee Culture.
Granite Monthly.
Hatter and Furrier.
Husbandman.
Homiletic Monthly.
Illinois School Journal.
Indiana School Journal.
Industrial Review.
Industrial World.
Inland Architect.
Inland Printer.
Iron.
Iron Industries Gazette.
Journal du Ciel.
Journal of Comparative Medicine.
Journal of Education.
Journal de l'Instruction Publique, Montreal.
Journal de Mathématiques Élémentaires.
Journal de Mathématiques Spéciales.
Journal of the Tyneside Geographical Society.
Journal of the United States Cavalry Association.
Loon.
Lutheran Church Review.
Magazine of Art.
Magazine of Christian Literature.
Manuel Général de l'Instruction Primaire.
Manufacturers' Gazette.
Massachusetts Ploughman.
Mathesis.
Mechanical News.
La Medicina Cientifica.
Milling World.
Mining and Scientific Press.
Mining and Scientific Review.
Missouri School Journal.
Mittheilungen des Deutsch-Amerikanischen Techniker-Verbandes.
Mittheilungen des Deutschen wissenschaftlichen Vereines in Mexico.
Moniteur du Praticien.
Mouvement Géographique.
Musical Herald.
National Car and Locomotive Builder.
National Educator.
North American Fauna.
Northwestern Miller.
Northwestern Mechanic.
Nouvelles Annales de la Construction.
Observer.
Ohio Educational Monthly.
Orchard and Garden.
Ornithologisches Jahrbuch.
Palmarès de l'école polytechnique et de l'académie Commerciale Catholique de Montreal.
Paper and Press.
Paper Trade Journal.
Papers of the American Astronomical Society.

Pharmaceutical Era.
Photographic Times.
Popular Gardening.
Popular Science News.
Portage Lake Mining Gazette.
Pottery and Glassware Reporter.
Public School Journal, Bloomington.
Public School Journal, Mount Washington, Ohio.
Prairie Farmer.
Proceedings of the Car Builders' Association.
Proceedings of the Civil Engineers' Association of Nebraska.
Proceedings of the Engineering Society of Western Pennsylvania.
Proceedings of the Long Island Historical Society.
Proceedings of the Western Society of Engineers.
Professional Papers of the United States Engineering School.
Quarterly Journal of Economics.
Railroad Engineering Journal.
Railway Age.
Railway News.
Railway Review.
Railway World.
Records of the Australian Museum.
Records of the Bible Society, New York.
Records and Papers of the New London County Historical Society.
Reports of the Boston Society of Civil Engineers.
Reports of the Brooklyn Institute.
Reports of the Denver Society of Civil Engineers.
Reports of the Geological Survey of Newfoundland.
Reports of the Iowa Society of Civil Engineers.
Reports of the Iron and Steel Association.
Reports of the Michigan Association of Civil Engineers.
Reports of the National Civil Service Association.
Reports of the Nebraska Weather Service.
Reports of the Ohio Society of Civil Engineers.
Reports of the State Horticultural Society of New Jersey.
Revista de Ciencias Médicas.
Revista da Sociedade de Geographia do Rio de Janeiro.
Roller Mill.
St. Louis and Canadian Photographer.
St. Louis Miller.
Samfundet.
School Bulletin.
School Education.
School Journal.
Selected Papers, Civil Engineers' Club, Champaign, Illinois.
Semi-Tropical Planter.
Sunday-School Times.
Shoe and Leather Reporter.
Southwestern Journal of Education.
Spirit of the Times.
Statistisk Tidskrift.
Teacher.
Techniker.
Texas School Journal.
Textile Colorist.
Transactions of the Canadian Society of Civil Engineers.
Transactions of the Geographical Society of Quebec.
Transactions of the Illinois State Horticultural Society.
Trudy. Vjestnik literatury i nauki.
Typographic Advertiser.
Ulster Agriculturist.
Vjestnik Estestvoznanija.
Wallace's Monthly.
Western Architect and Builder.
Western School Journal.
Western Sportsman.
Wood Worker.
World's Progress.
Le Yacht.
Zeitschrift für Katholische Theologie.
Zoe.

The following universities have sent complete sets of all their academic publications, including the inaugural dissertations published by the students on graduation: Basel, Bern, Bonn, Dorpat, Erlangen, Freiburg-im-Breisgau, Giessen, Göttingen, Greifswald, Halle-an-der-Saale, Helsingfors, Jena, Kiel, Köningsberg, Leipzig, Marburg, Strassburg, Tübingen, Utrecht, Würzburg, and Zürich.

Among other important accessions may be mentioned the following: A complete set of the catalogues of the Bodleian Library; a complete set of the publications of the National Civil Service Reform Association; a set of thirty graduating dissertations delivered at the University of Upsala during the rectorship of Linnæus, pre-

sented by the Högre Allmänna Läroverk at Vesterås, Sweden; a full set of the publications of the Board of Trade, London; a full set of the publications of Cornell University, comprising 23 volumes and 35 pamphlets; Lendenfeldt's "Monograph of Horny Sponges," presented by the Royal Society; two more volumes of the *Challenger* Report, namely Vol. 32 of the Zoology and Vol. 2 of the Chemistry and Physics, from the British Government; a large and important series of Indian government publications from the secretary of state for India, London; a large and valuable series of French Government publications, from the Bureau Français des Echanges Internationaux; A. Moksáry's "Monographia Chrysididarum orbis terrarum universi," from the Royal Hungarian Academy at Budapest in addition to the highly valuable series of publications usually sent by this academy; full sets of State reports, etc., from New Jersey and Vermont; complete sets of charts and other publications from the hydrographic offices of Great Britain and Russia; parliamentary reports from Germany and Sweden; a remarkable collection of photographs from Mecca, taken in the Holy City itself, entitled "Bilder aus Mecca," presented by the author, C. S. Hurgronje, Leiden, Netherlands; "Briefwechsel des Gottfried Wilhelm Leibnitz," from the Royal Public Library, Hanover; a collection of 21 physical papers, from Prof. G. Gore, of Birmingham, England; and the following books, from the respective authors: "Through and Through the Tropics;" "Norsk, Lapp, and Finn;" "Land of the White Elephant;" "Around and About South America," by Frank Vincent, jr.; "Avifauna Italica," by Professor Giglioli; "Flora of British India," pt. 16, by Sir Joseph Dalton Hooker; "Handbuch der Gewebelehre der Menschen," by Prof. Albert Kölliker; "Von der Capstadt ins Land der Maschukulumbe," by Dr. Emil Holub; and "Gypsies of Modern India," and "Ancient and Modern Britons," by David MacRitchie.

Very respectfully submitted.

JOHN MURDOCH,
Librarian.

Mr. S. P. LANGLEY,
Secretary of the Smithsonian Institution.

APPENDIX V.

PUBLICATIONS OF THE YEAR.

SMITHSONIAN CONTRIBUTIONS TO KNOWLEDGE.

As mentioned in the last report, a memoir on the "Genesis of the Arietidæ," by Prof. Alpheus Hyatt, had been accepted for publication in the series of "Contributions to Knowledge," and was in the hands of the printer. This work has been completed, and issued during the year as No. 673, in the Smithsonian list of publications. It forms a quarto volume of 265 pages (including introduction, index, and explanations of plates), and is illustrated with 35 figures in the text, 6 folding charts or tables, and 14 plates, of which 10 are heliographs.

No. 691. "The Solar Corona, discussed by Spherical Harmonics," by Prof. Frank H. Bigelow. This memoir is published in quarto form in the same style as the Contributions to Knowledge, though not designed to be included in the volumes of that series. It comprises 22 pages, and is illustrated with 4 diagrams, and 1 phototype plate.

No. 692. "Photographs of the Corona, taken during the Total Eclipse of the Sun, January 1, 1889. Structure of the Corona," by David P. Todd. This, like the preceding, although in quarto form, is not intended for the Contribution series. It consists of 9 pages of text, with 2 photographic plates, showing 9 different views of the Solar Corona during the total eclipse.

No. 731. Vol. XXVI of the Smithsonian Contributions to Knowledge. This volume comprises: Article 1, "Researches upon the venoms of Poisonous Serpents," by S. Weir Mitchell, M. D., and Edward T. Reichert, M. D., published in 1886; article 2, "Genesis of the Arietidæ," by Alpheus Hyatt, above described. This forms a quarto volume of xi + 461 pages, illustrated with 40 wood-cuts and 19 plates.

SMITHSONIAN MISCELLANEOUS COLLECTIONS.

No. 694. "Report on Smithsonian Exchanges for the year ending June 30, 1887," by George H. Boehmer. (From the Smithsonian Report for 1887.) Octavo pamphlet of 24 pages.

No. 695. "The Advance of Science in the last Half-century," by Thomas H. Huxley. (From the Smithsonian Report for 1887.) Octavo pamphlet of 42 pages.

No. 696. "An Account of the Progress in Astronomy in the year 1886," by William C. Winlock. (From the Smithsonian Report for 1887.) Octavo pamphlet of 89 pages.

No. 697. "An Account of the Progress in North American Geology in the year 1886," by Nelson H. Darton. (From the Smithsonian Report for 1887.) Octavo pamphlet of 41 pages.

No. 698. "Bibliography of North American Paleontology in the year 1886," by John Belknap Marcou. (From the Smithsonian Report for 1887.) Octavo pamphlet of 57 pages.

No. 699. "An Account of the Progress in Vulcanology and Seismology in the year 1886," by C. G. Rockwood, jr. (From the Smithsonian Report for 1887.) Octavo pamphlet of 24 pages.

No. 700. "An Account of the Progress in Geography and Exploration in the year 1886," by William Libbey, jr. (From the Smithsonian Report for 1887.) Octavo pamphlet of 13 pages.

No. 701. "An Account of the Progress in Physics in the year 1886," by George F. Barker. (From the Smithsonian Report for 1887.) Octavo pamphlet of 60 pages.

No. 702. "An Account of the Progress in Chemistry in the year 1886," by H. Carrington Bolton. (From the Smithsonian Report for 1887.) Octavo pamphlet of 61 pages.

No. 703. "An Account of the Progress in Mineralogy in the year 1886," by Edward S. Dana. (From the Smithsonian Report for 1887.) Octavo pamphlet of 28 pages.

No. 704. "An Account of the Progress in Zoology in the year 1886," by Theodore Gill. (From the Smithsonian Report for 1887.) Octavo pamphlet of 46 pages.

No. 705. "An Account of the Progress in Anthropology in the year 1886," by Otis T. Mason. (From the Smithsonian Report for 1887.) Octavo pamphlet of 45 pages.

No. 706. "Miscellaneous Papers relating to Anthropology." (From the Smithsonian Report for 1887.) This collection comprises: "An Indian Mummy," by James Lisle; "Mound in Jefferson County, Tennessee," by J. C. McCormick; "Ancient Mounds and Earthworks in Floyd and Cerro Gordo Counties, Iowa," with 6 figures, by Clement L. Webster; "Indian graves in Floyd and Chickasaw Counties, Iowa," with 1 figure, by Clement L. Webster; "Ancient Mounds in Johnson County, Iowa," with 1 figure, by Clement L. Webster; "Ancient Mounds in Iowa and Wisconsin," with 1 figure, by Clement L. Webster; "Mounds of the Western Prairies," by Clement L. Webster; "The Twana, Chemakum, and Klallam Indians of Washington Territory," by Myron Eells; "Anchor Stones," with 7 figures, by B. F. Snyder; "Antiquities in Mexico," with 1 figure, by S. B. Evans; forming in all an octavo pamphlet of 123 pages, illustrated with 17 figures.

No. 707. "Biographical Memoir of Arnold Guyot," by James D. Dana. (From the Smithsonian Report for 1887.) Octavo pamphlet of 30 pages.

No. 708. "A Clinical Study of the Skull," by Harrison Allen, M. D. Octavo pamphlet of 83 pages, illustrated with 8 figures. This is the tenth of the series of "Toner Lectures."

No. 709. "Report on the Section of Steam Transportation in the U. S. National Museum, for the year ending June 30, 1886," by J. Elfreth Watkins, with 8 plates. (From the Smithsonian Report for 1886, Part II.) Octavo pamphlet of 22 pages.

No. 710. "The Meteorite Collection in the U. S. National Museum, a Catalogue of Meteorites represented, November 1, 1886," by F. W. Clarke. With one plate. (From the Smithsonian Report for 1886, Part II.) Octavo pamphlet of 11 pages.

No. 711. "The Gem Collection of the U. S. National Museum," by George F. Kunz. (From the Smithsonian Report for 1886, Part II.) Octavo pamphlet of 9 pages.

No. 712. "The Collection of Building and Ornamental Stones in the U. S. National Museum: a Hand-book and Catalogue." With 14 figures and 9 plates. By George P. Merrill. (From the Smithsonian Report for 1886, Part II.) Octavo pamphlet of 372 pages.

No. 713. "How to Collect Mammal Skins for purposes of Study and for Mounting." With 9 figures. By William T. Hornaday. (From the Smithsonian Report for 1886, Part II.) Octavo pamphlet of 12 pages.

No. 714. "List of Accessions to the U. S. National Museum during the year ending June 30, 1886; with descriptive notes." (From the Smithsonian Report for 1886, Part II.) Octavo pamphlet of 109 pages.

No. 715. "Cradles of the American Aborigines." With 46 figures. By Otis T. Mason. (From the Smithsonian Report for 1887, Part II.) Octavo pamphlet of 52 pages.

No. 716. "Notes on the Artificial Deformation of Children among Savage and Civilized Peoples; with a Bibliography." By Dr. J. H. Porter. (From the Smithsonian Report for 1887, Part II.) Octavo pamphlet of 23 pages.

No. 717. "The Human Beast of Burden." With 54 figures. By Otis T. Mason. (From the Smithsonian Report for 1887, Part II.) Octavo pamphlet of 59 pages.

No. 718. "Ethno-Conchology: a Study of Primitive Money." With 22 figures and nine plates. By Robert E. C. Stearns. (From the Smithsonian Report for 1887. Part II.) Octavo pamphlet of 38 pages.

No. 719. "The Extermination of the American Bison." With 21 plates and 1 folding map. By William T. Hornaday. (From the Smithsonian Report for 1887, Part II.) Octavo pamphlet of 184 pages.

No. 720. The Preservation of Museum Specimens from Insects and the effects of Dampness." With 5 figures. (From the Smithsonian Report for 1887, Part II.) Octavo pamphlet of 10 pages.

No. 721. "List of Accessions to the U. S. National Museum, during the year ending June 30, 1887, with descriptive notes." (From the Smithsonian Report for 1887, Part II.) Octavo pamphlet of 129 pages.

No. 724. "The George Catlin Indian Gallery in the U. S. National Museum; with memoir and statistics." Illustrated with 138 plates and 6 folding maps. By Thomas Donaldson. From the Smithsonian Report for 1885, Part II.) Octavo volume of vii+939 pages.

No. 732. "Throwing-sticks in the National Museum." With 17 plates. By Otis T. Mason. (From the Smithsonian Report for 1884, Part II.) Octavo pamphlet of 11 pages.

No. 733. "Basket-work of the North American Aborigines." With 64 plates. By Otis T. Mason. (From the Smithsonian Report for 1884, Part II.) Octavo pamphlet of 16 pages.

No. 734. "A Study of the Eskimo Bows in the U. S. National Museum." With 12 plates. By John Murdoch. (From the Smithsonian Report for 1884, Part II.) Octavo pamphlet of 10 pages.

No. 741. "Index to the Literature of Thermodynamics." Comprising Part I, a subject index under 54 topics; and Part II, an author index, with the titles of papers in full. By Alfred Tuckerman. Octavo volume of 244 pages.

No. 745. "Check-list of Publications of the Smithsonian Institution, to July, 1890," Octavo pamphlet of 35 pages.

SMITHSONIAN ANNUAL REPORTS.

No. 689. "Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution for the year ending June 30, 1887. Part I." This part comprises the report of the Institution proper, and contains the Journal of Proceedings of the Board of Regents at the annual meeting held January 12, 1887; the Report of the Executive Committee of the Board, and the Report of Professor Baird, the Secretary of the Institution; followed by the "General Appendix," in which are given the following papers: Advance of Science in the Last Half Century, by T. H. Huxley; Progress in Astronomy in 1886, by William C. Winlock; in North American Geology, by Nelson H. Darton; in North American Paleontology, by J. B. Marcou; in Vulcanology and Seismology, by C. G. Rockwood; in Geography and Exploration, by William Libbey; in Physics, by George F. Barker; in Chemistry, by H. Carrington Bolton; in Mineralogy, by Edward S. Dana; in Zoölogy, by Theodore Gill; and in Anthropology, by Otis T. Mason. Also, papers on an Indian Mummy, by James Lisle; Mound in Jefferson County, Tennessee, by J. C. McCormick; Ancient Mounds in Iowa, etc., by Clement L. Webster; Indians of Washington Territory, by Myron Eells; Anchor Stones, by B. F. Snyder; Antiquities in Mexico, by S. B. Evans; concluding with a Biographical Memoir of Arnold Guyot, by James D. Dana. The Report forms an octavo volume of xx+735 pages, illustrated with 10 figures and 3 plates.

No. 690. "Annual report of the Board of Regents of the Smithsonian Institute for

the year ending June 30, 1887. Part II." Being the report of the operations and condition of the U. S. National Museum. This part contains: 1. The report of the assistant secretary, G. Brown Goode, upon the condition and progress of the Museum. 2. Reports of the curators of the different departments. 3. Papers illustrative of the collections in the U. S. National Museum. 4. Bibliography for the year, including (1) the publications of the National Museum, and (2) papers by officers of the National Museum and others relatiog to Museum material. 5. List of accessions for the year. This part forms an octavo volume of xviii + 771 pages, illustrated with 127 figures, 31 plates, and 1 folding map.

No. 722. "Report of S. P. Langley, Secretary of the Smithsonian Institution, for the year ending June 30, 1889." Octavo pamphlet of 84 pages.

PUBLICATION OF THE BUREAU OF ETHNOLOGY.

No. 693. "Sixth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1884-'85." By J. W. Powell, Director. This work contains the introductory report of the Director, 58 pages, with accompanying papers as follows: "Ancient Art of the Province of Chiriqui," by William H. Holmes; "A Study of the Textile Art in its relation to the Development of Form and Ornament," by William H. Holmes; "Aids to the Study of the Maya Codices," by Cyrus Thomas; "Osage Traditions," by Rev. J. Owen Dorsey; "The Central Eskimo," by Dr. Franz Boas. The report forms a royal octavo volume of lviii+675 pages, illustrated with 546 figures, 7 plates, and 3 maps.

APPENDIX VI.

REPORT ON PROFESSOR MORLEYS RESEARCHES.

WASHINGTON, *January* 17, 1891.

Prof. S. P. LANGLEY,

DEAR SIR: The accompanying letter from Prof. A. A. Michelson I can gladly indorse in every particular. I am familiar with Professor Morley's work, having followed it from the start, and I know it to be the best work of its kind in the history of science. A part of it involves a re-determination of certain physical constants of oxygen and hydrogen; and on this side of the question the classical researches of Regnault are far excelled by the investigations so far made by Morley. Hitherto (for a period of 3 or 4 years), the experiments have been carried on by Professor Morley at his own personal expense, without aid from any institution. Such a burden no private individual should be compelled to bear; and I feel sure that aid given by the Smithsonian Institution will redound to its credit, and in the most direct manner tend to fulfill the intention of its founder, himself a chemist.

The work upon which Professor Morley is engaged is, from a chemical stand-point, fundamental in its character, and it has both a theoretical and a practical bearing. All of the calculations upon which accurate chemical analyses depend rest upon our knowledge of the atomic weights; and the ratio between oxygen and hydrogen is the corner-stone of the entire system. It is both the most important and the most difficult to measure of all the atomic weight ratios, and it directly affects nearly every other value in the whole series of constants. Furthermore, all the physical properties of the atoms are now believed to be functions of their mass, and this idea is dominant in the periodic law of Mendelejeff. That law shows the elements to be not independent of each other, but closely related; so that the exact measurement of their atomic weights bears directly upon the problem of the ultimate constitution of matter. If all matter is one entity, then the weights of the different so-called "elementary" atoms should be connected by some definite mathematical law; and such a law can only be developed upon the basis of the most refined experimental researches. In the measurement of atomic weights "accidental errors," which practically vanish from averages, do little harm; but the "constant errors" are troublesome and all-pervasive. Furthermore, since one atomic weight serves as the starting point for the determination of others, the constant errors become cumulative, and their elimination is anything but easy.

In Morley's determinations of the atomic weight of oxygen, the errors are controlled by exact manipulation on the one hand, and by wide variations of method on the other. If six or seven distinct methods of measurement, involving different possibilities of error, give at last the same value sought, then the presumption is that constant errors have been eliminated altogether. Up to the present date Professor Morley has investigated the preparation of oxygen and hydrogen in absolute purity, the influence of impurities in known amounts, the composition of water by volume, and the relative densities of the two gases. The series of experiments upon the composition of water by volume have already been made public, and the results obtained are accurate for a single experiment, to within one part in 26,000. Such accuracy was never before

approached, even remotely, in investigations of this kind. He now has in view the synthesis of water by several distinct quantitative processes, and these involve large weighings. For example, hydrogen is so light that large bulks must be taken in order that the errors of weighing may not exercise an appreciable influence. In order to do this, glass globes holding 20 litres are used; and their weight is considerable. The ordinary analytical balances, ranging from 200 to 1.0000 grammes, are wholly unavailable for the purpose, and hence an exceptional balance, such as Rüprecht has made for the International Bureau of Weights and Measures at Paris, becomes necessary. In the office of our own Coast and Geodetic Survey there is a balance approaching these in character; so sensitive as to show the difference between two standard kilogrammes placed side by side or one on top of the other. This difference in position of two weights is a difference of distance from the center of the earth of a few centimetres only, and yet it corresponds to a difference in weight of about .000015 gramme. This difference, according to Professor Mendenhall, is perfectly appreciable with the balances now in use. I can not say whether or not Rüprecht keeps these finer balances in stock, but I suspect that one would have to be built to order, so that some months would elapse before it could be delivered. The cost should not exceed $500, and the balance, after serving Professor Morley's purpose, might be returned to the Institution, where it would have permanent value. The present investigation could thus be assisted with little or no actual sinking of capital, and the aid to research would continue long after the single investigation of Professor Morley was finished. I sincerely hope that the assistance sought may be given.

Very respectfully,

F. W. CLARKE.

APPENDIX VII.

REPORT ON INTERNATIONAL CONGRESS OF ORIENTALISTS.

Prof. S. P. LANGLEY,
Washington, D. C.:

SIR: In accordance with your instruction I attended the Eighth International Congress of Orientalists as delegate of the Smithsonian Institution. The meetings of the congress were held in Stockholm and Christiania, under the auspices of His Majesty the King of Sweden and Norway, from September 1 to September 12, 1889. The members assembled in Stockholm on September 1 and adjourned on September 7 to meet in Christiania from September 8 to September 11. September 12 was spent in Götheburg, where a farewell reception was given.

Five general meetings were held and the various sections met daily for the transaction of business.

There were registered as subscribers to the congress 710 names (204 Scandinavians and 506 foreigners); more than one-half of the (286) foreign members were present. The foreign members came from twenty-eight different countries, as indicated in the following table:

Country.	Subscribers.	Present.	Country.	Subscribers.	Present.
1. Abyssinia	1	1	16. Italy	48	9
2. America	39	16	17. Japan	3	2
3. Austria	36	25	18. Persia	4	4
4. Belgium	4	1	19. Portugal	4	4
5. Brazil	1	1	20. Roumania	1	....
6. Colombia	1	1	21. Russia	26	18
7. Denmark	19	18	22. Servia	1	1
8. Egypt	7	4	23. Siam	2	1
9. England	84	58	24. Spain	3	1
10. Finland	13	11	25. Switzerland	8	4
11. France	41	19	26. Turkey	28	5
12. Germany	80	60	27. Sweden	142	142
13. Greece	2	1	28. Norway	62	6
14. Holland	39	17			
15. India	11	5	Total	710	*435

* In the London Academy of November 1, 1890, it is stated that the congress was attended by 459 Europeans, 16 Americans, 13 Asiatic, and 5 African scholars. This calculation is evidently based on the supposition that the Swedish and Norwegian subscribers were all present.

If arranged according to the number of subscribers the list would be as follows:

1. Sweden	142	15. Switzerland	8
2. England	84	16. Egypt	7
3. Germany	80	17. Persia	4
4. Norway	62	18. Portugal	4
5. Italy	48	19. Belgium	4
6. France	41	20. Japan	3
7. Holland	39	21. Spain	3
8. America	39	22. Siam	2
9. Austria	36	23. Greece	2
10. Turkey	28	24. Abyssinia	1
11. Russia	26	25. Brazil	1
12. Denmark	19	26. Colombia	1
13. Finland	13	27. Servia	1
14. India	11	28. Roumania	1

If arranged according to the number of members present the order would be:

1. Sweden	142	15. Persia	5
2. Germany	60	16. Egypt	4
3. England	58	17. Portugal	4
4. Austria	25	18. Switzerland	4
5. France	19	19. Japan	2
6. Denmark	18	20. Greece	1
7. Russia	18	21. Belgium	1
8. Holland	17	22. Siam	1
9. America	15	23. Abyssinia	1
10. Finland	11	24. Brazil	1
11. Italy	9	25. Colombia	1
12. Norway (?)	6	26. Servia	1
13. Turkey	5	27. Roumania	0
14. India	5	28. Spain	0

Comparing these figures with those of the preceding Oriental congresses it would seem that there is an increase of devotion to Oriental studies among European scholars. At the Seventh Congress, held at Vienna in 1886, there were 414 subscribers and 228 members present; at the Sixth Congress, held at Leyden in 1883, there were 453 subscribers and 219 members present; at the Fifth Congress, held at Berlin in 1881, there were 296 subscribers and 189 members present.

The following table indicates the number of subscribers and members present at each of the eight international congresses of Orientalists:

	Subscribers.	Present.*		Subscribers.	Present.
1. Paris (1873)	1,063	(†)	5. Berlin (1881)	1,296	189
2. London (1874)	1,491	(†)	6. Leyden (1883)	1,453	219
3. St. Petersburg (1876)	1,507	(†)	7. Vienna (1886)	1,414	228
4. Florence (1878)	1,218	127	8. Stockholm (1889)	1,710	493

* Eighty-nine *foreign* members attended. † Not recorded.

The increased interest is even more marked on the part of Americans. To the Vienna congress there were eleven American subscribers of whom five (Briggs, Leland, S. A. Smith, Thatcher, and Whitehouse) were present. To the Stockholm congress there were forty American subscribers, of whom sixteen were present. A list of the American subscribers is herewith subjoined:

1. Dr. Cyrus Adler, Johns Hopkins University, Baltimore, Maryland.
2. Dr. W. M. Arnolt, Johns Hopkins University, Baltimore, Maryland.
3. Prof. Charles A. Briggs, Union Theological Seminary, 700 Park avenue, New York.
4. Prof. Francis Brown, Union Theological Seminary, 700 Park avenue, New York.
*5. Prof. Thomas Chase, 50 Barnes street, Providence, Rhode Island.
6. Rev. Lysander Dickermann, Public Library, Boston, Massachusetts.
*7. Prof. Richard T. Ely, Johns Hopkins University, Baltimore, Maryland.
*8. Prof. Richard H. Gottheil, Columbia College, New York.
9. Rev. J. T. Gracey, 202 Eagle street, Buffalo, New York.
*10. Prof. William R. Harper, Yale University, New Haven, Connecticut.
*11. Prof. James Taft Hatfield, Northwestern University, Naperville, Illinois.
*12. Prof. Paul Haupt, Johns Hopkins University, Baltimore, Maryland.
*13. Mrs. Paul Haupt, Baltimore, Maryland.
*14. Prof. Henry Hyvernat, Catholic University, Brookland, District of Columbia.
15. Prof. A. V. Williams Jackson, Columbia College, New York.
16. Prof. Morris Jastrow, jr., University of Pennsylvania, Philadelphia, Pennsylvania.
17. Dr. Christopher Johnston, jr., Johns Hopkins University, Baltimore, Maryland.
*18. Rev. S. H. Kellogg, D.D., 86 Charles street, Toronto, Canada.
*19. Prof. Charles R. Lanman, Harvard University, Cambridge, Massachusetts.
20. Charles G. Leland, Philadelphia, Pennsylvania.
*21. Mrs. Charles G. Leland, Philadelphia.
*22. Joseph Moore, jr., 1821 Walnut street, Philadelphia, Pennsylvania.
*23. Dr. Ed. Olsson, president University of Dakota, Vermillion, Dakota.
24. E. D. Perry, New York.
*25. Prof. Samuel B. Platner, Adelbert College, Cleveland, Ohio.
26. Prof. Robert W. Rogers, Dickinson College.
*27. Mrs. Karl Rydingsvärd, Boston, Massachusetts.
28. David Sulsberger, 1220 North Twelfth street, Philadelphia, Pennsylvania.
29. Mayer Sulsberger, 1303 Girard avenue, Philadelphia, Pennsylvania.
30. S. M. Swenson, New York.
31. Seymour D. Thomson, St. Louis, Missouri.
32. Dr. William H. Ward, 251 Broadway, New York.
33. Prof. R. F. Weidner, Augustana Theological Seminary, Rock Island, Illinois.
34. Dr. Charles E. West, 138 Montague street, Brooklyn, New York.
35. Captain Whitehouse, 15 Fifth avenue, New York.
36. Prof. W. D. Whitney, Yale University, New Haven, Connecticut.
*37. Prof. Alonzo Williams, Brown University, Providence, Rhode Island.
*38. Prof. Robert D. Wilson, Western Theological School, Allegheny, Pennsylvania. vania.
39. Johns Hopkins University, Baltimore, Maryland.
40. The Newberry Library, Chicago, Illinois.

* Present.

The following table indicates the number of American subscribers and members present at the Eight International Congresses of Orientalists:

	American subscribers.	American present.
1. Paris (1873)	[1] 38	[5] [illegible]
2. London (1874)	[2] 9	([illegible]
3. St. Petersburg (1876)	[3] 7	([illegible]
4. Florence (1878)	[4] 3	[illegible]
5. Berlin (1881)	6	[7] [illegible]
6. Leyden (1883)	8	[8] [illegible]
7. Vienna (1886)	11	[illegible]
8. Stockholm (1889)	40	1[illegible]

This marked increase was no doubt chiefly due to the circulation of a especial American edition of the programme for the Stockholm Congress, published by the Smithsonian Institution at the request of the secretary-general of the congress, Count Landberg. This circular contained a revised English translation of the original programme including additions and corrections especially furnished for this purpose by the secretary-general of the congress. Copies of this circular were sent to all the members of the American Oriental Society as well as to a great many libraries and colleges in this country.

But three American institutions were represented by delegates: Brown University, Providence, Rhode Island, by Prof. Alonzo Williams, and the Smithsonian Institution and the Johns Hopkins University by Prof. Paul Haupt.

It is to be regretted that the American Oriental Society did not sent a delegate to the congress. The sending of a representative and the presentation of a complete set of the journal ot the American Oriéntal Society to the honorary president of the congress would have been appreciated. There was no delegate of the United States Government, nor had England, Germany, or Prussia responded to the invitation to send governmental delegates. The following countries sent such delegates.

Austria,	Coburg-Gotha,	Italy,	Russia,
Baden,	Denmark,	Japan,	Roumania,
Bavaria,	Egypt,	Netherlands,	Saxony,
Bosnia,	France,	Persia,	Siam,
Brazil,	India,	Portugal,	Turkey.

The following universities were represented:

Bombay,	Giessen,	Kasan,	Petersburg,
Brown,	Greifswald,	London,	Prague,
Cambridge,	Halle,	Rund,	Rome,
Copenhagen,	Helsingfors,	Munich,	Upsala,
Edinburgh,	Johns Hopkins,	Oxford,	Vienna.

[1] I may be allowed to mention especially Charles A. Briggs, D. C. Gilman, Professor Henry, Professor Salisbury, A. Van Name, Andrew D. White, W. D. Whitney the American Oriental Society, the Philosophical Society, of Hartford, Connecticut the Smithsonian Institution. The late Dr. Schliemann, too, is registered as one of the American subscribers.

[2] W. D. Whitney, Egbert C. Smyth, General J. M. Read, etc.

[3] W. D. Whitney, E. E. Salisbury, and A. Van Name, of New Haven; G. Atwood and Rev. O. D. Miller, of Boston; S. S. Haldeman, of Philadelphia, and D. C. Gilman of Baltimore.

[4] W. D. Whitney, Prof. W. Benade, Dr. Berend. The latter two were present.

[5] Gen. J. Meredith ReadU. S. consul-general to Paris, Mrs. Read, and the sinologist Charles Reedy.

[6] Not recorded. [7] Peters. [8] F. Brown.

Among the learned societies and institutions represented by delegates may be mentioned the Royal Asiatic Society, the Society of Biblical Archæology, the Palestine Exploration Fund, the India Office of London, the Asiatic Society of Bengal, the Société Asiatique of Paris, the German Oriental Society, the Vatican Library, the Royal Academies of Rome, Turin, Munich, Pesth, etc.

In accordance with the statement in the programme that the patron and honorary president of the congress would be pleased to accept such works as would be deposited for presentation to His Majesty, scholars and institutions all over the world offered more than 3,000 valuable works and serials covering the entire range of oriental studies.

The following works were presented by American institutions and scholars:

1. American Mission Press and American Bible Society, Beirut, Syria. About 50 Arabic publications. (See *Liste des auvrages offerts*, p. 19.)

2. Johns Hopkins University, Baltimore, Maryland.

a. The American Journal of Philology, volumes I–IX, Baltimore, 1878–'89.

b. Johns Hopkins University Circulars, volumes I–VIII, Baltimore, 1878–'89.

c. The Williams Manuscript. Reproduction in phototype of 17 pages of a Syriac MS. containing the Epistles known as "Antilegomena," Baltimore, 1886.

d. Contributions to Assyriology and Comparative Semitic Philology, edited by Friedrich Delitzsch and Paul Haupt, with the coöperation of the Johns Hopkins University, Baltimore, Maryland, volume I, part 1, Leipsic, 1889.

3. The Smithsonian Institution on behalf of the U. S. National Museum:

Assyrian and Babylonian seals, facsimiles and flat impressions, illustrating the method after which the smaller Assyro-Babylonian objects preserved in private American collections are reproduced for the study collection at the U. S. National Museum.

Owing to a mistake of the European Express Company the box containing these objects did not arrive in time to be presented to the king at the general meeting of the congress in Stockholm. At the request of the Smithsonian delegate the United States minister to Sweden and Norway, Gen. W. W. Thomas, submitted the presents to King Oscar at a special audience. The expression of interest by the king on that occasion was conveyed in a letter of General Thomas herewith subjoined:

UNITED STATES LEGATION,
Stockholm, November 12, 1889.

Prof. PAUL HAUPT:

MY DEAR SIR: Your note from on board the steamer was duly received, and some time after the box came to hand.

I might at once have sent the box through the usual official channels to the king, but on opening it I found its contents of such value, and so neatly and orderly arranged and classified that I desired to make sure that His Majesty should see this model gift of the Smithsonian.

The opportunity desired has occurred.

I to-day had an audience of the King.

I took the box with me and had it carried into the audience chamber. In the ante room I unpacked and took out the inner box, and also unpacked some of the little boxes and the ancient seals contained therein. His Majesty himself took up the fine inner box or tray, containing all the small boxes, placed it on his writing desk, read the large general inscription in gold letters on black ground, and examined carefully several of the Assyrian seals and casts, and expressed his admiration at the beauty and clearness of the seals and the skill and method of the arrangement.

The King said he should take time at his leisure to look over the seals more thoroughly, and would then decide where to place the tray. His Majesty also desired me to express his thanks to the Smithsonian Institution for this beautiful and useful gift-as well as his appreciation of the method and skill displayed in the cases and general arrangement.

Congratulating the Smithsonian and yourself, not only upon this present and it gracious reception, but upon the general exhibit made by the United States at the Oriental Congress at Stockholm in seals and books, and last and best, *in men*,

I remain, my dear sir, yours very sincerely,

W. W. THOMAS, JR.

The following works were presented by American Orientalists:

1. Thomas Chase, of Providence, Rhode Island: Hellas, her Monuments and Scener[y], Cambridge, 1883.

2. E. van Dyck, Cairo: Real property, mortgage, and wakf, according to Ottoma[n] law.

3. J. T. Gracey, D D., of Buffalo, New York:
   - *a.* India by J. T. Gracey, Rochester, New York, 1884:
   - *b.* The Gulistán of Sa'di, edited in Persian by A. Sprenger, Calcutta, 1851.

4. Wm. R. Harper, of New Haven, Connecticut:
   - *a.* Elements of Hebrew, tenth ed.
   - *b.* Introductory Hebrew Method Manual, fifth ed.
   - *c.* Hebrew Vocabularies, third ed.
   - *d.* Elements of Hebrew Syntax.
   - *e.* Hebraica, volumes I–V.

5. Paul Haupt, of Baltimore, Maryland:
   - *a.* The Babylonian Nimrod Epic, Leipsic, 1890.
   - *b.* The Cuneiform Account of the Deluge, Leipsic, 1881.
   - *c.* A modern fragment of the old Babylonian Nimrod Epic, containing a lege[nd] of Noah and the demon Kater (inscribed day tablet).
   - *d.* Contributions to Assyriology and Comparative Semitic Philology, part [i], Leipsic, 1889.
   - *e.* On the Semitic Sounds and their Transliteration, Leipsic, 1889.

6. Henry Hyvernat, of Washington, District of Columbia:
   - *a.* Les actes des martyrs de l'Égypte, volume I, Rome, 1887.
   - *b.* Album de paléographie copte.

7. S. H. Kellogg, Western Theological Seminary, Alleghany, Pennsylvania:
   - *a.* A Grammar of the Hindi Language.
   - *b.* The Light of Asia and the Light of the World.

8. Ch. R. Hanman, of Cambridge, Massachusetts, a Sanscrit Reader, Parts I–III, Boston, 1888.

9. Dr. John Wortabet, Beirut:
   - *a* Elements of Anatomy.*
   - *b* Elements of Physiology.*
   - *c* Temples and Tombs of Thebes.*

The Eighth International Congress of Orientalists presented some special feature[s] distinguishing it from all its predecessors compared with the previous meetings.

The Government took an especial interest in the proceedings throughout. Kin[g] Oscar acted as patron and honorary president opened the congress (in the great es[-]cutcheon hall of Riddarhuset, the palace of the Swedish nobility, in Stockholm) with a happily worded French address; closed it with an admirably expressed Lati[n] oration; was in the chair at the general meeting of all the sections, and attended on[e] of the meetings of the Semitic section I*b* for cuneiform research.† At Christiania th[e]

* In Arabic.

† The Congress was organized in five sections; the first of which was divided int[o] two sub-sections.

1. Semitic and Islam.
   - *a* Languages and literatures of Islam.
   - *b* Semitic languages, other than Arabic; cuneiform texts and inscriptions, etc.
2. Aryan.
3. African, including Egyptology.
4. Central Asia and the Far East.
5. Malay and Polynesia.

meeting was opened in the name of the King by the minister of public instruction. The King had offered two prizes (by special decree of January 6, 1886), one for a work on the History of the Semitic Languages, and the other on the Civilization of the Arab Muhammed. No works of European or American Orientalists were submitted to the special committee appointed for the purpose of reporting the recommendation for the award of the prizes. But six Arabic works on the second-named subject by Oriental authors had been sent to the committee, and one of these Oriental scholars, Mahmud Shookree el-Aloasee of Baghdad, though not coming up fully to the requirements, was considered worthy of King Oscar's gold medal for art and science, with the ribbon of the order of Wasa.

Quite a number of native scholars from the East were present and took an active part in the proceedings.

Abdallah Fikri Pasha spoke on the Divan of Hasan Ibn Thâbit.

Sheikh Hamza Fathallah: On the right of women in the Islam.

Mahmud Omar: On Arabic proverbs in Egypt.

Emin Bey Fikri: Against those who prefer modern Arabic to the classical language.

These three papers were in Arabic, the following native Oriental scholars spoke in English:

Jivanji Jamshedji Modi: On the position of the Haoma in the Avesta of the Parsees.

In his opening address the secretary-general called attention to this special feature of the congress, and expressed the hope that this active participation on the part of native Oriental scholars would be the starting point of a new era for the civilization of the East.

A great many of the most distinguished Orientalists from all parts of the globe were present, among them may be mentioned: Brugsch, Bühler, Chwolson, Dillmann, Euting, Ginsburg, de Goeje, Donner, Gubernatis, Guidi, Halévy, Kern, Kremer, Mehren, Max Müller, Oppert, Reinisch, de Rosny, Rost, Sayce, Schefer, Schlegel, V. Schmidt, Spiegel, Weber.

Over a hundred papers of great value were read:

Twenty-four in the Semitic section I*a* (Arabic and Islam).

Twenty-six in T section I*b* (cuneiform research, etc).

Twenty-two in section II (Aryan).

Nineteen in section III (Egyptian, etc.).

The following papers were read by American Orientalists:

a. Prof. Paul Haupt, The Death of Sargon II.

b. Prof. Henry Hyvernat, the palæographical introduction to his Acts of the Martyrs in Egypt.

c Chas. G. Leland: The Pidjin (Chinese-English) dialect and its relation to other mixed dialects, followed by a communication on the dissidence of the Chinese philosophers concerning the question of human nature.

The scientific character of the meeting, however, was somewhat impaired by the almost excessive hospitality of the Scandinavian hosts, and especially by the number of tourists who attracted by the programme attended. It looked occasionally as though the Congress were rather a succession of festivities than a serious gathering of scholars for scientific purposes. It was especially regretted that there was hardly any time for private intercourse between individual fellow-workers. Since the meeting of the Congress some feeling has developed against so great a display of hospitality in the future.

Where the next International Congress of Orientalists is to meet has not yet been determined.

At the general meeting of all the sections held at Stockholm on August 6, under the presidency of King Oscar, it was suggested by the delegate of the Smithsonian Institution (after a special meeting of all the American orientalists present, with the

American minister in the chair) that the Tenth Congress should be held in Americ 1893. The idea seemed to meet with general approval, but it remains to be whether the American orientalists will be ready to extend a formal invitation.

Professor Haupt addressed King Oscar at this occasion as follows:

"I have the honor to present to your majesty the first part of a new publica which is intended to contribute, above all, to the solution of the problem set by majesty, viz, the history of the Semitic languages. The series is entitled Co butions to Assyriology and Comparative Semitic Philology. I submit the first on behalf of the Johns Hopkins University, of Baltimore, with whose co-operatio work is published. I beg leave to add some other publications issued under the spices of the Johns Hopkins University.

"1. The photo lithographic re-production of 17 pages of a Syriac MS.

"2. A complete series of the Johns Hopkins University circulars, which report the development of this new university since the year 1879 and which contai the same time numerous contributions to Oriental research.

"3. The 9 volumes of the American Journal of Philology (published at Baltin under the auspices of the Johns Hopkins University) which contain several im tant articles of our venerable leader in Oriental philology, Professor Whitney, as as papers by other American orientalists, both Indo-European and Semitic.

"I am also instructed as delegate of the Smithsonian Institution to present to majesty on behalf of the U. S. National Museum a number of Babylonian and syrian seals (facsimiles and flat impressions) illustrating the methods after w smaller Assyrian and Babylonian objects preserved in private American collec are reproduced for the study collection of the U. S. National Museum.

"Your majesty will see what interest is had in America in Oriental studies, cially in cuneiform research. There are more instructors in Assyriology now i United States than at all the European universities combined. Also at this gress there are nearly forty American orientalists inscribed as members.

"7. I can not suppress the hope that our European follow-workers in view of the gress of Oriental studies in America will be willing before long to have, perhaps, Tenth International Congress of Orientalists meet in the United States. The tance will hardly deter many. It will, perhaps, be possible to place at the dispos the members a steamer which would carry them to America and back agai Europe. Nor would the attendance at the Congress take much time. Even in there should be 6 days in Washington (or wherever we should agree to meet), follo by an excursion to the West, Chicago, the Lake region, Niagara Falls, and the again, through Boston, New York, Philadelphia, and Baltimore to Washington would be possible to do all that (including the passage across the Atlantic both w in a little more than one month. The gracious interest which your majesty devoted to Oriental studies will always exercise an encouraging influence, an trust that at the meeting of the Congress on American soil we shall not be too behind the older European centers of Oriental learning."

Respectfully submitted.

PAUL HAUP

GENERAL APPENDIX

TO THE

SMITHSONIAN REPORT FOR 1890.

ADVERTISEMENT.

The object of the GENERAL APPENDIX to the Annual Report of the Smithsonian Institution is to furnish brief accounts of scientific discovery in particular directions; occasional reports of the investigations made by collaborators of the Institution; memoirs of a general character or on special topics, whether original and prepared expressly for the purpose, or selected from foreign journals and proceedings; and briefly to present (as fully as space will permit) such papers not published in the Smithsonian Contributions or in the Miscellaneous Collections as may be supposed to be of interest or value to the numerous correspondents of the Institution.

It has been a prominent object of the Board of Regents of the Smithsonian Institution, from a very early date, to enrich the annual report required of them by law, with memoirs illustrating the more remarkable and important developments in physical and biological discovery, as well as showing the general character of the operations of the Institution; and this purpose has, during the greater part of its history, been carried out largely by the publication of such papers as would possess an interest to all attracted by scientific progress.

In 1880 the Secretary, induced in part by the dis-continuance of an annual summary of progress which for thirty years previous had been issued by well-known private publishing firms, had prepared by competent collaborators a series of abstracts, showing concisely the prominent features of recent scientific progress in astronomy, geology, meteorology, physics, chemistry, mineralogy, botany, zoölogy, and anthropology. This latter plan was continued, though not altogether satisfactorily, down to and including the year 1888.

In the report for 1889 a return was made to the earlier method of presenting a miscellaneous selection of papers (some of them original) embracing a considerable range of scientific investigation and discussion. This method has been continued in the present report, for 1890.

THE SQUARING OF THE CIRCLE.

AN HISTORICAL SKETCH OF THE PROBLEM FROM THE EARLIEST TIMES TO THE PRESENT DAY.*

By Hermann Schubert.

I.—UNIVERSAL INTEREST IN THE PROBLEM.

For two and a half thousand years both trained and untrained minds have striven in vain to solve the problem known as the squaring of the circle. Now that geometers have at last succeeded in giving a rigid demonstration of the impossibility of solving the problem with ruler and compasses, it seems fitting and opportune to cast a glance into the nature and history of this very ancient problem. And this will be found all the more justifiable in view of the fact that the squaring of the circle, at least in name, is very widely known outside of the narrow limits of professional mathematicians.

The resolution of the French Academy.—The Proceedings of the French Academy for the year 1775 contain, at page 61, the resolution of the Academy not to examine, from that time on, any so-called solutions of the quadrature of the circle that might be handed in. The Academy was driven to this determination by the overwhelming multitude of professed solutions of the famous problem, which were sent to it every month in the year—solutions which, of course, were an invariable attestation of the ignorance and self-consciousness of their authors, but which suffered collectively from a very important error in mathematics: they were *wrong*. Since that time all professed solutions of the problem received by the Academy find a sure haven in the waste-basket, and remain unanswered for all time. The circle-squarer, however, sees in this high-handed manner of rejection only the envy of the great towards his grand intellectual discovery. He is determined to meet with recognition, and appeals, therefore, to the public. The newspapers must obtain for him the appreciation that scientific societies have denied. And every year the old mathematical sea serpent more than once disports itself in the columns of our papers, that a Mr. N. N., of P. P., has at last solved the problem of the quadrature of the circle.

* From Holtzendorff and Virchow's *Sammlung gemeinverständlicher wissenschaftlicher Vorträge*, Heft 67. Hamburg: Verlagsanstalt, etc. Re-printed from *The Monist*, January, 1891, vol. I, No. 2, pp. 197–228.

General ignorance of quadrators.—But what kind of people are these circle-squarers, when examined by the light? Almost always they will be found to be imperfectly educated persons, whose mathematical knowledge does not exceed that of a modern college freshman. It is seldom that they know accurately what the requirements of the problem are and what its nature. They never know the two and a half thousand years' history of the problem, and they have no idea whatever of the important investigations and results which have been made with reference to the problem by great and real mathematicians in every century down to our time.

A cyclometric type.—Yet great as is the quantum of ignorance that circle-squarers intermix with their intellectual products, the lavish supply of conceit and self-consciousness with which they season their performances is still greater. I have not far to go to furnish a verification of this. A book printed in Hamburg in the year 1840 lies before me, in which the author thanks Almighty God at every second page that He has selected him and no one else to solve the "problem phenomenal" of mathematics, "so long sought for, so fervently desired, and attempted by millions." After the modest author has proclaimed himself the unmasker of Archimedes's deceit, he says: "It thus has pleased our mother nature to withhold this mathematical jewel from the eye of human investigation until she thought it fitting to reveal truth to simplicity."

This will suffice to show the great self-consciousness of the author. But it does not suffice to prove his ignorance. He has no conception of mathematical demonstration; he takes it for granted that things are so because they seem so to him. Errors of logic, also, are abundantly found in his book. But apart from this general incorrectness, let us see wherein the real gist of his fallacy consists. It requires considerable labor to find out what this is from the turgid language and bombastic style in which the author has buried his conclusions. But it is this: The author inscribes a square in a circle, circumscribes another about it, then points out that the inside square is made up of four congruent triangles, whereas the circumscribed square is made up of eight such triangles; from which fact, seeing that the circle is larger than the one square and smaller than the other, he draws the bold conclusion that the circle is equal in area to six such triangles. It is hardly conceivable that a rational being could infer that something which is greater than 4 and less than 8 must necessarily be 6. But with a man that attempts the squaring of the circle this kind of ratiocination *is* possible.

Similarly in the case of all other attempted solutions of the problem, either logical fallacies or violations of elementary arithmetical or geometrical truths may be pointed out. Only they are not always of such a trivial nature as in the book just mentioned.

Let us now inquire whence the inclination arises which leads people to take up the quadrature of the circle and to attempt to solve it.

The allurements of the problem.—Attention must first be called to the antiquity of the problem. A quadrature was attempted in Egypt 500 years before the exodus of the Israelites. Among the Greeks the problem never ceased to play a part that greatly influenced the progress of mathematics. And in the middle ages also the squaring of the circle sporadically appears as the philosopher's stone of mathematics. The problem has thus never ceased to be dealt with and considered. But it is not by the antiquity of the problem that circle-squarers are enticed, but by the allurement which everything exerts that is calculated to raise the individual out of the mass of ordinary humanity, and to bind about his temples the laurel crown of celebrity. It is ambition that spurred men on in ancient Greece and still spurs them on in modern times to crack this primeval mathematical nut. Whether they are competent thereto is a secondary consideration. They look upon the squaring of the circle as the grand prize of a lottery that can just as well fall to their lot as to that of any other. They do not remember that—

> Toil before honor is placed by sagacious decrees of Immortals,

and that it requires years of continued studies to gain possession of the mathematical weapons that are indispensably necessary to attack the problem, but which even in the hands of the most distinguished mathematical strategists have not sufficed to take the stronghold.

About the only problem known to the lay world.—But how is it, we must further ask, that it happens to be the squaring of the circle and not some other unsolved mathematical problem upon which the efforts of people are bestowed who have no knowledge of mathematics yet busy themselves with mathematical questions? The question is answered by the fact that the squaring of the circle is about the only mathematical problem that is known to the unprofessional world—at least by name. Even among the Greeks the problem was very widely known outside of mathematical circles. In the eyes of the Grecian layman, as at present among many of his modern brethren, occupation with this problem was regarded as the most important and essential business of mathematicians. In fact they had a special word to designate this species of activity, namely, τετραγωνίζειν, which means to busy one's self with the quadrature. In modern times, also, every educated person, though he be not a mathematician, knows the problem by name, and knows that it is insolvable, or at least, that despite the efforts of the most famous mathematicians it has not yet been solved. For this reason the phrase "to square the circle," is now used in the sense of attempting the impossible.

Belief that rewards have been offered.—But in addition to the antiquity of the problem, and the fact also that it is known to the lay world, we have yet a third factor to point out that induces people to take up with it. This is the report that has been spread abroad for a hundred years now, that the Academies, the Queen of England, or some other influen-

tial person, has offered a great prize to be given to the one that first solves the problem. As a matter of fact we find the hope of obtaining this large prize of money the principal incitement to action with many circle-squarers. And the author of the book above referred to begs his readers to lend him their assistance in obtaining the prizes offered.

The problem among mathematicians.—Although the opinion is widely current in the unprofessional world that professional mathematicians are still busied with the solution of the problem, this is by no means the case. On the contrary, for some two hundred years, the endeavors of many considerable mathematicians have been solely directed towards demonstrating with exactness that the problem is insolvable. It is, as a rule—and naturally—more difficult to prove that something is impossible than to prove that it is possible. And thus it has happened, that up to within a few years ago, despite the employment of the most varied and the most comprehensive methods of modern mathematics, no one succeeded in supplying the wished-for demonstration of the problem's impossibility. At last, Professor Lindemann, of Königsberg, in June, 1882, succeeded in furnishing a demonstration—and the first demonstration—that it is impossible by the exclusive employment of ruler and compasses to construct a square that is mathematically exactly equal in area to a given circle. The demonstration, naturally, was not effected with the help of the old elementary methods; for if it were, it would surely have been accomplished centuries ago; but methods were requisite that were first furnished by the theory of definite integrals and departments of higher algebra developed in the last decades; in other words, it required the direct and indirect preparatory labor of many centuries to make finally possible a demonstration of the insolvability of this historic problem.

Of course, this demonstration will have no more effect than the resolution of the Paris Academy of 1775 in causing the fecund race of circle squarers to vanish from the face of the earth. In the future as in the past, there will be people who know nothing and will not want to know anything of this demonstration, and who believe that they can not help but succeed in a matter in which others have failed, and that just they have been appointed by Providence to solve the famous puzzle. But unfortunately the ineradicable passion of wanting to solve the quadrature of the circle has also its serious side. Circle-squarers are not always so self-contented as the author of the book we have mentioned. They often see or at least divine the insuperable difficulties that tower up before them, and the conflict between their aspirations and their performances, the consciousness that they want to solve the problem but are unable to solve it, darkens their soul and, lost to the world, they become interesting subjects for the science of psychiatry.

II.—NATURE OF THE PROBLEM.

Numerical rectification.—If we have a circle before us, it is easy for us to determine the length of its radius or of its diameter, which must be double that of the radius; and the question next arises to find the number that represents how many times larger its circumference, that is the length of the circular line, is than its radius or its diameter. From the fact that all circles have the same shape it follows that this proportion will always be the same for both large and small circles. Now, since the time of Archimedes, all civilized nations that have cultivated mathematics have called the number that denotes how many times larger than the diameter the circumference of a circle is, π—the Greek initial letter of the word periphery. To compute π, therefore, means to calculate how many times larger the circumference of a circle is than its diameter. This calculation is called "the numerical rectification of the circle."

The numerical quadrature.—Next to the calculation of the circumference, the calculation of the superficial contents of a circle by means of its radius or diameter is perhaps most important; that is, the computation of how much area that part of a plane which lies within a circle measures. This calculation is called the "numerical quadrature." It depends, however, upon the problem of numerical rectification; that is, upon the calculation of the magnitude of π. For it is demonstrated in elementary geometry that the area of a circle is equal to the area of a triangle produced by drawing in the circle a radius, erecting at the extremity of the same a tangent—that is, in this case a perpendicular—cutting off upon the latter the length of the circumference, measuring from the extremity, and joining the point thus obtained with the center of the circle. But it follows from this that the area of a circle is as many times larger than the square upon its radius as the number π amounts to.

Constructive rectification and quadrature.—The numerical rectification and numerical quadrature of the circle based upon the computation of the number π are to be clearly distinguished from problems that require a straightline equal in length to the circumference of a circle, or a square equal in area to a circle, to be *constructively* produced out of its radius or its diameter; problems which might properly be called "constructive rectification" or "constructive quadrature." Approximately, of course, by employing an approximate value for π these problems are easily solvable. But to solve a problem of construction, in geometry, means to solve it with mathematical exactitude. If the value π were exactly equal to the ratio of two whole numbers to one another, the constructive rectification would present no difficulties. For example, suppose the circumference of a circle were exactly $3\frac{1}{7}$ times greater than its diameter; then the diameter could be divided into seven equal parts, which could be easily done by the principles of planimetry with ruler and

compasses; then we would produce to the amount of such a part a straight line exactly three times larger than the diameter, and should thus obtain a straight line exactly equal to the circumference of the circle. But as a matter of fact, and as has actually been demonstrated, there do not exist two whole numbers, be they ever so great, that exactly represent by their proportion to one another the number π. Consequently, a rectification of the kind just described does not attain the object desired.

It might be asked here, whether from the demonstrated fact that the number π is not equal to the ratio of two whole numbers however great, it does not immediately follow that it is impossible to construct a straight line exactly equal in length to the circumference of a circle; thus demonstrating at once the impossibility of solving the problem. This question is to be answered in the negative. For there are in geometry many sets of two lines of which the one can be easily constructed from the other, notwithstanding the fact that no two whole numbers can be found to represent the ratio of the two lines. The side and the diagonal of a square, for instance, are so constituted. It is true the ratio of the latter two magnitudes is nearly that of 5 to 7. But this proportion is not exact, and there are in fact no two numbers that represent the ratio exactly. Nevertheless, either of these two lines can be easily constructed from the other by the sole employment of ruler and compasses. This might be the case, too, with the rectification of the circle; and consequently from the impossibility of representing π by the ratio between two whole numbers the impossibility of the problem of rectification is not inferable.

The quadrature of the circle stands and falls with the problem of rectification. This is based upon the truth above mentioned, that a circle is equal in area to a right-angle triangle, in which one side is equal to the radius of the circle and the other to the circumference. Supposing, accordingly, that the circumference of the circle were rectified, then we could construct this triangle. But every triangle, as is taught in the elements of planimetry, can, with the help of ruler and compasses, be converted into a square exactly equal to it in area. So that, therefore, supposing the rectification of the circumference of a circle were successfully performed, a square could be constructed that would be exactly equal in area to the circle.

The dependence upon one another of the three problems of the computation of the number π, of the quadrature of the circle, and its rectification, thus obliges us, in dealing with the history of the quadrature, to regard investigations with respect to the value of π, and attempts to rectify the circle as of equal importance, and to consider them accordingly.

Conditions of the geometrical solution.—We have used repeatedly in the course of the discussion the expression "to construct with ruler and

compasses." It will be necessary to explain what is meant by the specification of these two instruments. When such a number of conditions is annexed to a requirement in geometry to construct a certain figure that the construction only of *one* figure or a limited number of figures is possible in accordance with the conditions given, such a complete requirement is called a problem of construction, or briefly a problem. When a problem of this kind is presented for solution it is necessary to reduce it to simpler problems, already recognized as solvable; and since these latter depend in their turn upon other still simpler problems, we are finally brought back to certain fundamental problems, upon which the rest are based but which are not themselves reducible to problems less simple. These fundamental problems are, so to speak, the undermost stones of the edifice of geometrical construction. The question next arises as to what problems may be properly regarded as fundamental; and it has been found that the solution of a great part of the problems that arise in elementary planimetry rests upon the solution of only five original problems. They are:

(1) The construction of a straight line which shall pass through two given points.

(2) The construction of a circle the center of which is a given point and the radius of which has a given length.

(3) The determination of the point that lies coincidently on two given straight lines extended as far as is necessary—in case such a point (point of intersection) exists.

(4) The determination of the two points that lie coincidently on a given straight line and a given circle—in case such common points (points of intersection) exist.

(5) The determination of the two points that lie coincidently on two given circles—in case such common points (points of intersection) exist.

For the solution of the three last of these five problems the eye alone is needed, while for the solution of the two first problems, besides pencil, ink, chalk, and the like, additional special instruments are required: for the solution of the first problem a ruler is most generally used, and for the solution of the second a pair of compasses. But it must be remembered that it is no concern of geometry what mechanical instruments are employed in the solution of the five problems mentioned. Geometry simply limits itself to the pre-supposition that these problems are solvable and regards a complicated problem as solved if, upon a specification of the constructions of which the solution consists, no other requirements are demanded than the five above mentioned. Since, accordingly, geometry does not itself furnish the solution of these five problems, but rather exacts them, they are termed *postulates*.* All

* Usually geometers mention only two postulates (Nos. 1 and 2). But since to geometry proper it is indifferent whether only the eye, or additional special mechanical instruments are necessary, the author has regarded it more correct in point of method to assume five postulates.

problems of planimetry are not reducible to these five problems alone. There are problems that can be solved only by assuming other problems as solvable which are not included in the five given: for example, the construction of an ellipse, having given its center and its major and minor axes. Many problems, however, possess the property of being solvable with the assistance solely of the five postulates above formulated, and where this is the case they are said to be "constructible with ruler and compasses," or "elementarily" constructible.

After these general remarks upon the solvability of problems of geometrical construction, which an understanding of the history of the squaring of the circle makes indispensably necessary, the significance of the question whether the quadrature of the circle is or is not solvable, that is, elementarily solvable, will become intelligible. But the conception just discussed of elementary solvability only gradually took clear form, and we therefore find among the Greeks as well as among the Arabs, endeavors, successful in some respects, that aimed at solving the quadrature of the circle with other expedients than the five postulates. We have also to take these endeavors into consideration, and especially so as they, no less than the unsuccessful efforts at elementary solution, have upon the whole advanced the science of geometry and contributed much to the clarification of geometrical ideas.

III.—HISTORICAL ATTEMPTS.

The Egyptian Quadrature.—In the oldest mathematical work that we possess we find a rule that tells us how to make a square which is equal in area to a given circle. This celebrated book, the Papyrus Rhind of the British Museum, translated and explained by Eisenlohr (Leipsic, 1887), was written, as it is stated in the work, in the thirty-third year of the reign of King Ra-a-us, by a scribe of that monarch, named Ahmes. The composition of the work falls accordingly into the period of the two Hiksos dynasties, that is, in the period between 2000 and 1700 B. C. But there is another important circumstance attached to this. Ahmes mentions in his introduction that he composed his work after the model of old treatises, written in the time of King Raenmat; whence it appears that the originals of the mathematical expositions of Ahmes, are half a thousand years older yet than the Papyrus Rhind.

The rule given in this papyrus for obtaining a square equal to a circle, specifies that the diameter of the circle shall be shortened one-ninth of its length and upon the shortened line thus obtained a square erected. Of course, the area of a square of this construction is only approximately equal to the area of the circle. An idea may be obtained of the degree of exactness of this original, primitive quadrature by our remarking that if the diameter of the circle in question is one metre in length, the square that is supposed to be equal to the circle is a little less than half a square decimetre larger; an approximation not so accurate as that computed by Archimedes, yet much more correct than

many a one later employed. It is not known how Ahmes or his predecessors arrived at this approximate quadrature; but it is certain that it was handed down in Egypt from century to century, and in late Egyptian times it repeatedly appears.

The Biblical and Babylonian quadratures.—Besides among the Egyptians we also find in pre-Grecian antiquity an attempt at circle-computation among the Babylonians. This is not a quadrature; but aims at the rectification of the circumference. The Bablyonian mathematicians had discovered that if the radius of a circle be successively inscribed as chord within its circumference, after the sixth inscription we arrive at the point of departure, and they concluded from this that the circumference of a circle must be a little larger than a line which is six times as long as the radius, that is, three times as long as the diameter. A trace of this Babylonian method of computation may even be found in the Bible; for in I Kings vii, 23, and II Chron. iv, 2, the great laver is described, which under the name of the "molten sea" constituted an ornament of the Temple of Solomon; and it is said of this vessel that it measured 10 cubits from brim to brim, and 30 cubits roundabout. The number 3 as the ratio between the circumference and the diameter is still more plainly given in the Talmud, where we read that "that which measures three lengths in circumference is one length across."

Among the Greeks.—With regard to the earlier Greek mathematicians—as Thales and Pythagoras—we know that they acquired the foundations of their mathematical knowledge in Egypt. But nothing has been handed down to us which shows that they knew of the old Egyptian quadrature, or that they dealt with the problem at all. But tradition says that subsequently the teacher of Euripides and Pericles, the great philosopher and mathematician Anaxagoras, whom Plato so highly praised, "drew the quadrature of the circle" in prison, in the year 434. This is the account of Plutarch in the seventeenth chapter of his work "De Exilio."

Anaxagoras.—The method is not told us in which Anaxagoras had supposably solved the problem, and it is not said whether knowingly or unknowingly he accomplished an approximate solution after the manner of Ahmes. But at any rate, to Anaxagoras belongs the merit of having called attention to a problem that bore great fruit, in having incited Grecian scholars to busy themselves with geometry, and thus more and more to advance that science.

The quadratrix of Hippias of Elis.—Again, it is reported that the mathematician Hippias of Elis invented a curved line that could be made to serve a double purpose; first, to trisect an angle, and, second, to square the circle. This curved line is the τετραγωνίζουσα so often mentioned by the later Greek mathematicians, and by the Romans, called "quadratrix." Regarding the nature of this curve we have exact knowledge from Pappus. But it will be sufficient, here, to state that the *quadratrix* is not a circle nor a portion of a circle, so that its construc-

tion is not possible by means of the postulates enumerated in the preceding section. And therefore the solution of the quadrature of the circle founded on the construction of the *quadratrix* is not an elementary solution in the sense discussed in the last section. We can, it is true, conceive a mechanism that will draw this curve as well as compasses draw a circle; and with the assistance of a mechanism of this description the squaring of the circle is solvable with exactitude. But if it be allowed to employ in a solution an apparatus especially adapted thereto, every problem may be said to be solvable. Strictly taken, the invention of the curve of Hippias substitutes for one insuperable difficulty another equally insuperable. Sometime afterwards, about the year 350, the mathematician Dinostratus showed that the *quadratrix* could also be used to solve the problem of rectification, and from that time on this problem plays almost the same rôle in Grecian mathematics as the related problem of quadrature.

The Sophists' solution.—As these problems gradually became known to the non-mathematicians of Greece, attempts at solution at once sprang up that are worthy of a place by the side of the solutions of modern amateur circle-squarers. The Sophists, especially, believed themselves competent by seductive dialectic to take a stronghold that had defied the intellectual onslaughts of the greatest mathematicians. With verbal nicety, amounting to puerility, it was said that the squaring of the circle depended upon the finding of a number which represented in itself both a square and a circle; a square by being a square number, a circle in that it ended with the same number as the root number from which, by multiplication with itself, it was produced. The number 36, accordingly, was, as they thought, the one that embodied the solution of the famous problem.

Contrasted with this twisting of words the speculations of Bryson and Antiphon, both contemporaries of Socrates, though inexact, appear in high degree intelligent.

Antiphon's attempt.—Antiphon divided the circle into four equal arcs, and by joining the points of division obtained a square; he then divided each arc again into two equal parts and thus obtained an inscribed octagon; thence he constructed an inscribed dodecagon, and perceived that the figure so inscribed more and more approached the shape of a circle. In this way, he said, one should proceed, until there was inscribed in the circle a polygon whose sides by reason of their smallness should coincide with the circle. Now this polygon could, by methods already taught by the Pythagoreans, be converted into a square of equal area; and upon the basis of this fact Antiphon regarded the squaring of the circle as solved.

Nothing can be said against this method except that, however far the bisection of the arcs is carried, the result must still remain an approximate one.

Bryson of Heraklea.—The attempt of Bryson of Heraklea was better still; for this scholar did not rest content with finding a square that was

very little smaller than the circle, but obtained by means of circumscribed polygons another square that was very little larger than the circle. Only Bryson committed the error of believing that the area of the circle was the arithmetical mean between an inscribed and a circumscribed polygon of an equal number of sides. Notwithstanding this error, however, to Bryson belongs the merit, first, of having introduced into mathematics by his emphasis of the necessity of a square which was too large and one which was too small, the conception of maximum and minimum "limits" in approximations; and secondly, by his comparison with a circle of the inscribed and circumscribed regular polygons, the merit of having indicated to Archimedes the way by which an approximate value for π was to be reached.

Hippocrates of Chios.—Not long after Antiphon and Bryson, Hippocrates of Chios treated the problem, which had now become more and more famous, from a new point of view. Hippocrates was not satisfied with approximate equalities, and searched for curvilinearly bounded plane figures which should be mathematically equal to a rectilinearly bounded figure, and therefore could be converted by ruler and compasses into a square equal in area. First, Hippocrates found that the crescent-shaped plane figure produced by drawing two perpendicular radii in a circle and describing upon the line joining their extremities a semicircle, is exactly equal in area to the triangle that is formed by this line of junction and the two radii; and upon the basis of this fact the endeavors of the untiring scholar were directed towards converting a circle into a crescent. Naturally he was unable to attain this object, but by his efforts to this end he discovered many a new geometrical truth; among others the generalized form of the theorem mentioned, which bears to the present day the name of *Lunulæ Hippocratis*, the lunes of Hippocrates. Thus it appears, in the case of Hippocrates, in the plainest light, how the very insolvable problems of science are qualified to advance science; in that they incite investigators to devote themselves with persistence to its study and thus to fathom its depths.

Euclid's avoidance of the problem.—Following Hippocrates in the historical line of the great Grecian geometricians comes the systematist Euclid, whose rigid formulation of geometrical principles has remained the standard presentation down to the present century. The Elements of Euclid, however, contain nothing relating to the quadrature of the circle or to circle-computation. Comparisons of surfaces which relate to the circle are indeed found in the book, but nowhere a computation of the circumference of a circle or of the area of the circle. This palpable gap in Euclid's system was filled by Archimedes, the greatest mathematician of antiquity.

Archimedes's calculations.—Achimedes was born in Syracuse in the year 287 B. C., and devoted his life, there spent, to the mathematical and the physical sciences which he enriched with invaluable contributions. He lived in Syracuse till the taking of the town by Marcellus, in the year

212 B. C. when he fell by the hand of a Roman soldier whom he had forbidden to destroy the figures he had drawn in the sand. To the greatest performances of Archimedes the successful computation of the number π unquestionably belong. Like Bryson he started with regular inscribed and circumscribed polygons. He showed how it was possible, beginning with the perimeter of an inscribed hexagon, which is equal to six radii, to obtain by way of calculation the perimeter of a regular dodecagon, and then the perimeter of a figure having double the number of sides of the preceding one. Treating, then, the circumscribed polygons in a similar manner, and proceeding with both series of polygons up to a regular 96-sided polygon, he perceived on the one hand that the ratio of the perimeter of the inscribed 96-sided polygon to the diameter was greater than $6336:2017\frac{1}{4}$, and on the other hand, that the corresponding ratio with respect to the circumscribed 96-sided polygon was smaller than $14688:4673\frac{1}{2}$. He inferred from this, that the number π, the ratio of the circumference to the diameter, was greater than the fraction $\frac{6336}{2017\frac{1}{4}}$ and smaller than $\frac{14688}{4673\frac{1}{2}}$. Reducing the two limits thus found for the value of π, Archimedes then showed that the first fraction was greater than $3\frac{10}{71}$, and that the second fraction was smaller than $3\frac{1}{7}$, whence it followed with certainty that the value sought for π lay between $3\frac{1}{7}$ and $3\frac{10}{71}$. The larger of these two approximate values is the only one usually learned and employed. That which fills us most with astonishment in the Archimedean computation of π, is, first, the great acumen and accuracy displayed in all the details of the computation, and then the unwearied perserverance that he must have exercised in calculating the limits of π without the advantages of the Arabian system of numerals and of the decimal notation. For it must be considered that at many stages of the computation what we call the extraction of roots was necessary, and that Archimedes could only by extremely tedious calculations obtain ratios that expressed approximately the roots of given numbers and fractions.

The later mathematicians of Greece.—With regard to the mathematicians of Greece that follow Archimedes, all refer to and employ the approximate value of $3\frac{1}{7}$ for π, without however contributing anything essentially new or additional to the problems of quadrature and of cyclometry. Thus Heron of Alexandria, the father of surveying, who flourished about the year 100 B. C., employs for purposes of practical measurement sometimes the value $3\frac{1}{7}$ for π and sometimes even the rougher approximation $\pi=3$. The astronomer Ptolemy, who lived in Alexandria about the year 150 A. D., and who was famous as being the author of the planetary system universally recognized as correct down to the time of Copernicus, was the only one who furnished a more exact value; this he designated, in the sexigesimal system of fractional notation which he employed, by 3, 8, 30—that is 3 and $\frac{8}{60}$ and $\frac{30}{3600}$, or as we now say 3 degrees 8 minutes (*partes minutæ primæ*) and 30 seconds (*partes minutæ secundæ*). As

a matter of fact, the expression $3 + \frac{8}{60} + \frac{30}{3600} = 3\frac{17}{120}$ represents the number π more exactly than $3\frac{1}{7}$; but on the other hand is, by reason of the magnitude of the numbers 17 and 120 as compared with the numbers 1 and 7, more cumbersome.

Among the Romans.—In the mathematical sciences, more than in any other, the Romans stood upon the shoulders of the Greeks. Indeed, with respect to cyclometry, they not only did not add anything to the Grecian discoveries, but often evinced even that they either did not know of the beautiful result obtained by Archimedes or at least did not know how to appreciate it. For instance, Vitruvius, who lived during the time of Augustus, computed that a wheel 4 feet in diameter must measure $12\frac{1}{2}$ feet in circumference; in other words, he made π equal to $3\frac{1}{8}$. And, similarly, a treatise on surveying, preserved to us in the Gudian manuscript of the library at Wolfenbüttel, contains the following instructions to square the circle: Divide the circumference of a circle into four parts and make one part the side of a square; this square will be equal in area to the circle. Aside from the fact that the rectification of the arc of a circle is requisite to the construction of a square of this kind, the Roman quadrature, viewed as a calculation, is more inexact even than any other computation; for its result is that $\pi = 4$.

Among the Hindus.—The mathematical performances of the Hindus were not only greater than those of the Romans, but in certain directions even surpassed those of the Greeks. In the most ancient source for the mathematics of India that we know of, the Culvasûtras, which date back to a little before our chronological era, we do not find, it is true, the squaring of the circle treated of, but the opposite problem is dealt with which might fittingly be termed the circling of the square. The half of the side of a given square is prolonged one-third of the excess in length of half the diagonal over half the side, and the line thus obtained is taken as the radius of the circle equal in area to the square. The simplest way to obtain an idea of the exactness of this construction is to compute how great π would have to be if the construction were exactly correct. We find out in this way that the value of π, upon which the Indian circling of the square is based, is about from five to six hundredths smaller than the true value, whereas the approximate π of Archimedes, $3\frac{1}{7}$, is only from one to two thousandths too large, and the old Egyptian value exceeds the true value by from one to two hundredths. Cyclometry very probably made great advances among the Hindus in the first four or five centuries of our era; for Aryabhatta, who lived about the year 500 after Christ, states that the ratio of the circumference to the diameter is $62832 \div 20000$, an approximation that in exactness surpasses even that of Ptolemy. The Hindu result gives 3·1416 for π, while π really lies between 3·141592 and 3·141593. How the Hindus obtained this excellent approximate value is told by Ganeça, the commentator of Bhâskara, an author of the twelfth century. Ganeça says that the method of Archi-

medes was carried still farther by the Hindu mathematicians; that by continually doubling the number of sides they proceeded from the hexagon to a polygon of 384 sides, and that by the comparison of the circumferences of the inscribed and circumscribed 384-sided polygons they found that π was equal to $3927 \div 1250$. It will be seen that the value given by Bhâskara is identical with the value of Aryabhatta. It is further worthy of remark that the earlier of these two Hindu mathematicians does not mention either the value $3\frac{1}{7}$ of Archimedes or the value $3\frac{17}{120}$ of Ptolemy, but that the later knows of both values and especially recommends that of Archimedes as the most useful one for practical application. Strange to say, the good approximate value of Arybhatta does not occur in Bramagupta, the great Hindu mathematician who flourished in the beginning of the seventh century; but we find the curious information in this author that the area of a circle is exactly equal to the square root of 10 when the radius is unity. The value of π as derivable from this formula (a value from two to three hundredths too large) has unquestionably arisen upon Hindu soil, for it occurs in no Grecian mathematician; and Arabian authors, who were in a better position than we to know Greek and Hindu mathematical literature, declare that the approximation which makes π equal to the square root of 10 is of Hindu origin. It is possible that the Hindu people, who were addicted more than any other to numeral mysticism, sought to find in this approximation some connection with the fact that man has ten fingers; and ten accordingly is the basis of their numeral system.

Reviewing the achievements of the Hindus generally with respect to the problem of the quadrature, we are brought to recognize that this people, whose talents lay more in the line of arithmetical computation than in the perception of spatial relations, accomplished as good as nothing on the pure geometrical side of the problem, but that the merit belongs to them of having carried the Archimedean method of computing π several stages farther, and of having obtained in this way a much more exact value for it;—a circumstance that is explainable when we consider that the Hindus are the inventors of our present system of numeral notation, possessing which they easily outdid Archimedes, who employed the awkward Greek system.

Among the Chinese.—With regard to the Chinese, this people operated in ancient times with the Babylonian value for π, or 3, but possessed knowledge of the approximate value of Archimedes, at least since the end of the sixth century. Besides this, there appears in a number of Chinese mathematical treatises an approximate value peculiarly their own, in which $\pi = 3\frac{7}{50}$; a value, however, which, notwithstanding it is written in large figures, is no better than that of Archimedes. Attempts at the *constructive* quadrature of the circle are not found among the Chinese.

Among the Arabs.—Greater were the merits of the Arabians in the advancement and development of mathematics, and especially in virtue of the fact that they preserved from oblivion both Greek and Hindu mathematics, and handed them down to the Christian countries of the West. The Arabians expressly distinguished between the Archimedean approximate value and the two Hindu values, the square root of 10 and the ratio 62832: 20000. This distinction occurs also in Muhammed Ibn Musa Alchwarizmî, the same scholar who in the beginning of the ninth century brought the principles of our present system of numerical notation from India and introduced the same into the Mohammedan world. The Arabians however studied not only the numerical quadrature of the circle, but also the constructive; as, for instance, Ibn Albaitam, who lived in Egypt about the year 1000, and whose treatise upon the squaring of the circle is preserved in a Vatican codex, which has unfortunately not yet been edited.

In Christian times.—Christian civilization, to which we are now about to pass, produced up to the second half of the fifteenth century extremely insignificant results in mathematics. Even with regard to our present problem we have but a single important work to mention—the work, namely, of Frankos Von Lüttich upon the squaring of the circle, published in six books, but only preserved in fragments. The author, who lived in the first half of the eleventh century, was probably a pupil of Pope Sylvester II, himself a not inconsiderable mathematician for his time, and who also wrote the most celebrated book on geometry of the period.

Cardinal Nicolaus de Cusa.—Greater interest came to be bestowed upon mathematics in general, but especially on the problem of the quadrature of the circle, in the second half of the fifteenth century, when the sciences again began to revive. This interest was especially aroused by Cardinal Nicolaus De Cusa, a man highly esteemed on account of his astronomical and calendarial studies. He claimed to have discovered the quadrature of the circle by the employment solely of compasses and ruler, and thus attracted the attention of scholars to the now historic problem. People believed the famous cardinal and marvelled at his wisdom, until Regiomontanus, in letters which he wrote in 1464 and 1465, and which were published in 1533, rigidly demonstrated that the cardinal's quadrature was incorrect. The construction of Cusa was as follows: The radius of a circle is prolonged a distance equal to the side of the inscribed square; the line thus obtained is taken as the diameter of a second circle, and in the latter an equilateral triangle is described; then the perimeter of the latter is equal to the circumference of the original circle. If this construction, which its inventor regarded as exact, be considered as a construction of approximation, it will be found to be more inexact even than the construction resulting from the value $\pi = 3\frac{1}{7}$. For by Cusa's method π would be from five to six thousandths smaller than it really is.

Bovillius, and Orontius Finæus.—In the beginning of the sixteenth century a certain Bovillius appears, who announced anew the construction of Cusa, meeting, however, with no notice. But about the middle of the sixteenth century a book was published which the scholars of the time at first received with interest. It bore the proud title "*De Rebus Mathematicis Hactenus Desideratis.*" Its author, Orontius Finæus, represented that he had overcome all the difficulties that had ever stood in the way of geometrical investigators; and incidentally he also communicated to the world the "true quadrature" of the circle. His fame was short-lived. For afterwards, in a book entitled "*De Erratis Orontii,*" the Portuguese Petrus Nonius demonstrated that Orontius's quadrature, like most of his other professed discoveries, was incorrect.

Simon Van Eyck.—In the period following this the number of circle-squarers so increased that we shall have to limit ourselves to those whom mathematicians recognize. And particularly is Simon Van Eyck to be mentioned, who towards the close of the sixteenth century published a quadrature which was so approximate that the value of π derived from it was more exact than that of Archimedes; and to disprove it the mathematician Peter Metius was obliged to seek a still more accurate value than $3\frac{1}{7}$. The erroneous quadrature of Van Eyck was thus the occasion of Metius's discovery that the ratio 355 : 113, or $3\frac{16}{113}$, varied from the true value of π by less than one one-millionth, eclipsing accordingly all values hitherto obtained. Moreover it is demonstrable by the theory of continued fractions that, admitting figures to four places only, no two numbers more exactly represent the value of π than 355 and 113.

Joseph Scaliger.—In the same way the quadrature of the great philologist, Joseph Scaliger, led to refutations. Like most circle-squarers who believe in their discovery, Scaliger also was little versed in the elements of geometry. He solved, however—at least in his own opinion he did—the famous problem; and published in 1592 a book upon it, which bore the pretentious title "*Nova Cyclometria,*" and in which the name of Archimedes was derided. The worthlessness of his supposed discovery was demonstrated to him by the greatest mathematicians of his time, namely, Vieta, Adrianus Romanus, and Clavius.

Longomontanus, John Porta, and Gregory of St. Vincent.—Of the erring circle-squarers that flourished before the middle of the seventeenth century three others deserve particular mention;—Longomontanus of Copenhagen, who rendered such great services to astronomy, the Neapolitan John Porta, and Gregory of St. Vincent. Longomontanus made $\pi = 3\frac{14185}{100000}$, and was so convinced of the correctness of his result that he thanked God fervently, in the preface to his work "*Inventio Quadraturæ Circuli,*" that He had granted him in his high old age the strength to conquer the celebrated difficulty. John Porta followed the initiative of Hippocrates, and believed he had solved the problem by

the comparison of lunes. Gregory of St. Vincent published a quadrature the error of which was very hard to detect, but was finally discovered by Descartes.

Peter Metius, and Vieta.—Of the famous mathematicians who dealt with our problem in the period between the close of the fifteenth century and the time of Newton, we first meet with Peter Metius, before mentioned, who succeeded in finding in the fraction 355 : 113 the best approximate value for π involving only small numbers. The problem received a different advancement at the hands of the famous mathematician Vieta. Vieta was the first to whom the idea occurred of representing π with mathematical exactness by an infinite series of continuable operations. By comparison of inscribed and circumscribed polygons, Vieta found that we approach nearer and nearer to π if we allow the operations of the extraction of the square root of $\frac{1}{2}$ and of addition and of multiplication to succeed each other in a certain manner, and that π must come out exactly if this series of operations could be indefinitely continued. Vieta thus found that to a diameter of 10,000 million units a circumference belongs of 31,415 million, and from 926,535 to 926,536 units of the same length.

Adrianus Romanus, Ludolf Van Ceulen.—But Vieta was outdone by the Netherlander Adrianus Romanus, who added five additional decimal places to the ten of Vieta. To accomplish this he computed with unspeakable labor the circumference of a regular circumscribed polygon of 1,073,741,824 sides. This number is the thirtieth power of 2. Yet great as the labor of Adrianus Romanus was, that of Ludolf Van Ceulen was still greater, for the latter calculator succeeded in carrying the Archimedean process of approximation for the value of π to 35 decimal places, that is, the deviation from the true value was smaller than one one thousand quintillionth, a degree of exactness that we can hardly have any conception of. Ludolf published the figures of the tremendous computation that led to this result. His calculation was carefully examined by the mathematician Griemberger and declared to be correct. Ludolf was justly proud of his work, and, following the example of Archimedes, requested in his will that the result of his most important mathematical performance, the computation of π to 35 decimal places, be engraved upon his tombstone, a request which is said to have been carried out. In honor of Ludolf, π is called to-day in Germany the Ludolfian number.

The new method of Snell. Huygens's verification of it.—Although through the labor of Ludolf a degree of exactness for cyclometrical operations was now obtained that was more than sufficient for any practical purpose that could ever arise, neither the problem of constructive rectification nor that of constructive quadrature was thereby in any respect theoretically advanced. The investigations conducted by the famous mathematicians and physicists Huygens and Snell, about the middle of the seventeenth century, were more important from a mathe-

matical point of view than the work of Ludolf. In his book *Cyclometricus* Snell took the position that the method of comparison of polygons, which originated with Archimedes and was employed by Ludolf, need by no means be the best method of attaining the end sought; and he succeeded, by the employment of propositions which state that certain arcs of a circle are greater or smaller than certain straight lines connected with the circle, in obtaining methods that make it possible to reach results like the Ludolfian with much less labor of calculation. The beautiful theorems of Snell were proved a second time, and better proved, by the celebrated Dutch promoter of the science of optics, Huygens (*Opera Varia*, pp. 365 et seq.; *Theoremata De Circuli et Hyperbolae Quadratura*, 1651), as well as perfected in many ways. Snell and Huygens were fully aware that they had advanced only the problem of numerical quadrature, and not that of the constructive quadrature. This, in Huygens's case, plainly appeared from the vehement dispute he conducted with the English mathematician, James Gregory. This controversy has some significance for the history of our problem, from the fact that Gregory made the first attempt to prove that the squaring of the circle with ruler and compasses must be impossible.

The controversy between Huygens and Gregory.—The result of the controversy, to which we owe many valuable treatises, was that Huygens finally demonstrated in an incontrovertible manner the incorrectness of Gregory's proof of impossibility, adding that he also was of opinion that the solution of the problem with ruler and compasses was impossible, but nevertheless was not himself able to demonstrate this fact. And Newton, later, expressed himself to a similar effect. As a matter of fact it took till the most recent period, that is over 200 years, until higher mathematics was far enough advanced, to furnish a rigid demonstration of impossibility.

Before we proceed to consider the promotive influence which the invention of the differential and the integral calculus had upon our problem, we shall enumerate a few at least of that never-ending line of mistaken quadrators who have delighted the world by the fruits of their ingenuity from the time of Newton to the present period; and out of a pious and sincere consideration for the contemporary world, we shall entirely omit in this to speak of the circle-squarers of our own time.

Hobbes's quadrature.—First to be mentioned is the celebrated English philosopher Hobbes. In his book, *De Problematis Physicis*, in which he chiefly proposes to explain the phenomena of gravity and of ocean tides, he also takes up the quadrature of the circle and gives a very trivial construction that in his opinion definitively solved the problem, making $\pi=3\frac{1}{5}$. In view of Hobbes's importance as a philosopher, two mathematicians, Huygens and Wallis, thought it proper to refute

Hobbes at length. But Hobbes defended his position in a special treatise, in which, to sustain at least the appearance of being right, he disputed the fundamental principles of geometry and the theorem of Pythagoras; so that mathematicians could pass on from him to the order of the day.

French quadrators of the eighteenth century.—In the last century France especially was rich in circle-squarers. We will mention: Oliver de Serres, who by means of a pair of scales determined that a circle weighed as much as the square upon the side of the equilateral triangle inscribed in it, that therefore they must have the same area, an experiment in which $\pi = 3$; Mathulon, who offered in legal form a reward of a thousand dollars to the person who would point out an error in his solution of the problem, and who was actually compelled by the courts to pay the money; Basselin, who believed that his quadrature must be right because it agreed with the approximate value of Archimedes, and who anathematized his ungrateful contemporaries, in the confidence that he would be recognized by posterity; Liger, who proved that a part is greater than the whole, and to whom therefore the quadrature of the circle was child's play; Clerget, who based his solution upon the principle that a circle is a polygon of a definite number of sides, and who calculated, also, among other things, how large the point is at which two circles touch.

Germany and Poland.—Germany and Poland also furnish their contingent to the army of circle-squarers. Lieutenant-Colonel Corsonich produced a quadrature in which π equaled $3\frac{1}{8}$, and promised 50 ducats to the person who could prove that it was incorrect. Hesse, of Berlin, wrote an arithmetic in 1776, in which a true quadrature was also "made known," π being exactly equal to $3\frac{14}{99}$. About the same time Professor Bischoff, of Stettin, defended a quadrature previously published by Captain Leistner, preacher Merkel, and schoolmaster Böhm, which made π *implicite* equal to the square of $\frac{62}{35}$ not even attaining the approximation of Archimedes.

Constructive approximations—Euler, Kocahnsky.—From attempts of this character are to be clearly distinguished constructions of approximation in which the inventor is aware that he has not found a mathematically exact construction, but only an approximate one. The value of such a construction will depend upon two things—first, upon the degree of exactness with which it is numerically expressed, and secondly on the fact whether the construction can be more or less easily made with ruler and compasses. Constructions of this kind, simple in form and yet sufficiently exact for practical purposes, have for centuries been furnished us in great numbers. The great mathematician, Euler, who died in 1783, did not think it out of place to attempt an approximate construction of this kind. A very simple construction for the rectification of the circle, and one which has passed

into many geometrical text books, is that published by Kochansky in 1685, in the Leipziger Berichte. It is as follows:

Erect upon the diameter of a circle at its extremities perpendiculars; with the center as vertex, mark off upon the diameter an angle of 30°; find the point of intersection with the perpendicular of the line last drawn, and join this point of intersection with that point upon the other perpendicular, which is at a distance of three radii from the base of the perpendicular. The line of junction thus obtained is then very approximately equal to one-half of the circumference of the given circle.

Calculation shows that the difference between the true length of the circumference and the line thus constructed is less than $\frac{3}{100000}$ of the diameter.

Inutility of constructive approximations.—Although such constructions of approximation are very interesting in themselves, they nevertheless play but a subordinate rôle in the history of the squaring of the circle; for on the one hand they can never furnish greater exactness for circle computation than the thirty-five decimal places which Ludolf found, and on the other hand they are not adapted to advance in any way the question whether the exact quadrature of the circle with ruler and compasses is possible.

The researches of Newton, Leibnitz, Wallis, and Brouncker.—The numerical side of the problem, however, was considerably advanced by the new mathematical methods perfected by Newton and Leibnitz, commonly called the differential and the integral calculus. And about the middle of the seventeenth century, some time before Newton and Leibnitz represented π by series of powers, the English mathematicians Wallis and Lord Brouncker, Newton's predecessors in a certain sense, succeeded in representing π by an infinite series of figures combined by the first four rules of arithmetic. A new method of computation was thus opened. Wallis found that the fourth part of π is represented more exactly by the regularly formed product

$$\frac{2}{3} \times \frac{4}{3} \times \frac{4}{5} \times \frac{6}{5} \times \frac{6}{7} \times \frac{8}{7} \times \frac{8}{9} \times, \text{ etc.,}$$

the farther the multiplication is continued, and that the result always comes out too small if we stop at a proper fraction, but too large if we stop at an improper fraction. Lord Brouncker, on the other hand, represents the value in question by a continued fraction in which all the denominators are equal to 2 and the numerators are odd square numbers. Wallis, to whom Brouncker had communicated his elegant result without proof, demonstrated the same in his "Arithmetic of Infinites."

The computation of π could hardly be farther advanced by these results than Ludolf and others had carried it, though of course in a more laborious way. However, the series of powers derived by the assistance of the differential calculus of Newton and Leibnitz furnished a means of computing π to hundreds of decimal places.

Other calculations.—Gregory, Newton, and Leibnitz next found that the fourth part of π was equal exactly to

$$1-\tfrac{1}{3}+\tfrac{1}{5}-\tfrac{1}{7}+\tfrac{1}{9}-\tfrac{1}{11}+\tfrac{1}{13}-\ldots.$$

if we conceive this series, which is called the Leibnitzian, indefinitely continued. This series is indeed wonderfully simple, but is not adapted to the computation of π, for the reason that entirely too many members have to be taken into account to obtain π accurately to a few decimal places only. The original formula, however, from which this series is derived, gives other formulas which are excellently adapted to the actual computation. This formula is the general series:

$$\alpha=a-\tfrac{1}{3}a^3+\tfrac{1}{5}a^5-\tfrac{1}{7}a^7+\ldots,$$

where α is the length of the arc that belongs to any central angle in a circle of radius 1, and where a is the tangent to this angle. From this we derive the following:

$$\frac{\pi}{4}=(a+b+c+\ldots)-\tfrac{1}{3}(a^3+b^3+c^3+\ldots)+\tfrac{1}{5}(a^5+b^5+c^5+\ldots)-\ldots,$$

where $a, b, c\ldots$ are the tangents of angles whose sum is 45°. Determining, therefore, the values of $a, b, c\ldots$, which are equal to small and easy fractions and fulfill the condition just mentioned, we obtain series of powers which are adapted to the computation of π. The first to add by the aid of series of this description additional decimal places to the old 35 in the number π was the English arithmetician Abraham Sharp, who, following Halley's instructions, in 1700, worked out π to 72 decimal places. A little later Machin, professor of astronomy in London, computed π to 100 decimal places; putting, in the series given above, $a=b=c=d=\frac{1}{5}$ and $e=-\frac{1}{239}$, that is employing the following series:

$$\frac{\pi}{4}=4.\left[\frac{1}{5}-\frac{1}{3.5^3}+\frac{1}{5.5^5}-\frac{1}{7.5^7}+\ldots\right]-\left[\frac{1}{239}-\frac{1}{3.239^3}+\frac{1}{5.239^5}-\ldots\right]$$

In the year 1819, Lagny, of Paris, outdid the computation of Machin, determining in two different ways the first 127 decimal places of π. Vega then obtained as many as 140 places, and the Hamburg arithmetician, Zacharias Dase, went as far as 200 places. The latter did not use Machin's series in his calculation, but the series produced by putting in the general series above given $a=\frac{1}{2}$, $b=\frac{1}{5}$, $c=\frac{1}{8}$. Finally, at a recent date, π has been computed to 500 places.

The computation to so many decimal places may serve as an illustration of the excellence of the modern method as contrasted with those anciently employed, but otherwise it has neither a theoretical nor a practical value. That the computation of π to say 15 decimal places more than sufficiently satisfies the subtlest requirements of practice may be gathered from a concrete example of the degree of exactness thus obtainable.

Idea of exactness obtainable with the approximate values of π.—Imagine a circle to be described with Berlin as center, and the circumference to pass through Hamburg; then let the circumference of the circle be computed by multiplying its diameter with the value of π to 15 decimal places, and then conceive it to be actually measured. The deviation from the true length in so large a circle as this even could not be as great as the 18 millionth part of a millimetre.

An idea can hardly be obtained of the degree of exactness produced by 100 decimal places. But the following example may possibly give us some conception of it. Conceive a sphere constructed with the earth as center, and imagine its surface to pass through Sirius, which is 134½ million million kilometres distant from us. Then imagine this enormous sphere to be so packed with microbes that in every cubic millimetre millions of millions of these diminutive animalcula are present. Now conceive these microbes to be all unpacked and so distributed singly along a straight line that every two microbes are as far distant from each other as Sirus from us, that is, 134½ million million kilometres. Conceive the long line thus fixed by all the microbes as the diameter of a circle, and imagine the circumference of it to be calculated by multiplying its diameter with π to 100 decimal places. Then, in the case of a circle of this enormous magnitude even, the circumference thus calculated would not vary from the real circumference by a millionth of a millimetre.

This example will suffice to show that the calculation of π to 100 or 500 decimal places is wholly useless.

Professor Wolff's curious method.—Before we close this chapter upon the evaluation of π, we must mention the method, less fruitful than curious, which Professor Wolff, of Zurich, employed some decades ago to compute the value of π to 3 places. The floor of a room is divided up into equal squares, so as to resemble a huge chess-board, and a needle exactly equal in length to the side of each of these squares is cast haphazard upon the floor. If we calculate now the probabilities of the needle so falling as to lie wholly within one of the squares, that is, so that it does not cross any of the parallel lines forming the squares, the result of the calculation for this probability will be found to be exactly equal to $\pi - 3$. Consequently a sufficient number of casts of the needle according to the law of large numbers must give the value of π approximately. As a matter of fact, Professor Wolff, after 10,000 trials, obtained the value of π correctly to 3 decimal places.

IV.—PROOF THAT THE PROBLEM IS INSOLVABLE.

Mathematicians now seek to prove the insolvability of the problem. Fruitful as the calculus of Newton and Leibnitz was for the evaluation of π, the problem of converting a circle into a square having exactly the same area was in no wise advanced thereby. Wallis, Newton, Leibnitz, and their immediate followers distinctly recognized this. The

quadrature of the circle could not be solved; but it also could not be proved that the problem was insolvable with ruler and compasses, although everybody was convinced of its insolvability. In mathematics, however, a conviction is only justified when supported by incontrovertible proof; and in the place of endeavors to solve the quadrature there accordingly now come endeavors to prove the impossibility of solving the celebrated problem.

Lambert's contribution.—The first step in this direction, small as it was, was made by the French mathematician Lambert, who proved in the year 1761 that π was neither a rational number nor even the square root of a rational number; that is, that neither π nor the square of π can be exactly represented by a fraction the denominator and numerator of which are whole numbers, however great the numbers be taken. Lambert's proof showed, indeed, that the rectification and the quadrature of the circle could not be possibly accomplished in the particular way in which its impossibility was demonstrated, but it still did not exclude the possibility of the problem being solvable in some other more complicated way, and without requiring further aids than ruler and compasses.

The conditions of the demonstration.—Proceeding slowly but surely it was next sought to discover the essential distinguishing properties that separate problems solvable with ruler and compasses, from problems the construction of which is elementarily impossible, that is, by solely employing the postulates. Slight reflection showed that a problem elementarily solvable, must always possess the property of having the unknown lines in the figure relating to it connected with the known lines of the figure by an equation for the solution of which equations of the first and second degree alone are requisite, and which may be so disposed that the common measures of the known lines will appear only as integers. The conclusion was to be drawn from this, that if the quadrature of the circle and consequently its rectification were elementarily solvable, the number π, which represents the ratio of the unknown circumference to the known diameter, must be the root of a certain equation, of a very high degree perhaps, but in which all the numbers that appear are whole numbers; that is, there would have to exist an equation, made up entirely of whole numbers, which would be correct if its unknown quantity were made equal to π.

Final success of Professor Lindemann.—Since the beginning of this century, consequently, the efforts of a number of mathematicians have been bent upon proving that π generally is not algebraical, that is, that it can not be the root of any equation having whole numbers for coefficients. But mathematics had to make tremendous strides forward before the means were at hand to accomplish this demonstration. After the French academician, Professor Hermite, had furnished important preparatory assistance in his treatise *Sur la Fonction Exponentielle*, published in the seventy-seventh volume of the *Comptes*

Rendus, Professor Lindemann, at that time of Freiburg, now of Königsberg, finally succeeded, in June, 1882, in rigorously demonstrating that the number π is not algebraical,* thus supplying the first proof that the problems of the rectification and the squaring of the circle, with the help only of algebraical instruments like ruler and compasses are insolvable. Lindemann's proof appeared successively in the Reports of the Berlin Academy (June, 1882), in the *Comptes Rendus* of the French Academy (vol. CXV, pp. 72–74), and in the *Mathematischen Annalen* (vol. XX, pp. 213–225).

The verdict of mathematics.—"It is impossible with ruler and compasses to construct a square equal in area to a given circle." These are the words of the final determination of a controversy which is as old as the history of the human mind. But the race of circle-squarers, unmindful of the verdict of mathematics, that most infallible of arbiters, will never die out so long as ignorance and the thirst for glory shall be united.

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* For the benefit of my mathematical readers I shall present here the most important steps of Lindemann's demonstration, M. Hermite in order to prove the transcendental character of

$$e = 1 + \frac{1}{1} + \frac{1}{1.2} + \frac{1}{1.2.3} + \frac{1}{1.2.3.4} + \cdots$$

developed relations between certain definite integrals (*Comptes Rendus* of the Paris Academy, 1873, vol. LXXVII). Proceeding from the relations thus established, Professor Lindemann first demonstrates the following proposition: If the coefficients of an equation of nth degree are all real or complex whole numbers and the n roots of this equation $z_1, z_2, \ldots, z_n$ are different from zero and from each other it is impossible for

$$e^{z_1} + e^{z_2} + e^{z_3} \ldots + e^{z_n}$$

to be equal to $\frac{a}{b}$, where a and b are real or complex whole numbers. It is then shown that also between the functions

$$e^{rz_1} + e^{rz_2} + e^{rz_3} + \ldots e^{rz_n},$$

where r denotes an integer, no linear equation can exist with rational coefficients variant from zero. Finally the beautiful theorem results: If z is the root of an irreducible algebraic equation the coefficients of which are real or complex whole numbers, then e^z can not be equal to a rational number. Now, in reality $e^{\pi\sqrt{-1}}$ is equal to a rational number, namely, —1. Consequently, $\pi\sqrt{-1}$, and therefore itself, cannot be the root of an equation of nth degree having whole numbers for coefficients, and therefore also not of such an equation having rational coefficients. If the squaring of the circle with ruler and compasses were possible, however, π would have the property last mentioned.

PROGRESS OF ASTRONOMY FOR 1889, 1890.

By William C. Winlock.

The following record of astronomy for the years 1889 and 1890 is presented in essentially the same form as its predecessors. The compiler has made free use of reviews, in the various branches of astronomy, contributed by specialists to the *Athenæum*, *Nature*, *Journal of the Astronomical Society of the Pacific*, the *Observatory*, *Bulletin Astronomique*, the *Astronomical Journal*, and other periodicals.

NEBULÆ.

Motions of the planetary nebulæ in the line of sight.—No. 11 of the Publications of the Astronomical Society of the Pacific contains a very important paper by Mr. James E. Keeler on the "Motions of the planetary nebulæ in the line of sight." The paper is an important one in a twofold aspect: first, in its bearing on a matter just now under discussion by the highest authorities, as to the character and position of the brightest nebular line, and secondly, in the evidence it affords of nebular movements.

As to the character of the nebular line, Mr. Keeler's testimony is most emphatic, and entirely confirms Dr. Huggins's observations. "The nebular lines," he reports, "appeared to be perfectly monochromatic images of the slit, widening when the slit was widened and narrowing to excessively fine sharp lines when it was closed up." The chief nebular line "showed no tendency to assume the aspect of a remnant of fluting under any circumstances of observation." This observation, made not on one nebula, but on a number, and with a dispersion often equivalent to that of 24 prisms of 60°, for the fourth spectrum of a Rowland's grating of 14,438 lines to the inch was often used, is by far the strongest evidence we have yet had on this question of the character of the chief nebular line, and it is dead against Mr. Lockyer's theory.

The position of the nebular line is also fixed with very considerable certainty; and here, again, Dr. Huggins's observations receive complete confirmation. It was not, in any one of the nebulæ observed, coincident with the fluting of magnesium, but was always seen some distance to

the blue. The importance of this observation, especially when taken with the report as to the character of the line, is of the highest kind in its bearing on Mr. Lockyer's great meteoritic theory. If the chief nebular line is not the remnant of the magnesium fluting the very keystone is knocked away from the arch and the edifice as such falls to pieces. No doubt there would be many isolated fragments of considerable value still left. The structure might even be put together again, hereafter, on a new plan, and with a more lasting result, but the theory as it now stands—the theory as a whole—would be irretrievably wrecked. On the other hand, if the identity which Mr. Lockyer asserts were established, it would be a victory for him of the first importance.

It is indicative of the progress of practical spectroscopy that the whole question turns on an almost inappreciable difference of position, the mean value for the wave-length of the nebular line as found by Mr. Keeler from ten nebulæ, being 5,005.68 tenth-meters, whilst that of the fluting of magnesium is 5,006.36. In the brightest nebula examined the wave-length obtained was 5,006.13 tenth-meters, only 0.23 distant from the magnesium fluting. As the observations stand they point strongly to the nebular line being slightly but distinctly more refrangible than the edge of the magnesium fluting, and therefore not due to it. But the amount of displacement is not so great as to make it altogether inconceivable that it is due to the relative motion of the nebulæ and the solar system, for all the ten nebulæ observed are in that hemisphere toward which the sun is travelling, and seven of them are within 45° of the apex of the "Sun's Way," so that a correction must be applied which would tend to bring the nebular line nearer to the fluting; how much nearer we cannot, in our ignorance of the speed of the sun's motion in space, at present say, but a rate of 36 miles per second would suffice to make the accord a perfect one. If Mr. Keeler could obtain a series of comparisons of the *F* line in these nebulæ with hydrogen, the problem would be solved. Or the determination of the place of the line in a number of nebulæ in the hemisphere we are leaving would go far to settle the matter. In the mean time it is still *possible* that the eventual result may favor Mr. Lockyer's theory. It may be added in reference to Mr. Lockyer's paper, appearing in No. 293 of the Proceedings of the Royal Society, that if we accept Mr. Keeler's measures it is clear that Mr. Lockyer did not employ sufficient dispersion to decide the point at issue.

The second point brought out by Mr. Keeler's measures is the fact that the nebulæ have very distinct movements of their own. As we do not yet know to what substance the chief nebular line is due, and as Mr. Keeler could not make any measures of the blue hydrogen line in the nebulæ at all comparable in accuracy to those he made of the chief line, we can not say that the difference in position of the chief line from any given comparison line is due to the motion of the nebula. All we can do at present is to observe a number of nebulæ, adopt the mean

place they give as the true position of the nebular line, and record the differences from this mean as due to differences of motion from the mean motion. The extreme difference observed between any two nebulæ amounted to very nearly 70 miles per second.

It is impossible to leave this paper without a word on the accuracy of the measures. The spectrum of Σ 6 was examined on nine nights. The greatest difference of any one night's observation from the mean was only 0.11 tenth-metre, or in miles per second 4.2; the mean difference but 0.04, or in miles per second 1.5. Such accuracy was only possible by using an enormous dispersion, and it implies very perfect instrumental and atmospheric conditions. But it also implies an extreme delicacy of eye and hand in the observer; the "man behind the telescope" is in evidence. For it should be remembered that the great size of the Lick telescope is no special advantage in work of this particular class, its high proportion of focal length to aperture being a distinct disadvantage. A much smaller object-glass, with a focal length of 12 to 1, would give brighter images.—(E. W. MAUNDER. *The Observatory*, No. 168.)

Mr. Lockyer having published some results at variance with those obtained by Dr. and Mrs. Huggins with respect to the principal line in the spectrum of the great nebula in Orion, they have made careful re determinations, decisively confirming their previous results: (1) that the principal line is not coincident with, but falls within, the termination of the magnesium flame band; (2) that in the nebula in Orion this line presents no appearance of being a "fluting."

The faint star discovered in the trapezium of the Orion nebula by Alvan Clark, when the Lick telescope was first mounted, has been found by Barnard to be double, another star has also been detected in the trapezium by Barnard, and also one of about the same magnitude (sixteenth) as the Clark star just preceding the trapezium.

Within the ring of the well-known ring nebula of Lyra six stars have been found by Holden and Schaeberle with the 36-inch Lick telescope where but one was known before, and five new stars have been found in the nebulosity.

ASTRONOMICAL CONSTANTS.

Refraction.—M. Radau has published in volume 19 of the Paris Observatory *Annales* a very complete memoir on astronomical refraction, which deals with the theoretical as well as the practical side of the question, and contains complete tables in a convenient form suitable for actual computation.

Diurnal nutation.—M. Folie's work on diurnal nutation has not met with general acceptance. One of the latest discussions of the subject is that by Herr Lehman Filhés, published in No. 2975 of the *Astronomische Nachrichten.*

Precession.—A useful table of the third term of the precession has been computed by Herr Kloock and published by the Kiel Observatory.

Harkness's astronomical, physical, and geodetic constants.—Prof. Wm. Harkness, of the U. S. Naval Observatory, has been at work for some time upon a homogeneous system of inter-related constants, more trustworthy values of which are to be attained by the solution of equations of condition, in which the best values resulting from observation are introduced and combined with the expression of their mutual relations.

A preliminary communication of results was made to the Astronomical Journal, No. 194, but it seems preferable to quote here the final values published by Professor Harkness in Appendix III to the Washington Observations for 1891, though the latter work was not issued till after the close of the year 1890.

Professor Harkness has collected the various determinations of each of the constants in question, decided upon the values to be adopted in the computations, often using the method of least squares for this purpose; and has then employed this method in order to obtain a resultant homogeneous system.

Among the results obtained are the following:

Earth's equatorial semi-diameter	6,377 972 ± 1 248 meters.
Earth's polar semi-diameter	6,356 727 ± 99.1 meters.
Length of seconds-pendulum	$0^m.990\ 91 + 0^m.005\ 29 \sin^2\varphi$.
Length of sidereal day	86 164.099 65 mean solar seconds.
Length of sidereal year	$365^d\ 6^h\ 9^m\ 9^s.314$.
Solar parallax	$8''.809\ 05 \pm 0''.005\ 67$.
Lunar parallax	$3\ 422''.542\ 16 \pm 0''.125\ 33$.
Constant of aberration	$20''.454\ 51 \pm 0''.012\ 58$.

The mean distance of the earth from the sun, with the above value of the solar parallax is 92,796 950 ± 59 715 miles, or 149,340 870 ± 96 101 kilometers.

STAR CATALOGUES.

The star catalogue of the Astronomische Gesellschaft.—The first parts of the great catalogue of the Astronomische Gesellschaft appeared in 1890. They are the volumes containing the catalogues of zones observed by Krueger at Helsingfors and Gotha, by Boss, at Albany, and by Fearnley and Geelmuyden at Christiania. The two first mentioned volumes contain respectively the positions of 14,680 and of 8,241 stars for the equinox of 1875.

It may be worth while to recall here the origin of this great undertaking, now nearing completion. The zones of the *Histoire céleste française*, published by Lalande comprise about 50,000 stars from the first to the eighth magnitude, but they were not catalogued till nearly half a century after their completion. Those of Bessel, observed at Koenigsberg from 1821 to 1833, contain 62,600 stars from the first to the ninth magnitude between −15° and + 45° declination; the two cata-

logues of Weisse appeared in 1846 and 1863. From 1841 to 1852 Argelander continued his work at Bonn, and his northern zones (published in 1846) contain 22,000 stars between +45° and +80° and the southern zones (published in 1852) 17,000 between −15° and −31°, catalogued by Oeltzen (1851 to 1857). The positions in these different catalogues depend upon meridian observations.

In 1852, having finished his zones, Argelander conceived the plan of a work of much greater extent. It was to fix approximately the positions of all stars to the ninth magnitude, and perhaps a little below (9.5), visible in our latitudes. To accomplish this the plan was to employ simply a small telescope, the observer, with his eye always at the telescope, to call out to a recorder, who sat close by with a chronometer. The preliminary trials, by J. Schmidt, being successful, the work was begun, and, with the help of Krueger and Schoenfeld, on whom the greater part of the labor fell, the revision of the northern sky was finished in 1859; and this is the work that we know as the "Bonn Durchmusterung."

The Durchmusterung, published between 1859 and 1862 in volumes 3, 4, and 5 of the Bonn Observations, contains no less than 324,198 stars, lying between 2° south declination and the north pole, the zone between +81° and the pole being a revision of Carrington's catalogue. Volume 6 of the "Bonn Beobachtungen," contains futhermore 34,000 positions, determined by Argelander with the meridian circle. The stars of the Durchmusterung are plotted on a series of charts published in 1863. Since Argelander's death Schoenfeld has completed a similar piece of work for our southern sky, the "Südliche Durchmusterung" (1886), containing more than 133,000 stars, between −2° and −23°, and Gould at Cordoba has extended the zones to the neighborhood of the south pole.

Upon the organization of the International Astronomische Gesellschaft in 1865, the question at once came up of undertaking, by the co-operation of several observatories, the exact determination of the positions of all these stars provisionally catalogued in the Durchmusterung. A programme for the work, prepared by a special committee, was finally decided upon at the meeting in Vienna in 1869. The new revision was confined to the limits of −2° and +80° declination, the positions of the circumpolars seeming to be sufficiently well known from the work of Carrington and that of the astronomers at Hamburg and Kazan. The zones were at first assigned as follows:

80°	to 75°	Kazan.	35°	to	30°	Leipzig.
75	to 70	Dorpat.	30	to	25	Cambridge (England).
70	to 65	Christiania.	25	to	15	Berlin.
65	to 55	Helsingfors.	15	to	10	Leipzig.
55	to 50	?	10	to	4	Mannheim.
50	to 40	Bonn.	4	to	1	Neufchâtel.
40	to 35	Chicago.	+1	to	−2	Palermo.

Pulkowa undertook the determination of 539 fundamental stars carefully selected by Dr. Auwers, which should form points of reference.

In the 20 years that have elapsed since the great catalogue was

decided upon several changes have been made in the original programme, the work being eventually divided up among the following observatories:

80° to 75°	Kazan.	35° to 30°	Leyden.
75 to 70	Dorpat.	30 to 25	Cambridge (Eng.)
70 to 65	Christiania.	25 to 15	Berlin.
65 to 55	Helsingfors-Gotha.	15 to 5	Leipzig.
55 to 50	Cambridge (U. S.).	5 to 1	Albany.
50 to 40	Bonn.	+1 to −2	Nicolaief.
40 to 35	Lund.		

The work of observation is now finished. Some of the zones have been published (Kazan, Christiania, Helsingfors, Lund), others are in press, and the catalogues have been begun. Three of the catalogues (Albany, Helsingfors-Gotha, and Christiania,) have just appeared. Meanwhile the zones have been extended to the southern sky, the following being to a greater or less extent under way:

−2° to − 6°	Strasburg.	−14° to −18°	Washington.
−6 to − 10	Vienna.	−18 to −23	Algiers.
−10 to − 14	Cambridge (U. S.).		

The positions of the 303 fundamental southern stars are furnished by observations undertaken at the Cape of Good Hope, Madison, Annapolis, Carlsruhe, Leiden, and Strasburg. Gould's southern zones extend from −23° to −80°, and it is to be hoped that before long we shall have a catalogue embracing the whole sky, the value of which will be in no wise diminished by the photographic chart which is about to be begun.

The observations for the Helsingfors-Gotha catalogue were made almost entirely by Dr. Krueger with a $0^{m}.15$ (5.9 inch) Reichenbach meridian circle. The star positions are for the epoch 1875, and besides the right ascension and declination, the precession and secular variation, and wherever possible the proper motion are given. The observations for the Albany zone were made by Professor Boss with a $0^{m}.20$ (7.9 inches) Pistor & Martin's meridian circle, the transits being recorded on the chronograph, while Dr. Krueger used the "eye-and-ear" method.

The probable errors come out:

	In right ascension.	In declination.
Helsingfors	$\pm 0^{s}.101$	$\pm 0''.51$
Gotha	.125	.76
Albany:		
2 observations	.025	.39
3 observations	.021	.32
4 observations	.018	.27

Experiments were made with wire-gauze screens by Professor Boss to determine the effect of difference of magnitude upon the observations, his result being that a change of one magnitude produced a change of $0^{s}.014$ in the personal equation in observing a transit.

A third installment of the catalogue, that containing the stars from $+64^\circ 50'$ to $+70^\circ 10'$, has also appeared. The observations were made by Professors Fearnley and Geelmuyden with the Ertel meridian circle of the Christiania Observatory, of 48 lines aperture. The probable error of a single observation is given as $\pm 0^s.054$ in right ascension ($\pm 0^s.02$ in a great circle) and $\pm 0''.54$ in declination.

Yarnall's catalogue.—A third edition of the catalogue of southern stars observed with the transit instrument and mural circle at the U. S. Naval Observatory from 1845 to 1877 has been published, the work of revision having been conducted by Professor Frisby. Great pains have been taken to eliminate all errata detected in the previous editions, both by the careful examination of published lists of corrections and by comparisons with other catalogues. The whole number of stars in the new edition is 10,964.

Munich catalogue.—Band 1 of the "Neue Annalen der k. Sternwarte in Bogenhausen bei München" contains a catalogue of 33,082 stars down to the tenth magnitude inclusive, between -32° and $+24^\circ$ declination, reduced to the epoch 1880.0. The observations were made with a Reichenbach meridian circle of 109^{mm} (4.3 inches) aperture and circle of 0.95^m (37.4 inches) diameter.

Second Melbourne catalogue.—This catalogue contains the results of observations made with the old transit circle of 5 inches aperture from the beginning of 1871 to the end of August, 1884; places of 1,211 stars are given for 1880.

Brussels catalogue.—The Brussels catalogue contains 10,792 stars for the epoch 1865, observed with the Brussels transit instrument and mural circle in the years 1857—1878; the general catalogue is preceded by the positions of the fundamental stars used in the reductions. A supplement is to be published giving corrections to the catalogue due to a number of inaccuracies detected in the reductions.

The Williams College catalogue of north polar stars.—Professor Safford has published a catalogue of right ascensions of 261 stars, mostly within 10° of the north pole, and observed by him with the 4½-inch Repsold meridian circle of the Field Memorial Observatory at Williamstown. The results have been reduced to the epoch 1885.0. Professor Safford characterizes his catalogue as an "attempt to strengthen the weak point of all our standard catalogues—the right ascensions of polar stars," and he draws the following conclusions from his work.

"First. That it is highly conducive to accuracy, systematic as well as in detail, to base a catalogue of polar right ascensions upon standard places in all hours of right ascension, rather than upon double transits alone.

"Second. That the introduction of meridian marks according to Struve (long-focus object glasses, also suggested by Rittenhouse) is a great advantage to the primary catalogues.

"Third. That the eye-and-ear method should be retained as the standard within a narrow rather than a wide range of polar distance.

"Fourth. That modern meridian instruments are subject to irregular small changes of position, which are not direct functions of the temperature; so that in all differential work it is better to keep a close watch upon clock rate and instrumental adjustments rather than to trust the instrumental zero points for more than 2 hours without redetermination of the most essential.

"Fifth. That the right ascensions here given are reasonably accurate.

"Sixth. That a thorough comparison of the chronographic and eye-and-ear method within a wide range, both of magnitude and declination, is desirable."

Greenwich 10-year catalogue, 1877 *to* 1886, published in the volume of Greenwich Observations for 1887, contains 4,059 stars for the epoch 1880.0.

The catalogue of 303 reference stars for the southern zones of the Astronomische Gesellschaft has been published by Dr. Auwers, and although the material accumulated since 1880, when the provisional list was issued, is not sufficient to give places of a thoroughly satisfactory degree of accuracy, the final corrections will probably be extremely small.

A collection of all available meridian observations of stars that will be within 1° of the north pole in 1900 has been prepared, under the direction of Professor Pickering, by Miss Winlock and published as the ninth memoir in volume 18 of the Harvard Observatory Annals.

STELLAR PARALLAX.

Professor Pritchard intends to examine for parallax, by the aid of photography, all stars of the second magnitude suitably situated for observation at Oxford, in the hope of contributing to our knowledge of what Herschel called the "construction of the heavens." With reference to the differences in the results obtained by different observers, Professor Pritchard says: "Guided by the suggestions of recent experience, I now think that such differences of 'parallax' might very reasonably have been anticipated and may probably be accepted as matters of fact without in any degree impugning the accuracy of the observations. For in process of this work on parallax, and also from the general history of similar inquiries, it has been made abundantly evident that no necessary connection exists between the brightness of a star and its position in space, or distance from the sun. Nevertheless it is this very difference of brightness mainly which guides us in the selection of comparison stars. The 'parallax' is, in fact, and is becoming more and more generally recognized to be, a differential quantity, fainter stars being in very many instances much nearer to us than others possessing incomparably greater brightness. In passing I may here instance α Lyræ as compared with 61 Cygni; β Centauri as

compared with ε Indi. In fact, the position in space of the faint comparison stars in relation to that of the star whose parallax is sought is, if not a matter of accident, at all events wholly unknown until the observations and computations are complete."

Professor Pritchard's results for stellar parallax, as published in the third volume of the Oxford Observations, are as follows:

Star.	Magnitude.	Proper motion.	Parallax.
		$''$	$''$
61^1 Cygni	4.98	5.16	0.44
61^2 Cygni	4.98	5.16	0.44
μ Cassiopeiæ	5.40	3.75	0.04
Polaris	2.05	0.05	0.08
α Cassiopeiæ	2.41	0.05	0.04
β Cassiopeiæ	2.32	0.55	0.16
γ Cassiopeiæ	2.19	0.02	0.01
α Cephei	2.57	0.16	0.06

The greater part of this volume is devoted to a discussion of the parallax of 61 Cygni and the results seem to justify his remark that "the four comparison stars probably belong to a remote system not containing 61 Cygni." The probable errors deduced are small.

At the annual visitation to the Oxford Observatory on June 12, 1890, Professor Pritchard announced the results of the determination of parallaxes of six more stars by the photographic method, as follows:

	Parallax.	Prob. error.
	$''$	$''$
ε Cygni	+ 0.115	± 0.034
γ Cygni	— .040	.029
β Andromedæ	+ .092	.023
α Arietis	+ .080	.027
α Persei	+ 0.74	.029
β Ursæ Minoris	+ .022	.030

The subjoined table forms a summary of a paper published in the Astronomische Nachrichten, Nos. 2915 and 2916, by Dr. Oudemans, in which he collects the scattered results for stellar parallax obtained in the past sixty years. Dr. Oudemans concludes that "stars with proper motions greater than 0.″05 have probably an annual parallax of 0.″10 to 0.″50.

No. of stars.	Proper motion.	Annual parallax.	Distance in light years.
	$''$	$''$	
9	4.93	0.32	10
9	2.33	.20	16
9	1.00	.20	16
9	0.38	.18	18
10	0.05	.16	20

PROPER MOTIONS.

Professor Boss has published in the Astronomical Journal the proper motions of 295 stars of the Albany zone (+ 0° 50′ to + 5° 10′).

In the *Bulletin Astronomique* for March, 1890, is a most useful catalogue, compiled by Bossert, of all stars whose proper motion is known to exceed 0.″50. They are thus distributed:

No. of stars.	Proper motion greater than
	″
5	5.0
4	4.0
6	3.0
9	2.0
11	1.5
30	1.2
15	1.0
38	0.8
77	0.6
73	0.5

DOUBLE AND MULTIPLE STARS.

Some very elegant and simple formulæ for determining the true orbit of a binary star, originally published in Russian, have been brought out by Professor Glasenapp.

ξ Scorpii.—Herr Schorr has made a study of the motions in this triple system by methods similar to those employed by Dr. Seeliger on ζ Cancri. The star is known as Σ 1998, the magnitudes of its components being A = 3.9, B = 5.2, C = 7.2.

η Ophiuchi has been divided into two nearly equal components by Burnham with the 36 inch Lick telescope, and he thinks that it will prove to be a binary of short period. He has also found companions for Aldebaran, γ Cassiopeiæ, and δ Cygni, and has been able to separate and measure a companion to the principal star in the pair ε Hydræ, the existence of which was suspected by previous observers.

Photographs of the spectrum of Spica have put beyond question the reality of its motion in the direction of the line of sight. Dr. Vogel has deduced from observations of 1889 and 1890 a period of revolution of about 4 days.

PHOTOMETRY.

The results of observations made with the meridian photometer of the Harvard observatory by Prof. E. C. Pickering and Mr. Wendell during the years 1882-1888, have appeared as volume 24 of the Harvard Annals. The principal work done with this instrument was "the determination of the magnitudes of a sufficient number of stars contained in the Durchmusterung, and distributed with approximate uniformity, to serve for future estimates or measures of magnitude, and to enable previous estimates to be reduced to the photometric scale."

The number of stars of which observations are recorded is 20,125; so that when the stars enumerated in volume 23 of the Annals are reckoned, the total number of stars observed reaches 20,982. Measures have also been made of 166 variable stars and of several planets and satellites. In the "Harvard Photometry" the brightest stars were compared solely with Polaris. In the present observations λ Ursæ Minoris was selected as the standard star, but the results are made to depend upon a series of 100 circumpolar stars, the magnitudes of which were frequently determined with the smaller instrument.

Photographic photometry.—The readiest and most effective means of determining the magnitudes of stars from an examination of the disks impressed on a sensitized film is a problem that has received much attention recently, and contributions to the literature of the subject have been made from the three observatories of Harvard, Stockholm, and Potsdam.

Professor Pickering gives in volume 18 of the Harvard Annals three catalogues of magnitudes, embracing, on the whole, some 2,500 stars, the first catalogue giving the photographic magnitudes of all the stars brighter than the fifteenth magnitude within 1° of the pole; the second, the magnitudes of many of the stars in the Pleiades; and the third the magnitudes of 1,131 stars generally brighter than the eighth magnitude near the equator.

The contribution from the Potsdam observatory is confined to the discussion of the magnitudes of stars in the Pleiades as impressed on plates taken with a chemically corrected object-glass by Dr. Scheiner, and with the reflecting telescope of the Herény observatory, supplemented by some photographs of the artificial stars in a Zöllner photometer. The principal results of the inquiry are twofold: first, that the increase of the diameter of the star disk varies as the square root of the time of exposure; and secondly, that a simple linear relation exists between the observed diameter and the magnitude.

The third contribution to this subject is from Dr. Charlier, of Stockholm, who deduces a formula which expresses the connection between the photographic brilliancy of a star and its photographed image in such a manner as to insure a coincidence as far as possible between the photographic and photometric magnitudes.

VARIABLE AND COLORED STARS.

Chandler's catalogue of variable stars.—Chandler's admirable catalogue of variable stars has been adopted by Schoenfeld in the ephemerides published in the Vierteljahrsschrift, and it also furnishes the data for the ephemerides of the Annuaire du Bureau des Longitudes and the Observatory, and is thus formally recognized as the standard authority on variables. Mr. Chandler publishes in the Astronomical Journal (No. 216) three tables supplementary to the catalogue, containing (1) a list of new variables arranged as in the original catalogue; (2) a list of

additions and corrections to the elements of the catalogue; and (3) a list of stars probably variable, but whose variability needs further confirmation before definitive letters can be assigned. The attention of observers is directed to this list.

Taking his catalogue of 1888 as a basis, Mr. Chandler has made an investigation of the relation existing between the lengths of the periods and the number of the variables; their color, range of fluctuation, forms of light curves, irregularities of periods and of light variations. Periods under 20 days predominate, while for the long-period stars a well-marked maximum is indicated about a period of 320 days. With regard to color, the redder the tint the longer the period; and with regard to range of fluctuation, while it is probable that there is a dependence of range upon the duration of the period, the relation is not one of strict proportionality of range to period. It furthermore appears that the average ratio of increase to decrease for stars with periods less than 100 days is about 0.65; between 100 and 200 days it is slightly in excess of unity; it then declines as the periods lengthen; at first, gradually, but in the neighborhood of a year, with extraordinary suddenness, recovering as quickly until it again exceeds unity in the group of extremely long periods. In the case of the numerical laws of the perturbations of the periods, Mr. Chandler remarks that his researches are not yet complete, but that, broadly, in the case of long-period variables, the irregularities are periodic in their nature, and in the case of those of short period, secular and exceptional.

Algol.—Prof. H. C. Vogel, of Potsdam, has published the results of some interesting observations of the changes in the spectrum of Algol at the times of the diminution and recovery of its light. These, whilst fully confirming the view originally suggested by Goodricke, that the periodic variability of this star is caused by the revolution of a dark companion cutting off part of its light in the manner of an eclipse, and the calculation of Professor Pickering that the diameter of the companion amounts to about eight-tenths of that of the principal star, have enabled Professor Vogel to obtain approximate values of the mutual distance and actual sizes and masses of the two stars, as well as of their orbital velocities round their common center of gravity. He finds, in fact, that their diameters are probably about 1,080,000 and 850,000 English miles respectively; that the distance of their centers from each other amounts to about 3,290,000 miles, and that the orbital velocity of Algol is about 27, whilst that of its companion is about 56 miles. The mass of the former he determines to be about double that of the latter, the one being approximately four-ninths and the other two-ninths of the sun's mass. It is not necessary, he remarks, to suppose that the companion is absolutely opaque, but only that its light is very much feebler than that of the principal star.

It may be added that the Greenwich observations confirm Dr. Vogel's conclusion of the motion of the star in a small orbit.

A remarkable star of the Algol type, having the shortest period known, was discovered in 1888 by Prof. H. M. Paul, of the U. S. Naval Observatory. The star is 12 Antliæ of Gould's Uranometria Argentina, $\alpha = 9^h\ 26^m\ 50^s$, $\delta = -28°\ 4.7$ (1875.0). The range of magnitude is 6.7 to 7.3, and according to Chandler it goes through its changes in $3^h\ 20^m$.

From an examination of one of the photographic plates taken by the Harvard observatory party, at the Chosica station in Peru, Professor Pickering has announced the discovery of a long-period variable in Cælum of the same class as O Ceti, R Hydræ, and R Leonis. The spectra show bright hydrogen lines.

A number of other new variables have been detected in the examination of the photographic plates taken at the observatory, and have been announced by Professor Pickering in the Astronomische Nachrichten. Some attention has also been paid to this subject by Dr. J. C. Kapteyn in measuring the plates taken at the Cape of Good Hope for the formation of Dr. Gill's photographic *southern Durchmusterung*, and also by Mr. Roberts in the prosecution of his work in astronomical photography.

A general index to observations of variable stars, prepared under the direction of Prof. E. C. Pickering, forms No. 8 of Vol. 18 of the Harvard Annals. A large number of unpublished observations are referred to, particularly three extensive series of observations by Argelander, Heis, and Schmidt, to whose manuscripts access was given.

A new edition or rather revision of *Birmingham's Red Star Catalogue* has been printed in No. V of the Cunningham Memoirs of the Royal Irish Academy. The work of revision was undertaken by Rev. T. E. Espin in 1886, with the $17\frac{1}{4}$-inch equatorial reflector, and in the course of the work a number of new red stars, new variables, and stars with bright lines in their spectra were discovered. There is also an additional list of 629 "ruddy stars."

STELLAR SPECTRA.

Spectrum of ζ Ursæ Majoris.—Professor Pickering has reported a remarkably interesting peculiarity in the spectrum of this star. It was noticed that the K line was double in the photographs taken March 29, 1887, May 17, 1889, and August 27 and 28, 1889, while on many other dates the line appeared hazy as if the components were slightly separated, and at other times the line was well defined and single. It was concluded that the line was double at intervals of 52 days beginning March 27, 1887, and it was predicted that the doubling would occur again on December 9, 1889, and this prediction was confirmed by each of three photographs on the latter date. Professor Pickering says:

"The only satisfactory explanation of this phenomenon as yet proposed is that the brighter component of this star is itself a double star having components nearly equal in brightness and too close to have been separated as yet visually. Also that the time of revolution of the sys-

tem is 104 days. When one component is approaching the earth al the lines in its spectrum will be moved toward the blue end, while al the lines in the spectrum of the other component will be moved by a equal amount in the opposite direction if their masses are equal. Eac line will thus be separated into two. When the motion becomes pe pendicular to the line of sight, the spectral lines recover their tru wave-length and become single."

From the amount of separation of the lines Professor Pickering con cludes that the relative velocity of the two components must be abou 100 miles per second. If the orbit is circular and its plane passe through the sun, the distance traveled by one component, regardin the other as fixed, would be 900,000,000 miles, and the distance apa of the two components would be 143,000,000 miles, or about that Mars and the sun. The combined mass would be about forty time that of the sun to give the required period.

Several other stars have been found from the Harvard photograph with a similar doubling of the lines, among them β Aurigæ and b Oph uchi. For β Aurigæ Professor Pickering deduced a period of 4 day and his results have been fully confirmed by observations made wi quite different apparatus by Dr. Vogel at Potsdam.

A doubling of the K line in several photographs of the spectrum Vega taken by Mr. A. Fowler, apparently indicating that Vega was double star of the ζ Ursæ Majoris type, has not been confirmed by t photographs of Pickering, Vogel, and Henry.

The Henry Draper Memorial.—The third annual report of Profess Pickering announces the practical completion of two branches of t work undertaken, the photographic survey of the spectra of all sta north of — 25° declination having been effected on a twofold scale, t one survey including all stars brighter than the seventh magnitude, t other including stars two magnitudes fainter. The Bache 8-inch dou let employed in this work has been transferred to a station near Cho ica in Peru and similar surveys for the stars down to the south po have been commenced.

The fourth annual report of the Henry Draper Memorial contains a frontis piece an engraving showing the periodical duplication of t K line in the spectrum of β Aurigæ, the study of which, with oth similar cases has been the most important work of the 11-inch equ torial at Harvard. The spectroscopic survey of the brighter sta in the northern hemisphere (to — 25° declination) is nearly print and the work on fainter stars is being satisfactorily pressed. Besid the spectra, charts of the entire sky are being formed with the sam telescopes. A photographic map of the sky will thus be provided, a proximately on the scale of the Durchmusterung, but including faint stars; so far as it has been completed it has proved very convenie for studying suspected variables and in detecting errors in star cat logues.

Reference should also be made here to the lists of stars with peculiar spectra detected upon the Harvard Observatory photographic plates and published from time to time by Professor Pickering in the *Astronomische Nachrichten.*

A spectroscopic survey of the southern heavens by direct observation has been undertaken at the Melbourne Observatory. An 8-inch refracter and the 4-foot reflector will be used in the work.

MOTIONS OF STARS IN THE LINE OF SIGHT.

The following is a comparison of the results for motion in the line of sight obtained by Dr. Vogel at Potsdam with a photographic telescope, and those obtained by Maunder at the Greenwich Observatory by eye observations. The motions are given in geographical miles, + representing recession, and — approach:

	Vogel.	Maunder.
Capella	+17.1	+22.5
Aldebaran	+30.3	+31.6
α Persei	— 7.2	—22.5
Procyon	— 7.2	+ 3.8

Dr. Vogel's interesting results with regard to Algol and other stars have been alluded to elsewhere.

Bright lines in stellar spectra.—Mr. Espin has detected bright lines in the spectra of a number of variables when near their maxima, among them R Leonis, R Hydræ, χ Cygni, R Andromedæ, and S Cassiopeiæ all of Secchi's third type. Similar lines in the spectra of U and V Cygni, of the fourth type have been suspected by the Lick observers, and when these stars were far removed from their maxima. Mr. Keeler also finds that he is able to break up the apparently continuous spectra of stars of the type of the Wolf-Rayet stars in Cygnus into an extremely complicated range of absorption bands and faint bright lines.

A remarkable form of spectrum has been discovered by Professor Pickering in that of the star Pleione, for the F line consists in this case of a narrow bright line superposed on a broader dark line, the other hydrogen lines showing some indications of a similar character.

ASTRONOMICAL PHOTOGRAPHY.

The photographic chart of the heavens.—The permanent committee appointed by the Astrophotographic Congress at Paris, in 1887, as noted in the Review of Astronomy for the years 1887–88, held their first meeting at the Paris Observatory in September, 1890. The results of the seven séances are contained in a series of twenty-eight resolutions, some of the most important of which are mentioned below.

The zones were assigned to the several participating as follows:

NORTH.

	Latitude.		Zone.	
	°	′	°	°
Helsingfors	+60	9	+90 to	+70
Potsdam	52	22	70	58
Oxford	51	45	58	48
Greenwich	51	28	48	40
Paris	48	50	40	32
Vienna	48	13	32	24
Bordeaux	44	50	24	18
Toulouse	43	37	18	12
Catania	37	30	12	6
Algiers	36	48	+ 6 to	0
San Fernando	36	27	0 to	— 6
Chapultepec	19	26	— 6 to	—12
Tacubaya	+19	24	—12 to	—18

SOUTH.

Rio de Janeiro	—22	54	—18 to	—26
Santiago	—33	26	26	34
Sydney	—33	51	34	42
Cape of Good Hope	—33	56	42	52
La Plata	—34	55	52	70
Melbourne	—37	50	—70 to	—

No observatory in the United States appears on this list. A bill was introduced in Congress making an appropriation to enable the United States Naval Observatory to undertake a share of the work, but the bill failed to become a law.

The committee decided that the field of the telescope available for measurement should be 2° square; that the photographic plates employed (which are to be of plate glass) should be 160^{mm} ($6\frac{1}{4}$ inches) square and the series of reference lines 130^{mm} ($5\frac{1}{8}$ inches) square with the lines 5^{mm} apart.

Twelve test objects were selected, all of which are situated near the equator, at intervals of about two hours of right ascension. In addition to these, the Pleiades, Præsepe, and a group in Cygnus were selected for the use of the more northern observatories.

To fix the time of exposure so that the plates shall contain stars to the eleventh magnitude, it was decided to determine first the time necessary to photograph a star of the 9.0 magnitude of Argelander's scale, and thereby multiplying by 6.25 the time of exposure for magnitude 11.0 will be obtained.

Three more numbers (3, 4, and 5) of the *Bulletin du Comité International permanent* pour l'Exécution Photographique de la Carte du Ciel have been published. Among the many papers contributed to these

bulletins which have a very important bearing upon astronomical photography, may be mentioned one by Dr. Bakhuysen on the measurement of the plates by the method of rectangular coördinates, in which he obtains star-places comparing favorably with those from meridian observations. Dr. Vogel contributes one or two papers on the "réseaux" and the measurement of the plates, and Professor Kapteyn suggests the expediency of taking the catalogue plates with three exposures at intervals of six months, for the purpose of determining the stars' proper motions and parallaxes. Dr. Scheiner has an important paper on the application of photography to the determination of stellar magnitudes.

In the fifth number of the *Bulletin*, Professor Holden has two papers on the photographic magnitudes of stars, and Mr. Schaeberle one on the same subject. There is also an abstract of Dr. Lindemann's photometric determination of the star magnitudes of the Bonn Durchmusterung, and a paper by M. Trépied on the necessity of coming to some understanding as to what is meant by stars of the 9th, 11th, and 14th magnitudes on the photographic plates.

The question of the reproduction of the plates and of the publication of the map has been left open, but it is probable that one or more bureaus will be established for measuring the negatives obtained at observatories not provided with special apparatus for the purpose, and photographic copies of all plates will be preserved in selected places in case of accident to the original negatives.

A meeting of those interested in the various branches of astronomical photography other than the chart was called by Messrs. Janssen and Common in September, 1889. The chief matters for discussion being a complete photographic record of solar phenomena, including solar spectrum photography; a systematic description of the lunar surface by photography on a large scale; photographs of planets and their satellites, of comets, meteors, and particularly of nebulæ, clusters, and of stellar spectra.

In discussing the theory of the photography of a star projected upon a bright background, Professor Holden calls attention to the fact that the most important factor is the ratio of the focal length to the aperture of the objective; generally speaking it would be an advantage to diaphragm the objective during the day. This is also true with regard to ordinary observations during the day, a point of particular importance in connection with meridian observations.

Authoritative testimony as to the value of photography for obtaining accurate measures of star clusters is given by Dr. Elkin, who has compared Dr. Gould's reductions of Rutherfurd's photographs of the Pleiades taken over 20 years ago, with the heliometer measures made at Königsberg and New Haven. The smallness of the probable error Dr. Elkin regards as proof that in photography we have a means of investigation for micrometric work at least on a par with any existing

method, and doubtless far surpassing the present methods in ease of measurement and output of work.

The Henry Brothers are reported to have made a decided advance in lunar photography in the plates taken with the equatorial of $0^{m}.32$ (12.6 inches) aperture intended for the chart work. The improvement is attributed especially to the process of enlargement employed, which makes the diameter of the moon about 1^{m} (39 inches). This photographic work is to be continued with the great equatorial coudé, which is soon to be mounted and provided with a photographic objective.

Mr. Roberts has devised a machine, which he calls a "pantograver," for measuring the magnitudes of the stars depicted upon the photographic plates and transferring them to metallic plates for printing.

COMETS.

The origin of comets.—Dr. Bredichin, the present director of the Pulkowa Observatory, who has devoted much time to the study of cometary phenomena, has expressed the opinion that periodic comets owe their origin to the segmentation of ordinary parabolic comets, having been thrown off from the latter by an eruption such as it is generally supposed we have witnessed in the great comet of 1882, and earlier in Biela's comet. Dr. Kreutz's monograph on this great September comet of 1882 forms one of the most important of recent contributions to cometary literature. The formidable obstacles to an accurate determination of its orbit presented by the disintegration of the nucleus into several points of condensation seem to have been most skillfully surmounted by the computer. His final value for the period of revolution is 772.2 years.

Dr. Holetschek claims that the systematic grouping of cometary perihelia in certain directions (270° and 90° of heliocentric longitude) has no connection with the general motion of the solar system in space, but is due to the position of the earth at the time that such discoveries are most readily made.

An important paper on the capture theory of comets will be found in the *Bulletin Astronomique* for June, 1889, and in the same journal for December, 1890, M. Tisserand has a further contribution to the same subject.

The *Observatory* for August, 1889, has a useful table of the approximate positions at the time of discovery of all comets seen since 1869, with brief notes on the physical appearance of each. Mr. Denning, who has compiled this table, proposes to supplement it by one with similar data for the comets from 1840 to 1868.

Brorsen's comet.—A careful search for Brorsen's comet, which passed perihelion in 1890, was made by Brooks and Swift, but without effect. This comet was discovered in 1846, and was last seen in 1879; it could not be seen at the return in 1884. Tempel's second comet, and Barnard's comet 1884 II, were also expected to return to perihelion in

1890, but were unfavorably situated for observation and escaped detection.

Comets of 1889 *and* 1890.—W. R. Brooks reported the discovery, on the morning of January 15, 1889, of a faint comet in Sagittarius, and to it the designation Comet *a* 1889 was given, as the first comet discovered during the year. A careful search for the object was made by a number of observers, especially by Barnard and Swift, but without success. As the three observations necessary for determining the orbit were not secured, the comet is not catalogued with those of the year. A comet announced by Swift on July 15, 1889, is also omitted, as it proved to be identical with the comet discovered by Brooks on August 7, 1888. (1888 III).

A phenomenon reported at Grahamstown, South Africa, on the 27th of October, 1890, should be mentioned in connection with the notes on comets. It was described as a bright band one-fourth of a degree wide and 30° longitude, afterwards increasing to 90°. At one end it looked like the head of a comet, while the other end faded out gradually. Its motion was extraordinary, as it swept over more than 100° in less than $1^h\ 15^m$.

The comets for the years 1889 and 1890, with their final designations, in the order of perihelion passage are as follows:

Comet 1889 I:
= Comet *e* 1888.

The first comet of 1889, in the order of perihelion passage, was that discovered by Barnard at the Lick Observatory with a 4-inch comet-seeker on September 2, 1888, or the morning of September 3. It was also independently discovered by Brooks, at Geneva, New York, on the following morning. At the end of November, and as late as January 4, 1889, it was visible to the naked eye. Perihelion was passed on January 31, 1889, and by that time, the comet disappeared in the sun's rays. The first observations after conjunction were made about May 24, and it was followed till its light was again overpowered by that of the sun, late in October, 1889, its appearance being about the same as before perihelion, small, round, quite bright, and with a short tail. The orbit seems to be hyperbolic.

Barnard remarked on June 3 that there was an anomalous tail directly following the comet, about 1° in length and some 2′ or 3′ broad, a phenomenon which, according to Bredichin, was probably an effect of perspective.

The comet was observed again at the Lick Observatory by Barnard August 17, 1890, although its distances from the earth and sun were then, respectively, 6.0 and 6.5 in terms of the earth's mean distance. The later observations confirm the hyperbolic character of the orbit.

Comet 1889 II:
= Comet *b* 1889.

On the evening of March 31, 1889, E. E. Barnard discovered, with the 12-inch equatorial of the Lick Observatory, a very small and slender comet, with a tail 15′ long. By the end of April it was lost in the evening twilight, reappearing again, with extremely slow geocentric motion, about July 25, and remaining visible to November 21. The great perihelion distance of this comet is

especially noteworthy, amounting to $2\frac{1}{4}$ times the distance of the earth from the sun, a distance which seems to have been surpassed in the catalogue of comets only by comet 1885 II, with a perihelion of $2\frac{1}{2}$, and the comet of 1729, with perihelion distance 4.

Comet 1889 III:
= Comet *c* 1889.

Mr. Barnard discovered another comet at about 2 o'clock on the morning of June 24, in the constellation Andromeda. At the time of discovery the comet was only three days past perihelion. It was then very faint and rapidly became still fainter, being last observed on August 6. The elements computed by Berberich show considerable ellipticity in the orbit, the period of revolution being 128 years.

Comet 1889 IV:
= Comet *e* 1889.

A tolerably bright comet was discovered with the naked eye by Mr. J. Ewen Davidson at Branscombe, Mackay, Queensland (latitude -21° $9'$ south), on July 19. It had a sharp, stellar nucleus, and a tail $30'$ long; in a photograph taken by Barnard at the Lick Observatory on July 30, the tail could be followed still farther, to a distance of almost 1° from the head. A second tail was reported by Kammermann, of Geneva, on the 17th of August, and a segmentation of the nucleus by Riccò about a week earlier.

Professor Holden finds that the brightest part of the tail was $\frac{1}{400}$ of the brightness of the brightest part of the solar corona during the eclipse of January 1, 1889, and $\frac{1}{8000000}$ that of the full moon.

The comet was followed in the northern hemisphere to about the end of the year.

The spectrum according to the Lick and Palermo observations in July and August showed no peculiarity; the carbon bands, and the continuous spectrum of the nucleus, alone being recoided.

Comet 1889 V:
= Comet *d* 1889.

William R. Brooks, of Geneva, New York, while sweeping in the southwestern sky on the morning of July 6, 1889, detected a suspicious looking nebulous object, the cometary character of which he was able to confirm on the following morning; it was then faint, of about 11th magnitude, a diameter of $1'$, stellar nucleus, and tail $10'$ long. The comet attracted no especial attention from astronomers till August 1, when Barnard discovered that it had two small and nebulous companions, and on the morning following it was evident that these two objects were moving with the parent comet through space. Mr. Barnard says:

"On August 3 they were examined with the 36-inch equatorial, which showed the whole group very beautifully. Each of the companions had a very small nucleus and condensation in a very small head and a short faint tail, presenting a perfect miniature of the larger one, which was pretty bright and well developed, with small nucleus and slightly fan-shaped tail $\frac{1}{4}^\circ$ long. There was then absolutely no nebulous connection with the larger, nor has there been at any time since, either in the 12-inch or in the 36-inch telescope. Nothing whatever has been seen here of the nebulous envelope spoken of by the Vienna observers as appar-

ently inclosing the whole group (A. N., 2914). I have from the first carefully looked for a nebulous connection. Under unfavorable circumstances the tails of B and C might be imagined to be a connecting nebulosity, but the tail of B falls short of A, and that of C does not nearly reach B. Each comet is in appearance absolutely independent of the other. The tails of all three have lain in the line of the nucleus of A, and therefore have not sensibly deviated, from the position-angle 241°."

"On August 4, two other companions were detected with the great telescope, one of which was measured, the other being too elusive to set the wire on. I have numbered these four companions B, C, D, E, in the order of increasing right ascension, A being the larger comet, D and E being the two last discovered. D has been seen several times since the moon withdrew, but has always been too faint to observe. It has not sensibly changed its position. E has only been seen once. Its position angle referred to C would be the same as that of D, and its distance twice as great. Four or five other nebulous bodies observed near the comet, August 2, have not since been seen, and were probably nebulous.

"The results of the observations of the two brighter companions are extremely interesting. Measures of B have been made on eighteen, and of C on seventeen nights. These two have almost exactly the same position-angles, which have been sensibly constant. Their distances from the main body have, however, been increasing. At the last observations, B seems to be stationary, the distance from A remaining constant, while C continues to recede."

Mr. Chandler's investigation of the orbit of this comet has developed a strong probability that it is identical with a comet discovered by Messier in 1770, often called Lexell's lost comet, because that astronomer calculated that it was moving in an elliptic orbit with a period of about 5½ years, though it was not seen afterwards. It is now well known that this was due to the fact that at the return in 1776 its position was such as to render any observation impossible, and before another return could take place the comet made in 1779 so close an approach to the planet Jupiter as completely to change the nature of the orbit. Mr. Chandler finds that Brooks's comet also made a near approach to Jupiter, so near, in fact, on May 20, 1886, that it was only about nine diameters of Jupiter distant, or only a little outside the orbit of his third satellite. Calculation of the elements of the comet orbit before this appulse leads to the conclusion that they present a great similarity to those of Lexell's comet after its approach to the planet in 1779, rendering the probability great that the bodies are identical. Mr. Chandler shows that no similar serious disturbance will occur again until 1921, so that appearances may be looked for in 1896, 1903, 1910, and 1917, at each of which return the condition of visibility will be favorable, giving opportunities for further investigations into the motions of this interesting comet, which, it appears, narrowly

escaped being converted into a fifth satellite of Jupiter. Mr. Barnard succeeded in finding and observing the comet again, on the night of November 21, 1890, with the 36-inch Lick telescope, eight months after it had been given up as beyond reach; and when its distance from the earth was 3.09, and from the sun 3.55.

Comet 1889 VI: =Comet *f* 1889. Swift, at Rochester, discovered a new comet on November 16, while searching for new nebulæ; it was a faint round nebulous mass, without tail, and it remained exceedingly faint during its entire period of visibility; being seen in only the most powerful telescopes about the middle of January. The orbit proved to be elliptical, and with the remarkably short period of 8.8 years, according to Searle's computation.

Comet 1890 I: =Comet *g* 1889. A faint comet was discovered by Borelly at the Marseilles Observatory on December 12, 1889, this being the first comet, after an interval of three years, discovered in Europe. On January 8, 1890, it appeared in the finder of the Munich refractor like a faint star of the seventh or eighth magnitude.

Comet 1890 II: = Comet *a* 1890. Discovered by W. R. Brooks at the Smith Observatory, Geneva, New York, March 19, 1890. A small comet with stellar nucleus and short tail. It was still observable about the middle of October.

Comet 1890 III: = Comet *b* 1890. Discovered by Coggia at the Marseilles Observatory, July 18, 1890. It was quite bright, round, with central condensation comparable with a star of about tenth or eleventh magnitude. Its light rapidly diminished and it soon disappeared below the northwest horizon. Parabolic elements represent the observations quite accurately, though they show some resemblance to those of the comet of 1580.

Comet 1890 IV: = Comet *e* 1890. This comet was discovered three months and a half after perihelion passage by Zona at Palermo, November 15, 1890. It was at first quite bright, but grew fainter rapidly, though it was still observed after the close of the year.

Comet 1890 V: = Comet *d* 1890. = d'Arrest's comet. An ephemeris for d'Arrest's periodic comet had been prepared by Leveau, and the comet was looked for without success for some time, and it was feared that it had gone by undetected, when it was picked up by Barnard at the Lick Observatory on October 6, as an entirely unexpected object. On the first few nights the comet was extremely faint and diffused, but it was seen later with a $3\frac{1}{4}$-inch finder.

Comet 1890 VI: = Comet *c* 1890. Discovered by W. F. Denning at Bristol, England, July 23, with a 10-inch reflector, a faint, round nebulosity, about 1′ diameter with faint central condensation, and quite near ϑ and ζ Ursæ Minoris. It moved directly towards the equator, and was visible till November, having a small stellar nucleus of the thirteenth magnitude, and a faint diffused tail.

Comet 1890 VII: = Comet *f* 1890. This comet, the most interesting perhaps of those found during the year, on account of its short period, was discovered by R. Spitaler at Vienna, November 16, 1890.

Dr. Spitaler, in looking for the comet discovered by Zona, turned the 27-inch telescope towards the place which it should occupy, according to the dispatch received by him, and immediately perceived a very faint comet, but concluding from the description that Zona's was brighter, by turning the telescope a little he found the latter, physical connection between the two being excluded by the slower motion of his own. The period appears to be about 6.4 years.

Approximate elements of the comets of 1889 *and* 1890.*

Designation.	Perihelion=T Greenwich mean time.		Ω	ω	i	q	e
	1889.		° ′	° ′	° ′		
1889 I	Jan.	31.21	357 25	340 29	166 22	1.815	....
II	June	10.82	310 42	236 6	163 51	2.256	....
III	June	20.78	270 58	60 8	31 13	1.110	0.957
IV	July	19.27	286 11	345 52	65 57	1.040	....
V	Sept.	30.01	17 58	343 28	6 4	1.950	....
VI	Nov.	29.82	330 25	70 1	10 15	1.354	0.683
1890 I	Jan.	26.48	8 29	199 52	56 45	0.270	....
II	June	1.47	320 21	68 54	120 34	1.908	....
III	July	8.65	14 25	85 53	63 15	0.766	....
IV	Aug.	7.09	85 23	331 19	154 18	2.047	....
V	Sept.	16.	146 5	172 58	15 43	....	0.627
VI	Sept.	24.48	100 7	163 0	98 56	1.260	....
VII	Oct.	26.56	45 6	12 51	12 51	1.818	0.472

Designation.	Discoverer.	Date of discovery.	Synonym.	Remarks.
		1888,'9.		
1889 I	Barnard	Sept. 2	1888 *e*	
II	....do	Mar. 31	1889 *b*	
III	....do	June 23	1889 *c*	Elliptic?
IV	Davidson	July 21	1889 *e*	
V	Brooks	July 6	1889 *d*	Lexell's?
VI	Swift	Nov. 17	1889 *f*	
1890 I	Borrelly	Dec. 12	1889 *g*	
		1890.		
II	Brooks	Mar. 19	1890 *a*	
III	Coggia	July 18	1890 *b*	
IV	Zona	Nov. 15	1890 *e*	
V	Barnard	Oct. 6	1890 *d*	d'Arrest's.
VI	Denning	July 23	1890 *c*	
VII	Spitaler	Nov. 16	1890 *f*	Period 6.4 years.

* See Astronomical Journal, Nos. 212 and 238.

METEORS.

A valuable résumé of meteoric astronomy has been published by Prof. J. R. Eastman in the Bulletin of the Philosophical Society of Washington. (Vol. XI.) Abstracts of the various theories propounded in ex-

planation of meteors are given, and extensive catalogues of observed meteors and meteorites.

Mr. Denning pointed out several years ago that there were a number of meteor streams in which the meteors seemed to radiate from the same point in the sky for a period of three months or more. The only explanation of this phenomenon seemed to be that the meteors were moving with frightful velocity through space, but M. Tisserand, from a mathematical study of the problem, shows that these meteors do not all come from the same stream; they may perhaps belong to a family presenting certain common characteristics, but they are in reality different streams accidentally falling together, a not very improbable assumption considering the great number of meteor streams and the difficulty of determining the radiant with any degree of precision.

Mr. Denning does not, however, admit that an accidental coincidence of radiant points of different streams is a sufficient explanation of the phenomena he has observed.

THE ZODIACAL LIGHT.—Prof. Arthur Searle, who has made a special study of the zodiacal light, finds that the permanence of the ordinary western light, subject only to slight variations in the degree of visibility, is confirmed by the observations of the last 50 years at the Hanaud Observatory. The zodiacal bands, which are said to form a prolongation of the ordinary zodiacal light, were not seen, though stellar or nebulous bands, one extending from Aquila to the Pleiades, and the second from Præsepe to Coma Berenices have been noticed and perhaps offer an explanation of the zodiacal bands. The Gegenschein, it is suggested, may be due to a maximum of light reflected from the meteoric matter scattered in the solar system.

The observations of Prof. C. Michi Smith, carried on at intervals since 1875 indicate a periodic appearance of the line at wave-length 553 in the zodiacal light spectrum; a line differing but little in wave-length from the auroral line (wave-length 556.7).

PLANETS.

A very laborious work is being carried on in the office of the American Ephemeris, under the superintendence of Professor Newcomb—the re-determination of the elements of all the larger planets. Professor Newcomb's plan includes the re-reduction of the older planetary observations and the discussion of the later ones, with a view of reducing them all to a uniform system. Another branch of this planetary work is a determination of the mass of Jupiter from the motions of Polyhymnia, and a comparison of Hansen's tables of the moon with observed occultations since 1750.

The first volume of this series of memoirs upon the theories of the major planets has appeared in the "Astronomical Papers," of the American ephemeris, being a new discussion of Jupiter and Saturn by Hill. He has determined the complete analytical expressions for the

coördinates of these two planets, giving also a provisional comparison of his theory with observations. The method followed is in general that of Hansen.

In commenting upon recent determinations of planetary masses from the motions of comets, Professor Hall says:

"The objection to deducing values of planetary masses from the motions of comets consists, I think, in the fact that apparently other forces than that of gravitation act on these bodies. As a comet approaches the sun it changes form, disintegrates, and matter is thrown off to form a tail. Until we know more of the theory of these changes the computation of masses from the motions of comets and inferences about the resisting medium in space must be uncertain."

MERCURY.—The observations of Schroter early in the present century indicated that Mercury had a motion of rotation about its axis of about 24 hours. Subsequent observers failed, however, to confirm his observations, and the question of Mercury's rotation has generally been regarded as one of the unsettled problems of astronomy. M. Schiaparelli, taking advantage of the clear sky of Milan, has observed Mercury since 1881, obtaining about one hundred and fifty sketches, showing quite well-marked spots, from which he has deduced a rotation period of 88 days, the same, in fact, as the period of rotation of the planet around the sun. Schiaparelli also concludes that the axis of rotation must be nearly perpendicular to the orbit of the planet, the rotation being uniform.

Dr. von Hærdtl has obtained the following values for the mass of Mercury:

I. Mass of Mercury, 1 : 5,012,842 from Winnecke's comet.
II. Mass of Mercury, 1 : 5,514,700 Le Verrier's equation modified.
III. Mass of Mercury, 1 : 5,648,600 Encke's comet, 1819–1868.
IV. Mass of Mercury, 1 : 5,669,700 Encke's comet, 1871–1885.

VENUS.—Schiaparelli has concluded, from an exhaustive rediscussion of all the older observations, combined with his own observations of 1877 and 1878, that Venus rotates upon its axis in 225 days, or the same time that it rotates about the sun, contrary to the generally received hypothesis that its rotation period is about 23 hours. Venus, then, as well as Mercury, would seem to turn always the same face to the sun, as the moon turns the same face to the earth.

THE EARTH— *Variation of latitude.*—The subject of the change of terrestrial latitudes, to which allusion has been made in previous reports, continues to receive considerable attention from astronomers and geographers. The following results have been obtained by Dr. Küstner, in continuation of his former researches, from 7 pairs of stars at three different times of the year:

Epoch.	Latitude of Berlin.
1884.32	$+52° 30' 16''.73 - 0.82 \varDelta A$
1884.70	$16''.96 + 0.83 \varDelta A$
1885.31	$16''.52 - 0.85 \varDelta A$

where Δ A represents the correction to the assumed constant of aberration. The direct inference from these figures is that in 7 months the latitude of Berlin decreased 0″.44. Pulkowa showed about the same time a similar change:

Epoch.	Latitude of Pulkowa.
1882.31	+59° 46′ 18″.52
1883.51	18″.54
1884.70	18″.63
1885.23	18″.31
1885.31	18″.30

a decrease of 0″.33 from 1884.70 to 1885.31.

The general agreement of these results certainly calls for further investigation; and to test the matter Mr. Preston has been sent out by the U. S. Coast Survey, and Dr. Marcuse by the International Geodetic Commission, to Honolulu, which is at the opposite end of the earth's diameter from Berlin, and by simultaneous observations at these two stations it is hoped the question will be settled.

It is quite possible that the origin of the apparent change at Berlin in 1884–1885 is meteorological, a view to which Dr. Foerster inclined in bringing the matter before the Association Géodésique in 1888. The whole question is, then, whether there are changes in the disposition of atmospheric strata sufficient to account for the facts observed, or the axis of rotation and the axis of inertia of the earth are not sensibly coincident.

A complete résumé of the subject is given by M. Tisserand in the *Bulletin Astronomique* for September, 1890.

Mr. Riccò has experimented with a somewhat novel demonstration of the rotundity of the earth. At the observatory of Palermo, which is situated at a distance of 1¼ miles from the Mediterranean and 236 feet above its level, a great number of photographs of the sun reflected from the surface of the water have been taken a few minutes after rising or before setting, and they show that the diameter in the plane of reflection is less in the reflected image than in the direct. This deformation is due to the fact that the surface of the water forms a cylindrical mirror, with axis horizontal and normal to the plane of reflection. The amount of the observed flattening accords well with that demanded by theory.

Standard time.—The introduction of the system of standard time, which has been found of such practical usefulness in the United States, has been quietly agitated in other countries for several years past, and a well-written article upon the subject by Dr. Robert Schram will be found in the *Observatory* for April, 1890. The adoption of a uniform time system, the time of the fifteenth meridian east of Greenwich, has been very favorably looked upon in Austria and Germany for railroad purposes.

Of the proposed change of the beginning of the astronomical day from midday to the preceding midnight nothing has been heard since the original agitation of the subject at the time of the Meridian Conference at Washington in 1884.

The moon's physical libration.—Dr. Julius Franz of the Königsberg observatory has done an excellent piece of work in bringing to light and discussing (vol. 38, Königsberg Beobachtungen) the observations of the moon made by Schlüter, an assistant of Bessel's, in 1841–1843, the work having been undertaken by Schlüter under the immediate supervision of his distinguished chief. The observations were continued by Wichmann after Schlüter's death, but Wichmann was never able to do more than to reduce his own observations for preliminary results to be used in a discussion of all the material available.

Dr. Franz recommends the substitution of observations of the spot Mösting A for those of the limbs, in determining the moon's place, a method upon which a report was published by the late Dr. C. H. F. Peters in the U. S. Coast Survey volume for 1856.

Temperature of the moon.—A memoir on the temperature of the moon by Mr. S. P. Langley forms a part of the fourth volume of the publications of the National Academy of Sciences, and is re-published in a somewhat abbreviated form in the *American Journal of Science* for December, 1889. The paper may be regarded as the completion of a piece of work commenced in 1883, and represented by papers read in 1884 and 1886, as well as the present one. The principal conclusion drawn is "that the mean temperature of the sunlit lunar soil is much lower than has been supposed, and is most probably not greatly above zero centigrade." The principle by which this temperature is estimated is that the position of the maximum in a curve, representing invisible radiant heat of different wave-lengths, furnishes a criterion as to the temperature of the radiating solid body. In the lunar spectrum two distinct heat maxima are found—one corresponding to radiation reflected from the soil, the other to that emitted by it (when warmed by sunshine). The determination of the second maximum with accuracy would give an accurate value for the temperature of the sunlit soil; but, unfortunately, the absorption-bands produced by the earth's atmosphere obscure this maximum, and render the conclusions somewhat uncertain; so that Professor Langley is compelled to state his principal conclusion in a guarded manner, as above quoted.

The Proceedings of the American Academy of Arts and Sciences (vol. 24) contains an account of some measures of lunar radiation made by Mr. C. C. Hutchins, by means of a new thermograph which he has devised. This instrument consists of a single thermal junction of nickel and iron placed in the focus of a small concave mirror, and is found to be much more sensitive than a thermopile of forty-eight couples. The measures of lunar radiation were made with an arrangement similar to that of a Herschel's telescope with the thermograph in place of an eye

piece, the conclusion reached being that the heat which the earth receives from the moon is to that from the sun as 1 is to 184,560. From observations during the eclipse of January 28, 1888, Mr. Hutchins infers that all but a minute portion of the rays from the lunar soil and rock are cut off by our atmosphere, as it seems impossible that a surface like that of the moon, upon which the sun has been shining for many days, should suddenly cease to radiate when the sun's light is withdrawn.

MARS.—Durin gthe opposition of 1890 Mars again received special attention from the Lick observers. Experiments were tried with colored glasses, with diminished apertures, etc., all with small success. Many photographs were also secured, but none that were pronounced satisfactory. The mystery of the "canals" is still further increased by the fact that while Professor Holden and Mr. Keeler always saw the canals as dark, broad, somewhat diffused bands, and Mr. Schaeberle saw them in the same way when the seeing was bad, but under good conditions described them as narrow lines a second of arc or so in width. On April 12 Mr. Schaeberle saw two of the canals doubled, thereby verifying Professor Schiaparelli's observations. The positions of most of the canals have also been verified by some of the Lick astronomers.

JUPITER.—Mr. J. E. Keeler publishes in the monthly notices for November a drawing of Jupiter made with the Lick 36-inch on the night of August 28, 1890. The great red spot is described as being of about the same dimensions as in 1889, with a dark shading at its following end, but the middle whiter and the arrangement of belts somewhat different. "It would seem, on the whole, that the surface features of Jupiter indicate less activity in the internal forces of the planet than was manifest a year ago."

Barnard and Burnham have reported a very curious doubling of the first satellite as seen with the 12-inch equatorial of the Lick observatory. Of this phenomenon there seems to be but two possible explanations: either there is a white belt on the satellite parallel to the belts of Jupiter or the satellite is actually double.

M. Belopolsky has brought out from an examination of drawings of Jupiter a peculiar variation in the time of rotation (first noted by Cassini) with the latitude. A velocity of $9^h\ 51^m$ was found in the zone 0° to 5° in both hemispheres, and a time of rotation of $9^h\ 55.5^m$ for the remainder of the surface, both hemispheres, except between 5° and 10° of north and south latitude, where the two velocities appear to occur with equal frequency.

SATURN.—A peculiar white spot on the rings of Saturn attracted considerable attention in the early part of 1889. This spot was first seen by Dr. Terby, of Louvain, on March 6, 1889, who reported it as adjacent to the shadow of the ball and similar to the white spots sometimes seen upon Jupiter; on March 12 it was again seen with an 8 inch Clark telescope, but on the 15th, 20th, 22d, and 23d, and on April 2, it was

invisible. While several observers confirmed Dr. Terby's discovery, nothing to correspond sufficiently with his description could be made out by others, though provided with much more powerful apparatus. Professor Hall has expressed the opinion that it was an optical effect of contrast.

The very fine division of the outer ring detected with the 36-inch Lick refractor early in 1888 was again seen in 1889 at a distance of about one-sixth of the breadth of ring A from its outer edge. A dark shading extended inwards from the new division almost to the inner edge of the ring. Professor Holden has noted also an extremely narrow brighter polar cap about 5 seconds of arc wide, in a direction parallel to the equator, and perpendicular to this about the width of the Cassini division at the ansæ.

An interesting monograph on Saturn, the result of fourteen years work, is contributed by Prof. Asaph Hall as Appendix II to the Washington Observations, 1885. The characteristic of this memoir is great caution, and the three drawings of the planet, where a few scanty markings represent all that Professor Hall can certainly see with a fine telescope, should re-assure those who have been dissatisfied with their modest instruments because they could not therewith recognize the elaborate detail described by more imaginative observers. To quote the author's own words: "The appearance of Saturn in our 26-inch refractor undergoes great changes from night to night, and sometimes even from hour to hour during the same night. Probably these changes are due to variations in our own atmosphere and in the action of the objective, and they do not therefore indicate real changes in the planet. Whenever we have a steady and transparent atmosphere, the outlines of the planet, the faint belts and markings on the ball, the shadow of the ball on the ring, the dusky ring, and the Cassini division are clear and distinct, and the abnormal phenomena sometimes seen are not visible. Without exception, my experience is that on good nights the planet always has this natural appearance. But on poor nights, when the image is blazing and unsteady, one can see and imagine many strange things about this wonderful object."

Professor Hall finds for the rotation period of the planet from observations of the white spot (1876, December 7 to 1877, January 2) $10^h\ 14^m\ 23^s.8 \pm 2^s.3$ mean time (see Astron. Nachr. No. 2146). Careful discussions are also given of the position and dimensions of the ring.

The notch in the outline of the shadow was never seen at Washington, either by Professor Hall or his assistant. "The curvature of the outline of the shadow presented an anomaly in 1876 when the convexity appeared to be turned towards the ball, contrary to what we should expect from geometrical considerations. The notes show that something of this kind was seen after the re-appearance of the ring in 1878. After the ring was well opened, the curvature of the outline always appeared natural or turned away from the ball." (*Observatory.*)

The last determination of the thickness of Saturn's ring, as Professor Hall has pointed out, was made in 1848 by W. C. Bond, who found that it was less than 0.″01; Duséjour estimated its thickness at 0.″2, and Schroeter at 0.″13. At the disappearance of the ring in September and October, 1891, the conditions of observation are not very favorable, a better opportunity occurring in 1892.

In connection with the approaching disappearance of the ring, an account of observations made by M. E. L. Trouvelot upon the passage of the sun and earth through the plane of the rings in 1877–'78 is of especial interest.

Saturn's satellites.—Dr. Hermann Struve has published the second installment of his work on the theory of Saturn's satellites. In this he discusses the orbits of Mimas and Enceladus, and their connection with the other satellites, and he has been able to account satisfactorily for the large corrections to the computed position of Mimas required during the past few years. In his previous paper Dr. Struve was led to assume a sensible mass for the ring-system of Saturn, but he now concludes that this hypothesis must be rejected, the mass of the ring being so small that the terms to which it would independently give rise in the disturbing function are as yet undetected by observation.

A determination of the orbit of Titan and the mass of Saturn, the result of several years' work with the Yale observatory heliometer, is published by Mr. Asaph Hall, jr., in the Transactions of the Yale Observatory, 1889. His value for Saturn's mass is 1:3500.5 ± 1.44, agreeing well with Bessel's value 1 : 3502, and that obtained by Struve 1:3498.

URANUS.—Dr. Huggins has found evidence of solar lines in the photographic spectrum of Uranus, with an exposure of two hours (June 3, 1889). All the principal solar lines were seen, but no others either bright or dark. Mr. Taylor, on the other hand, has reported bright flutings seen with a direct vision spectroscope attached to the five-foot reflecter of Common's observatory, Ealing, and if this observation is confirmed it will of course prove that the planet is at least in part self-luminous.

THE MINOR PLANETS.

The discovery of additional members of the zone of asteroids goes on without the least signs of abatement, and the number has now reached 301, no fewer than 6 having been found in 1889, and 14 in 1890. Twice during 1890 (April 25 and September 9) two were discovered on the same evening by the same observer; and the two discovered by Palisa on April 25 were independently discovered by Charlois on the following evening, April 26.

List of minor planets discovered in 1889 *and* 1890.

Number.	Name.	Discoverer.	Date of discovery.
			1889.
282	Clorinda	Charlois, at Nice	Jan. 28.
283	Emma	do	Feb. 8.
284	Amelia	do	May 29.
285	Regina	Palisa, at Vienna	Aug. 3.
286		Charlois, at Nice	Do.
287	Nephthys	Peters, at Clinton	Aug. 25.
			1890.
288	Glauke	R. Luther, at Düsseldorf	Feb. 20.
289	Nenetta	Charlois, at Nice	Mar. 10.
290	Bruna	Palisa, at Vienna	Mar. 20.
291	Alice	do	Apr. 25.
292	Ludovica	do	Do.
293	Brasilia	Charlois, at Nice	May 20.
294	Félicia	do	July 15.
295	Theresia	Palisa, at Vienna	Aug. 17.
296		Charlois, at Nice	Aug. 19.
297		do	Sept. 9.
298		do	Do.
299		Palisa, at Vienna	Oct. 6.
300		Charlois, at Nice	Oct. 3.
301		Palisa, at Vienna	Nov. 16.

An asteroid discovered by Charlois, November 14, 1890, and supposed by him to be 298 (discovered September 9), proved to be not identical with the latter. Consequently it takes the number 302.

SOLAR SYSTEM.

Prof. Lewis Boss has made a new determination of the amount and direction of the solar motion based upon a list of 253 stellar proper motions derived from the Albany zone observations. Professor Boss considers, as the most probable result from these data, that the apex of the sun's way is in right ascension $18^h\ 40^m$; declination $+40°$, or not far from the star Vega.

Herr Oscar Stumpe, of Born, has made a new determination of the direction of the solar motion from the proper motions of 1,054 stars, which he divides into four groups, according to the magnitudes of their proper motions in a great circle. He thus obtains four different values of the apex of the sun's way, all agreeing in locating that point in the constellation Lyra, or in the adjacent part of Cygnus.

Prof. J. R. Eastman, in an address as president of the Philosphical Society of Washington, has given an analysis of the investigations to determine the apex of the sun's motion and its velocity of translation. He shows that, contrary to the ordinarily accepted belief, faint stars are nearer us than bright stars; a result also shown by the list of stellar parallaxes recently published by Oudemans.

SUN.

Rotation of the sun.—Mr. Crew, whose observations of the rotation of the sun were noted in a previous summary, has made a new series of observations for the correction or confirmation of his conclusion that the angular velocity of rotation increased with an increase of latitude. He still finds shorter rotation periods for the higher latitudes, the mean value for the period at latitude 45° being 18 hours shorter than at the equator, though owing to the smallness of this amount and the uncertainty of the observations he is of the opinion that "no certain variation of period with latitude has been detected with the spectroscope." Attention is called however to the wide differences of the equatorial period as obtained by different methods, differences which may be due to the fact that we are really dealing with different strata of the sun, though here also much reliance must not be placed upon the observations.

Spectroscopic observations made by Dunér for determining the rotation time of the sun, confirm the slowing down of the time of rotation with an increase of heliocentric latititude, quite contrary to the result recently obtained by Wilsing. A period of 25.46 days is deduced for the sidereal rotation at the equator, and 38.54 days for that at latitude 74.8°.

Diameter of the sun.—Dr. Auwers discusses, in the third memoir on the diameter of the sun, communicated to the Berlin Academy, the observations at Greenwich by Maskelyne and his assistants from 1765 to 1810. Curious differences of personal equation between different observers are brought out. Instead of Maskelyne's observations giving progressively smaller values of the sun's diameter during his whole observing life, as has hitherto been supposed, Dr. Auwers's very exhaustive discussion indicates that after the first two years (which gave a very large value) the observed diameter remained nearly constant for the period 1767–1772, then during the years 1772–1790 the diameter was continually decreasing, lastly from 1790–1810 the observations gave a diameter continually increasing. The minimum value in 1790 was 31′ 58″.13—about 1″ smaller than the value obtained from modern heliometer measures.

Spoerer's researches on sun spots.—Professor Spoerer, who has devoted much attention not only to the current state of the solar activity, but also to the early records of sun spots, published early in 1889 two important papers on the results of his researches in the latter field. The two papers are entitled respectively, *Ueber die Periodicität der Sonnenflecken seit dem Jahre* 1618, communicated to the Royal Leopold-Caroline Academy, and *Sur les différences que présentent l'hémisphère nord et l'hémisphère sud du Soleil*, appearing in the number of *Bulletin Astronomique* for February, 1889. The conclusions arrived at in these two papers may be summarized under the three following heads:

First: These earlier observations afford us many examples of the operation of the "law of zones;" that is to say, a little before a mini-

mum spots are only seen in low latitudes, at about the time of minimum spots near the equator cease to appear, while a fresh series of spots break out a great distance from it, and thenceforward to the next minimum the mean heliographic latitude of the spots tends to decline continuously, until at length spots are again seen only in the vicinity of the equator. This law held good, Professor Spoerer shows, for the minima of 1619, 1755, 1775, 1784, 1833, and 1844, and to some extent for that of 1645.

Second: Though in general a predominance of spots for a time in one hemisphere is sooner or later balanced by a corresponding predominance in the other, this is not always the case, and Professor Spoerer calls attention to three periods in which the southern hemisphere was decidedly the more prolific. The first was from 1621 to 1625, there being no northern spots in 1621 and 1622, and but few in the three following years. Another is the present period, for from 1883 to the present time the southern spots have been nearly twice as numerous as the northern. But the third was the most remarkable, for from 1672 to 1704 we have no record of any northern spots at all; and Cassini and Maraldi expressly declared, on the appearance of a northern spot in 1705, that they did not recollect ever to have observed a spot in that hemisphere before. Northern spots continued to be infrequent until 1714.

Third: For a period of about seventy years, ending in 1716, there seems to have been a very remarkable interruption of the ordinary course of the spot cycle. In several years no spots appear to have been seen at all, and in 1705 it was recorded as a most remarkable event that two spots were seen on the sun at the same time, for a similar circumstance had scarcely ever been seen during the sixty years previous. So far as the observations go, the "law of zones" also seems to have been in abeyance, for no regular drift was apparent, the mean latitude being low—about 8° or 9°—during the entire time.

Professor Spoerer is still continuing his researches into ancient sun-spot records, and hopes to be able to examine the manuscripts of Plantade (1705–1726) and of Flaugergues (1794–1830). (E. W. M. *Monthly Notices* R. A. S., February, 1890).

Attention should be directed to a paper in the Monthly Notices for December, 1890, by Rev. A. L. Cortie, S. J., on the sun-spot observations made at Stonyhurst in the years 1882–'89.

A comparison of sun-spot statistics for 1878 with the records of 1889 gives a sun-spot period of exactly 11 years, and it seems probable that the real minimum occurred about the end of 1889. This probability is increased by the appearance on March 4, 1890, of a large spot in heliographic latitude +34°, which during its period of visibility in a semi-rotation of the sun passed within one-sixth of the sun's diameter from its northern edge. During the whole of the year 1889 the southern hemisphere of the sun manifested greater activity than the northern;

protuberances were seen, according to Tacchini, in both hemispheres at high latitudes where there were neither spots nor faculæ; there were also zones with spots and without faculæ.

Mr. Lockyer has presented a second report to the Solar Physics committee on the observations of sun-spot spectra made at South Kensington. He finds that the observations (to February, 1888) confirm the conclusion which he arrived at in 1886, that "as we pass from minimum to maximum the lines of the chemical elements gradually disappear from among those most widened, their places being taken by lines of which we have at present no terrestrial representatives.

SOLAR SPECTRUM.

Thollon's chart of the solar spectrum.—In 1879 Thollon presented to the Académie des Sciences a map of the solar spectrum, extending from A to H, made with his great spectroscope. His work was renewed with more perfect apparatus, but on account of the great labor of the undertaking he confined himself to the region from A to *b;* this was presented to the Academy in 1885, and gained the Lalande prize. Thollon continued this work until his death, and it has now been published in 33 maps with a total length of $10^{m}.23$ (33.6 feet), and contains about 3,200 lines, between the limits adopted, A and *b*, the positions of which were determined from 252 sharp lines adopted as "fundamentals."

Thollon made special efforts to distinguish the telluric rays from those entirely due to the sun; and with this end in view he observed the sun at different altitudes, noting the hygrometric conditions of the air. Of these 3,200 lines mapped, 2,090 were of solar origin, 866 telluric, and 246 mixed, that is to say resulting from the superposition of telluric and solar lines. The breadth and intensity of each line is given upon an arbitrary scale.

M. Bigourdan, in a review of Thollon's work, published in the May number of the Bulletin Astronomique, says that for the part of the spectrum studied no work is comparable with that of Thollon except the magnificent photographs of Rowland, and he finds upon a critical comparison of different regions of considerable extent that Rowland's photographs contain no lines not upon Thollon's chart, though the faintest lines given upon the chart are frequently lacking in the photographs. Between wave-lengths 5,262 and 5,337, for example, in Rowland's photograph, there are not half the number of lines that there are upon Thollon's chart, though it is probable that the original negatives would not show so large a difference.

Rowland's determination of elements in the sun.—Professor Rowland's examination by photography of the spectra of 58 elements and their comparison with the spectrum of the sun shows the existence in the sun of 35 different elements; the existence of 8 more in the sun is doubtful, while of 10 he finds no trace. The element represented by the greatest number of lines is iron, there being 2,000 or more lines in the spectrum

of iron found also in the solar spectrum. Iron is followed by nickel, titanium, manganese, chromium, cobalt, carbon, with decreasing frequency of coincidences, ending with lead and potassium, for which but one line is found in common with the sun.

The full list of elements in the sun, arranged according to the intensity and the number of lines in the solar spectrum, is as follows:

Elements in the sun, arranged according to the intensity and the number of lines in the solar spectrum.

According to intensity.		*According to number.*	
Calcium.	Zirconium.	Iron (2,000 +).	Magnesium (20 +).
Iron.	Molybdenum.	Nickel.	Sodium.
Hydrogen.	Lanthanum.	Titanium.	Silicon.
Sodium.	Niobium.	Manganese.	Strontium.
Nickel.	Palladium.	Chromium.	Barium.
Magnesium.	Neodymium.	Cobalt.	Aluminium (4).
Cobalt.	Copper.	Carbon (200 +).	Cadmium
Silicon.	Zinc.	Vanadium.	Rhodium.
Aluminium.	Cadmium.	Zirconium.	Erbium.
Titanium.	Cerium.	Cerium.	Zinc.
Chromium.	Glucinum.	Calcium (75 +).	Copper (2).
Manganese.	Germanium.	Scandium.	Silver (2).
Strontium.	Rhodium.	Neodymium.	Glucinum (2).
Vanadium.	Silver.	Lanthanum.	Germanium.
Barium.	Tin.	Yttrium.	Tin.
Carbon.	Lead.	Niobium.	Lead (1).
Scandium.	Erbium.	Molybdenum.	Potassium.
Yttrium.	Potassium.	Palladium.	

Doubtful elements.

Iridium.	Platinum.	Tantalum.	Tungsten.
Osmium.	Ruthenium.	Thorium.	Uranium.

Not in solar spectrum.

Antimony.	Cæsium.	Rubidium.
Arsenic.	Gold.	Selenium.
Bismuth.	Indium.	Sulphur.
Boron.	Mercury.	Thallium.
Nitrogen (vacuum tube).	Phosphorus.	Preæseodymium.

Substances not yet tried.

Bromine.	Iodine.	Oxygen.	Gallium.	Thulium.
Chlorine.	Fluorine.	Tellurium.	Holmium.	Terbium, etc.

Professor Rowland says: "With the high dispersion here used the 'basic lines' of Lockyer are widely broken up and cease to exist. Indeed it would be difficult to prove anything except accidental coincidences among the lines of the different elements. Accurate investigation generally reveals some slight difference of wave length or a common impurity. Furthermore, the strength of the lines in the solar

spectrum is generally very nearly the same as that in the electric arc, with only a few exceptions, as, for instance, calcium. The cases mentioned by Lockyer are generally those where he mistakes groups of lines for single lines or even mistakes the character of the line entirely. Altogether there seems to be very little evidence of the breaking up of the elements in the sun, as far as my experiments go."

M. Janssen, in August, 1890, repeated the observations that he made in 1888, upon Mont Blanc, this time ascending to the summit. He confirmed completely his former result that the lines of the spectrum due to the action of oxygen in our atmosphere diminish with the altitude, indicating that at the limit of the atmosphere these rays would disappear entirely and in consequence that oxygen is not actually present in the sun's atmosphere. This conclusion had already received confirmation from a series of observations of the spectrum of an electric light placed on the Eiffel Tower, as viewed from the observatory at Meudon.

ECLIPSES.

Eclipses of 1889, *and* 1890.—During the year 1889, there were five eclipses, three of the sun and two of the moon; and during 1890, three eclipses, two of the sun and one of the moon. Two of the solar eclipses of 1889 were total, and one of 1890 was total over a portion of the central line.

The Almanac records also a lunar appulse on June 2, 1890, the nearness of the approach and the uncertainty as to the effect of the earth's atmosphere rendering it doubtful whether the moon would actually enter the earth's shadow. Of the eclipses of the moon nothing of especial interest has been reported. A brief summary of the observations of the solar eclipses is given below;

Total eclipse of the sun January 1, 1889.—The event of chief astronomical interest in the year 1889, was the eclipse of the sun on New Year's day, the last total solar eclipse visible in the United States in this century. The line of central eclipse crossed California, Nevada, Idaho, Wyoming, Montana, and Dakota, the width of the belt of totality being about 96 miles in California; the partial phases of the eclipse were visible over the greater part of North America, first contact being observed at Washington a few minutes before sunset. Ample preparations were made for utilizing the less than two minutes of totality, and printed circulars suggesting to amateur observers the most efficient manner of employing the means at their command were widely circulated. The most thoroughly equipped party in the field was that from the Harvard observatory under the charge of Prof. W. H. Pickering, at Willows, California. This party alone secured between 50 and 60 photographs taken with 14 telescopes or cameras and 8 spectroscopes, one of the telescopes being of 13 inches aperture, the largest ever used in observing a total solar eclipse. A party from the Lick observatory

under Mr. Keeler was at Bartlett Springs; one from Washington University observatory, St. Louis, under Prof. H. S. Pritchett at Norman; one from Carleton College observatory under Professor Payne at Chico; and many other available points were occupied by individual astronomers or photographers. At Cloverdale the Pacific Coast Amateur Photographic Association was represented by 30 cameras.

Professor Holden has published a full report of the Lick observatory party and its coöperators—the frontispiece being an admirable photograph of the corona by Barnard, taken with a telescope of $3\frac{1}{4}$ inches aperture stopped down to $1\frac{3}{4}$ inches. Professor's Holden's "conclusions" in which he summarizes the observations are as follows:

I. That the characteristic coronal forms seem to vary periodically as the sun spots (and auroras) vary in frequency, and that the coronas of 1867, 1878, and 1889 are of the same strongly marked type, which corresponds, therefore, to an epoch of minimum solar activity.

II. That so-called "polar" rays exist at all latitudes on the sun's surface, and are better seen at the poles of the sun, simply because they are there projected against the dark background of the sky and not against the equatorial extensions of the outer corona. There appears to be also a second kind of rays or beams that are connected with the ring-like extensions. These are parts of the "groups of synclinal structure" of Mr. Ranyard.

III. The outer corona of 1889 terminated in branching forms. These branching forms of the outer corona *suggest* the presence of streams of meteorites near the sun, which, by their reflected light and by their native brilliancy, due to the collisions of their individual members, *may* account for the phenomena of the outer corona.

IV. The disposition of the extensions of the outer corona along and very near the plane of the ecliptic might seem to show that, if the streams of meteorites above referred to really exist, they have long been integral parts of the solar system.

V. The photographs of the corona which were taken just before contact II and just after contact III prove the corona to be a solar appendage, and are fatal to the theory that any large part of the coronal forms are produced by diffraction. - - -

VI. The spectroscopic observations of Mr. Keeler show conclusively that the length of a coronal line is not always an indication of the depth of the gaseous coronal atmosphere of the sun at that point, and hence to indicate the important conclusion that the true atmosphere of the sun may be comparatively shallow.

VII. Mr. Keeler draws the further conclusion in his report - - - that the "polar" rays are due to beams of light from brighter areas of the sun illuminating the suspended particles of the sun's gaseous envel-

NOTE.—The conclusions III and IV appear to be contradictory to that expressed in I. The electrical theory announced by Dr. Hygius in the Bakerian lecture for 1885 seems to reconcile the conclusions I, III, and IV.

opes. In order that the conclusion may stand it is necessary to show that all these "polar" beams are composed of rectilinear rays. - - - An important conclusion from [the photographic and photometric] measures seems to be that it is impracticable to photograph the corona in full sunshine with our present plates, and that a photographic search for Vulcan is hopeless.

The Smithsonian Institution has published a series of photographs of the corona of this eclipse made by different observers and reduced for convenience to a uniform scale, and has also published a suggestive paper by Prof. F. H. Bigelow tracing a close agreement between magnetic lines of force computed for the sun and the curves of the polar filaments shown upon the Pickering photograph.

Eclipse of the sun 1889, *June* 27.—An annular eclipse visible in the southern part of Africa. Dr. Auwers and Dr. Gill obtained a number of measures of the cusps with the Cape heliometer.

Eclipse of the sun 1889, *December* 21–22.—Three principal points were available as observing stations: the southwest corner of the island of Trinidad totality lasting 1^{m} 46^{s}; Cayenne on the coast of French Guiana, totality 2^{m} 3^{s}; and Cape Lado a point on the western coast of Africa just south of St. Paul de Loanda, totality 3^{m} 12^{s}. Two expeditions went out to Africa, one sent by the United States Government under Prof. D. P. Todd, and provided with most elaborate apparatus, and the other from the Royal Astronomical Society of England under the direction of Mr. A. Taylor. Cloudy weather prevented both of these parties from securing observations. Another party from the Royal Astronomical Society under Father Perry, at the Salut Islands, was partially successful as far as observations go, but resulted most disastrously in the death of Father Perry from dysentery within a few days after the eclipse. M. de la Baume Pluvinel was also at the Salut Islands and secured a number of photographs. The Lick observatory party at Cayenne, Messrs. Burnham, Schaeberle and Rockwell, were successful; securing good photographs.

Eclipse of the sun 1890, *June* 17.—The annular eclipse of June 17, 1890, was central over portions of Northern Africa and Southern Asia, and was visible as a partial eclipse over the whole of Europe. In the southern part of Italy three-fourths of the sun's disk was covered by the moon. Observations partially successful were obtained by Professor Ricco at Palermo. At Canea, M. de la Baume Pluvinel secured several photographs of the partial and annular phases, and also of the spectrum of the annulus, the latter proving to be the same as the ordinary solar spectrum.

Eclipse of the sun 1890, *December* 11.—A total eclipse of the sun occurred on December 11, 1890, the central line being confined to the ocean south of Australia. In consequence of the earth's globular surface, the eclipse was annular at the beginning and end, and total between 13^{h} $55^{m}.3$ and 16^{h} $20^{m}.5$ Greenwich mean time. In portions of

Australia, and in Tasmania, and in New Zealand, it was visible as a partial eclipse. No observation of special interest was reported.

Mr. J. M. Schaeberle has published in the *Monthly Notices* a theory of the solar corona, in which he concludes that the corona is due to the light emitted and reflected by the filaments of matter thrown out by the sun, the corresponding forces being variable and with a period about the same as the sun-spot period. The rays of double curvature are explained by the rotation of the sun, and the apparent changes in the general form of the corona by the position of the observer with reference to the plane of the sun's equator.

The Smithsonian Institution published in 1889 a series of re-productions of a number of photographs of the eclipse of January 1, 1889, sent from various stations on the Pacific coast. The photographs are for convenience of comparison reduced to a uniform scale of about 1 inch diameter. Explanatory notes and remarks suggested by a study of the photographs are added by Prof. David P. Todd.

Mr. H. H. Turner in the Philosophical Transactions (vol. 180, p. 385–393) discusses the observations of the eclipse of August 29, 1886, made at the island of Grenada.

SOLAR PARALLAX AND THE TRANSITS OF VENUS.

Transits of Venus in 1761 *and* 1769.—A thorough, and probably the final, re-reduction of the observations of the transits of Venus in 1761 and 1769 has been made by Professor Newcomb in volume 2, part 5, of the astronomical papers of the American Ephemeris, a primary object being the determination of the position of the node of Venus. The value obtained for the solar parallax is $8''.79$ with a probable error of $+ 0''.034$.

Professor Harkness of the U. S. Naval Observatory has devoted several years of work to an elaborate discussion of the solar parallax and its related constants. His principal results are elsewhere referred to, the definitive value for the solar parallax being $8''.80905 \pm 0''.00567$.

The French photographs of the transit of Venus give for the solar parallax the value $8''.80 \pm 0''.06$.

OBSERVATORIES.

Information in regard to the work going on at astronomical observatories has been derived from the reports contained in the *Vierteljahrsschrift*, in the *Monthly Notices*, and in Loewy's *Observatoires astronomiques de Provence*, and also from the separate reports published by a few observatories. The compiler is indebted in some instances to directors of observatories who have communicated to him directly data in relation to the institutions under their charge. When it has seemed necessary to make a distinction, the year has been added to the note.

ALLEGHENY: *Langley*.—Work upon radiant energy has been continued, and the time service has been maintained as in previous years.

ALGIERS: *Trépied.*—A meridian circle of $0^m.19$ (7.5 inches) and an equatorial of $0^m.12$ (4.7 inches) have been added to the equipment. Observations have been made upon a catalogue of 10,000 stars in the zone $-18°$ to $-23°$. It is expected that the photographic equatorial will soon be installed. (1889.)

ARMAGH: *Dreyer.*—Observations of nebulæ and physical observations of Jupiter and Saturn; time service.

BASEL: *Riggenbach.*—Devoted entirely to the instruction of students.

BERLIN: *W. Foerster.*—Observations with the transit circle, observations with the 9-inch equatorial of asteroids, comets, and double stars, and with the small transit of comparison stars and stars occulted by the moon.

BESANÇON: *Gruey.*—Observations of comets; horology. The observatory possesses an equatorial coudé.

BIRR CASTLE: *Lord Rosse.*—Preparing for publication a series of sketches of the milky way; measures of lunar heat during the eclipse of January 28, 1888, have been reduced.

BONN: *Schönfeld.*—Zone observations $+40°$ to $+50°$ with the transit circle. Reductions in a forward state. (1889.)

BORDEAUX: *Rayet.*—Preparations are being made for observing the zone $-20°$ to $-25°$. The photographic equatorial has been mounted. (1889.)

BRESLAU: *Galle.*—Chiefly magnetic and meteorological work. Small transit used for time service.

CAMBRIDGE (England): *Adams.*—Mr. Newall has presented his 25-inch refractor to the university observatory, and the university authorities have voted to spend about $11,000 on its installation near the present observatory, and to appoint an observer, at $1,200 per annum, to devote himself to research in stellar physics. It is understood that the work with this instrument will be under the charge of Mr. H. F. Newall.

Volume 22 of the publications has been issued and deals with the observations from 1866 to 1869.

CAMDEN.—The amateur astronomical society at Camden, New Jersey, has a small observatory, with 5½-inch equatorial, transit instrument, chronograph, clock, etc.

CAPE OF GOOD HOPE: *Gill.*—With the meridian circle regular observations have been made of the sun, Mercury, Venus, comparison stars, stars occulted by the moon, etc. The heliometer has been constantly in use and much attention has been given to astronomical photography. Prof. J. C. Kapteyn has measured definitively 389 negatives of the plates of the southern photographic Durchmusterung, covering 8,769 square degrees of the sky. This work represents 489,490 observations of about 193,000 stars, or about 63 per cent. of the whole work.

Dr. Gill, the astronomer royal for the Cape, and Dr. Auwers, of Berlin, by taking alternate watches of observation (June 10 to August 26, 1889) secured an admirable series of observations of Victoria, which was in an exceptionally favorable position for determining the solar parallax. A large part of Dr. Gill's report for 1889 is devoted to the geodetic work which is under his direction.

CARLETON COLLEGE: *Payne.*—The first volume of publications consists of a catalogue of 644 comparison stars observed with the Repsold meridian circle, by Dr. Wilson.

CATANIA: *Riccò.*—The observatory recently founded at Catania will be chiefly devoted to astrophysics, photography, meteorology, and seismology. It contains a Merz refractor of $0^{m}.35$ (13.8 inches) aperture, one by Cooke of $0^{m}.15$ (5.9 inches), and a photographic telescope, by Steinheil, which will be used for photographing the zone $+12^{\circ}$ to $+6^{\circ}$. (1890.)

CHAMBERLIN: *H. A. Howe.*—The disks for the 20-inch refractor are being worked by Clark, and the mounting is well advanced at the shop of Fauth & Co., Washington. The initial publication of the new observatory is a report upon observations of the eclipse of January 1, 1889.

DEARBORN: *Hough.*—An illustrated description of the new observatory at Evanston will be found in the Sidereal Messenger for October, 1889.

DENVER.—In addition to the working observatory founded by Mr. Chamberlin, an observatory for students is in course of erection. A 6-inch equatorial and a 3-inch transit have been ordered.

DENVER. (*See*, also, Chamberlin.)

DRESDEN: *von Engelhardt.*—Observations of nebulæ star-clusters and comets. Baron von Engelhardt has recently published a second part of his "Observations Astronomiques," containing principally measures of double stars, star charts, nebulæ, and comets. (1889).

DUNSINK: *Ball.*—A new reflecting telescope of 15 inches aperture has been presented to the observatory by Mr. Isaac Roberts for photographic researches on stellar parallax.

DÜSSELDORF: *Luther.*—Observations of small planets, and computation of their ephemerides. Since 1847, 1,474 observations of 172 asteroids have been made. (1889).

EDINBURGH: *Copeland.*—The site for a new observatory building two minutes of arc south of the present observatory was selected in 1889. The plans have been completed and it is hoped that the work of construction will soon be begun. It is interesting to note that though the new site is within 500 yards of the suburban railway, the porphyrite rock of which the hill consists does not appear to transmit any perceptible vibration from the railway even when the heaviest trains are passing. Dr. Becker has continued his determinations of the positions of nebulæ and work in stellar and solar spectroscopy.

GEORGETOWN: *Hagen.*—Observations of variable stars have been made systematically, and experiments in photographic observations of star transits by Father Hagen and his assistant, Father Fargis.

GENEVA: *Gautier.*—Chiefly engaged in testing chronometers and watches. Observations of the sun and of comets have been made with the equatorial. Dr. Raoul Gautier has been appointed professor of astronomy and director of the observatory, Col. E. Gautier retaining the title of honorary director.

GLASGOW (England): *Grant.* Transit circle observations.

GÖTTINGEN: *Schur.*—Heliometer used in measuring Præsepe, Pleiades, and double stars. (1889.)

GREENWICH: *Christie.*—In the report for 1889 it is noted that the observations with the transit circle by reflexion have been much facilitated and improved by using an amalgamated copper-bottom mercury trough for the artificial horizon. Two photographic objectives have been tried, one of 6 inches aperture to be used as a pilot for the 13-inch star-charting telescope stars, and the other of 4 inches in connection with the 28-inch refractor.

The annual visitation in 1890 took place on June 7. The collection of historical instruments and the new photographic equatorial especially attracted the attention of some 300 visitors present. It is proposed to put up a large new building with four wings to relieve the overcrowded condition of the older buildings. It is expected that the new 28-inch refractor will be installed at an early day. The 13-inch photographic equatorial was received from Grubb on March 17, 1890, and was mounted and made ready for use. The astronomer royal reported that the work of the observatory had proceeded without essential modification.

"The observations for the longitude of Paris made in 1888 have now been completely reduced and the definitive results found by the French and English observers are respectively, $9^m\ 21^s.04$ and $9^m\ 20^s.84$. In view of this unsatisfactory discordance - - - it seems desirable that the determination should be repeated with interchange of instruments as well as of observers."

The 1887 volume of Greenwich observations contains among its appendices the *ten-year catalogue* deduced from observations made from 1877 to 1886. The total number of stars is 4,059, the positions being given for 1880. 0

HARVARD COLLEGE: *Pickering.*—Miss C. W. Bruce, of New York, has made a gift of $50,000 to the Harvard observatory to be applied to the construction and maintenance of a photographic telescope having an objective of about 24 inches aperture and a focal length of 11 feet. The figuring of the lens has been intrusted to Alvan Clark, who has experienced some difficulty in securing proper glass. The Bache 8-inch telescope of similar construction has been in constant use in Cambridge

for four years, and is now in Peru photographing the southern sky; with it stars too faint to be seen with the 15-inch refractor have been photographed, and a corresponding advantage is anticipated from the increase of the aperture to 24 inches.

Volume 17 of the *Annals* is now completed and consists of the following papers, which have been separately printed and distributed during the last few years: I. Magnitudes of stars employed in various nautical almanacs; II. Discussion of the Uranometria Oxoniensis; III. Photometric observations of asteroids; IV. Total eclipse of the moon, 1888, January 28; V. Total eclipse of the sun, 1886, August 29; VI. Detection of new nebulæ by photography; VII. A photographic determination of the brightness of the stars; VIII. Index to observations of variable stars; IX. Meridian-circle observations of close north polar stars; X. Meridian-circle observations of close south polar stars.

Volume 21, part 1, contains the observations of the New England Meteorological Society made during 1888. Volume 22 contains a long series of meteorological observations made on the summit of Pike's Peak, Colorado, between January, 1874, and June, 1888, by U. S. Army Signal Service observers.

KALOCSA: *Fenyi.*—Physical observations of the sun. (1889.)

KEW: *Whipple.*—Meteorological, magnetic, and solar observations.

KIEL: *Krueger.*—The catalogue of zone +55° to +65° has been published. Computation of the orbits of comets and asteroids.

KÖNIGSBERG: *C. F. W. Peters.*—Observations of zone +83° to +90°; also heliometer observations of wide double stars. (1889.)

KREMSMÜNSTER: *Wagner.*—Observations of comets and asteroids; time service.

LEIPZIG: *Bruns.*—Observations of zone +5° to +10°; observations with the heliometer; time service.

LUND: *Möller.*—Spectroscopic observations to determine the sun's rotation period. The printing of the Zone Catalogue is in progress. The second volume of Zone Observations, +36° to +40°, has been published.

LYNN (*Massachusetts*).—Private observatory of Mr. C. W. Wilson. Latitude +42°.5, longitude 71° west. The principal instrument is one of Alvan Clark & Sons' 6-inch refractors of unusual excellence.

LYONS: *André.*—Meridian work; physical observations of the sun and of Jupiter.

MCCORMICK: *Stone.*—Chiefly engaged in observations of double stars and nebulæ. Volume 1, part 4, of the Publications contains double-star measures made in 1885 and 1886 by Leavenworth and Muller.

MARSEILLES: *Stephan.*—Revision of Rümker's Catalogue; observations of comets, asteroids, nebulæ and variable stars. (1889.)

MELBOURNE: *Ellery.*—Transit-circle observations, observations of comets and astroids and of stellar spectra. The great reflector has been repolished, and its performance is reported as improved. The photographic telescope for the international chart work has been received and mounted. The Second Melbourne General Catalogue of Stars, containing 1,211 stars and embodying the results of observations made with the old transit circle from the beginning of 1871, has been published.

MILAN: *Schiaparelli.*—The 18-inch equatorial was used for double-star measures; the observations of Mercury, 1881–'88, were discussed, and the rotation period determined. (1889.)

MUNICH: *Seeliger.*—Work on a catalogue of 33,082 stars; observations of comets and measures of the star cluster in Perseus.

NATAL: *Nevill.*—Observations of the position of the moon. There has been formed a manuscript catalogue of about 4,000 observations of right ascensions of zodiacal stars used in determining the places of the moon during the years 1883–'88. Time service.

NICE: *Perrotin.*—Charlois has been remarkably successful in his search for new asteroids. The third volume of Annals contains a new chart of the solar spectrum by Thollon, the concluding part of the discussion of the theory of Vesta by Perrotin, and the observations made in the years 1887–'88.

O'GYALLA: *Konkoly.*—Observations of sun spots and meteors; photographic researches.

OXFORD UNIVERSITY: *Pritchard.*—Experimental work on the new photographic objectives by Grubb has occupied much time; the parallaxes of six more stars have been determined by photography. (1890.)

PARIS: *Mouchez.*—The large transit circle has been used for the sun, planets, and stars of Lalande's catalogue; the Gambey transit for observation of fundamental stars in groups of 24 to 48 hours; the Gambey circle for experiments on flexure and the determination of latitude; comets and nebulæ have been observed with the west equatorial, and the equatorial coudé has been used in determining the constants of refraction and aberration. The work for which the Paris observatory has been especially known of late years, astronomical photography, has been actively pursued by the Henrys. The frontispiece of Admiral Mouchez's report for 1889 is a representation of the great equatorial coudé of 18 metres focal length and 0.6 metre (23.62 inches) aperture. Attention has been given to photographing of stellar spectra by placing prisms of 22° or 45° in front of the objective of the telescope, and Admiral Mouchez has announced that spectroscopic observations will form a regular part of the observatory work in future.

POTSDAM: *Vogel.*—Astrophysical work, determination of the motion of stars in the line of sight by means of photography; spectrum analysis in general; photometric measures of large planets and a photometric

Durchmusterung of the northern sky; observations of sun spots. The new refractor for the photographic star chart is erected and some experimental work has been done. (1889.)

PRAG: *Safarik.*—Double-star measures; drawings of the moon; chiefly devoted to observations of variable stars. (1889.)

PRAG (University observatory): *Weinek.*—Drawings of moon; occultations. Time service. (1889.)

PULKOWA: *Bredichin.*—Prof. Otto Struve retired from the directorship of the observatory, which he has held for over 25 years, and has been succeeded by Dr. Bredichin, formerly director of the observatory at Moscow. Three volumes were issued in 1889: Volume 8 containing the catalogue of Bradley's stars, a volume containing an investigation by Lindemann of the photometric scale of the Bonn Durchmusterung, and the third volume, the "Jubilee" volume, with an historical account of the observatory for 25 years, a monograph on the 30-inch refractor, and a description of the astrophysical observatory. The volume contains several fine engravings of the observatory and 30-inch. (1889–'90.)

RADCLIFFE: *Stone.*—Transit-circle observations of the zone 0°–15°, and of the sun and moon.

ROME: *Denza.*—The new observatory of the Vatican has been built partly upon the site of the old observatory, founded in 1582, and partly upon a tower dating from the time of Leo IV. Special attention will be given to astronomical photography.

ROUSDON (Lyme Regis): *Peek.*—Observations of variables. Time service.

STOCKHOLM: *Gyldén.*—Largely engaged in mathematical researches upon orbits. Photographs have been taken of the Pleiades and of a region extending about 4° around the north pole. (1889.)

STONYHURST: *Sidgreaves.*—Father Perry, whose sad death immediately after observing the total eclipse of the sun on December 21, 22, 1889, has been elsewhere referred to, has been succeeded in the directorship of the observatory by Father Walter Sidgreaves. (1889.)

STRASSBURG: *E. Becker.*—Observations of comets and heliometer measures of the sun's diameter; also transit-circle observations of the sun and major planets.

SYDNEY: *Russell.*—Transit-circle observations, and with the 11½-inch equatorial observations of comets and of double stars. The photographic telescope for chart work has been mounted upon an elevated site 620 feet above the sea and 11 miles inland from the present observatory. Each instrument has its own group of accumulators, conveniently charged by the help of a gas engine.

SMITHSONIAN ASTRO-PHYSICAL OBSERVATORY: *Langley.*—An astrophysical observatory has been established as a department of the Smithsonian Institution at Washington, occupying at present a tem-

porary building in the Smithsonian grounds, erected in 1889–'90. The principal instruments are a very large siderostat by Grubb, a large spectro-bolometer, special galvanometer, and resistance box. Researches in telluric and astro-physics will be carried on.

SWARTHMORE COLLEGE: *Miss S. J. Cunningham*.—The observatory building contains four rooms: A transit room, in which is a 3-inch Warner and Swasey transit and mean-time clock; a pier room at present utilized as a sidereal clock room; a work room containing the chronograph, chronometer, and a small reference library; and the dome, in which is a 6-inch Warner and Swasey equatorial. Connected with the observatory is the signal service station of the state weather service, fully provided with the necessary meteorological and other apparatus. (1890.)

TACUBAYA: *Anguiano*.—The construction of the new observatory has progressed favorably, the photographic department being entirely finished and the instruments mounted. The photographic equatorial is by Grubb, of the pattern adopted by the astrophotographic congress in 1889 and furnished for most of the observatories taking part in the international chart. Among the minor apparatus added to the equipment of the observatory may be mentioned a complete portable photographic outfit; a Merz polariscope for the 15-inch equatorial; a Pritchard's wedge photometer by Hilger; a mercury artificial horizon by Gauthier for the meridian circle; a complete meteorological outfit; a petroleum motor and electric light installation.

In August, 1889, two additions were made to the observatory staff, Messrs. Camilo A. Gonzalez and Guillermo Puga, who have been assigned to duty on the meridian circle. They have been engaged in studying the instrumental constants and have undertaken the observation of certain stars to the tenth magnitude, conveniently situated for reference stars for the zone of the photographic map assigned to the Tacubaya observatory. Sr. Felipe Valle has been engaged with the equatorial in observations of comets, asteroids, and nebulæ.

A series of daily observations of sun spots and faculæ has been made. Photographs of the sun have been taken with the photoheliograph. Two parties were sent out to observe the total solar eclipse of October 22, 1889, one to Yucatan and one to San Luis Potosi. (1890.)

TANANARIVO: *Colin*.—An observatory has been established on a hill about 4,400 feet high a short distance to the east of Tananarivo on the island of Madagascar. It has an equatorial, meridian instrument, and photographic telescope for solar work. (1889.)

TOKYŌ: *Terao*.—A large number of observations of comet *e*, 1888, made by Professor Teara and Mr. J. Midzuhara have been published as the second fasciculus of volume 1 of the *Annals*. (1889.)

TOULOUSE: *Baillaud*.—The photographic telescope has been mounted.

UNITED STATES NAVAL OBSERVATORY: *McNair.*—The reports of the superintendents of the Naval Observatory show no material change in the character of the work from the years immediately preceding. On June 28, 1890, Capt. F. V. McNair succeeded Capt. R. L. Phythian as Superintendent, Capt. McNair's report covering the fiscal year June 30, 1890. The walls of the main building for the new observatory were practically completed by the end of 1890; also the great equatorial and clock and observer's rooms. The iron work for the three transit-circle rooms is ready. The buildings will scarcely be ready for occupancy before the summer of 1892.

UPSALA: *Dunér.*—From an extensive series of spectroscopic observations to determine the rotation period of the sun, it appears that the period varies from 25.5 days to 38.6 days, increasing with the heliographic latitude.

VIENNA (*von Kuffner's* observatory): *Herz.*—The latitude from observations with the Repsold meridian circle, 1889-'90, is $+48° 12' 46''.67$.

WASHINGTON (Catholic University of America): *Searle.*—A small observatory has been built at the Catholic University in the suburbs of Washington (D. C.), and is under the direction of Rev. G. M. Searle. The position is latitude $+38° 56' 15''$; longitude $5^h 8^m 0^s.0$ west of Greenwich. The telescope, which will be mounted in 1891 is 9 inches aperture, 9 feet focus, glass and tube by Clacey, mounting by Saegmuller (Fauth & Co.). The cells and center piece of tube are made of aluminum. A small meridian circle, and photographic and spectroscopic apparatus will also be provided. A 5-inch telescope is now in use. (1890.)

WASHINGTON. (*See*, also, Georgetown; also, Smithsonian astrophysical observatory; also, U. S. Naval Observatory.)

WASHBURN: *Comstock.*—The sixth volume of publications contains the meridian observations of 1887 and observations of double stars.

YALE: *Newton.*—The heliometer triangulation of the region near the north pole has been completed, and some observations of Iris, Victoria and Sappho have been obtained in coöperation with the observatories at the Cape of Good Hope and Leipsic, for the determination of the solar parallax.

ZURICH: *Wolf.*—Physical observations of the sun.

ASTRONOMICAL INSTRUMENTS.

In the fourth part of the Bulletin of the Astro-photographic congress, Dr. H. C. Vogel describes the photographic refractor constructed for the observatory at Potsdam by the Repsolds. This instrument has two objectives; eye-piece and plate-holder are in the same tube, conforming to the resolutions of the congress in 1887, but the peculiarity is in the form of mounting, which is quite different from both the Eng-

lish and the French forms. The pillar that supports the polar axis is not upright, but L-shaped, the lower part being inclined nearly in the plane of the equator, the upper almost at right angles to this, extending toward the north pole and inclosing the polar axis. The support possesses very great stability, and its form permits an uninterrupted motion of the telescope in all positions.

In *Engineering* for December 19, 1890, will be found a description of the Melbourne photographic telescope made by Sir Howard Grubb.

An instrument for comparing and measuring celestial photographs, somewhat similar to that designed by Mr. Roberts, has been devised by Mr. Common.

An apparatus for eliminating personal equation in the observation of sudden phenomena, such as the disappearance of a star when occulted by the moon has been devised by Mr. S. P. Langley, and is described in the Bulletin of the Philosophical Society of Washington, vol. XI. The principle of the method consists in associating a motion, real or apparent, of the object, with intervals of time so that the apparent position of the object at the instant of the occurrence of any phenomenon being noted the time of the occurrence will be known. Experiments made with artificial stars show that it is quite possible for a comparatively inexperienced person to observe an occultation with a probable error of only one-fortieth of a second.

The great Lick refractor of 36 inches diameter is to be surpassed by one still larger, ordered for the University of Southern California, at Los Angeles. This lens is to be 40 inches in diameter, and the crown glass disk for the achromatic combination is now in the hands of the Clarks, who pronounce it a remarkably fine piece of glass.

It may perhaps be mentioned here that a bill was introduced in the United States Congress making an appropriation of $1,000,000 for a refractor of 5 feet aperture for the U. S. Naval Observatory, but the plan never received support from the Government astronomers.

Mr. Brashear has under way at his shop in Allegheny a 16-inch objective for Carleton College Observatory, one of 12 inches for Brown University, and a second of 12 inches for Mr. G. E. Hale, of Chicago. He is also making a large spectroscope and spectrograph for Professor Young, at Princeton, which is expected to be the finest in the United States; a very complete spectroscope with Jena glass objectives and prism is being made for Carleton College, and a new star spectroscope for Lick Observatory. For the Willard photographic telescope of the Lick Observatory, he is making an equatorial mounting with controlled clock.

MISCELLANEOUS.

Personal equation.—The attention of astronomers interested in the subject of personal equation should be directed to a paper prepared by a physiologist, Dr. E. C. Sanford, of the Johns Hopkins University,

and published in volume 2 of the American Journal of Psychology. An important contribution to the astronomical side of the subject is an investigation by Dr. Wislicenus, of the Strasburg Observatory, who has investigated the personal equation in transit observations, not only for a horizontal position of the telescope, but for all inclinations. By placing a small convex lens behind the ocular an artificial star is obtained which is easily moved in the plane of the reticule with a velocity corresponding to any declination. Dr. Wislicenus concludes from his experiments that the inclination of the telescope has a considerable effect upon the observer's personal equation.

One of the essays contributed to the celebration of the Pulkowa Jubilee was a discussion of absolute personal equation by H. G. van de Sande Bakhuyzen. The artificial star observed was the meridian mark of the transit circle, to which an apparent motion was given by interposing a prism fixed excentrically to a circular rotating plate. Very satisfactory results were obtained. The personality depending upon direction of apparent motion seemed to be generally small for seven observers who tried the apparatus.

ASTRONOMICAL SOCIETIES.

The Astronomical Society of the Pacific.—Under the leadership of Professor Holden and the astronomers at the Lick Observatory the Astronomical Society of the Pacific was founded February 7, 1889, as a result of the cordial coöperation of amateur and professional astronomers in successfully observing the total solar eclipse of the preceding New Year's day. Any person interested in astronomy is invited to join its membership. Three meetings each year are held in San Francisco and three meetings at Mount Hamilton. An excellent series of publications, in octavo form, issued at irregular intervals, has reached the second volume. These "publications" contain papers read before the society, and also notices from the Lick Observatory prepared by members of the observatory staff. A fund has been established known as the "Donohoe fund for the maintenance of the comet medal of the Astronomical Society of the Pacific," the principal conditions of the gift, a medal of bronze, being the discovery of a new comet or the first precise determination of position of a periodic comet at any one of its expected returns. The discoverer is to make his discovery known in the usual way, and also to communicate it immediately to the director of the Lick Observatory. No application for the bestowal of the medal is required.

The British Astronomical Association.—A new astronomical society, to be called the British Astronomical Association, has been formed in England to meet the wishes and needs of those who find the subscription of the Royal Astronomical Society too high, or its papers too advanced, or who are, as in the case of ladies, practically excluded from becoming fellows; it is also to afford a means of direction and organization in the work of observation to amateur astronomers. The

new society is thus to be regarded as supplementary to the older one, and not its rival. The first general meeting was held on October 24, 1890, in the hall of the Society of Arts, Adelphi, London, and the officers nominated by a provisional committee were elected, Capt. W. Noble being made president. The sections under which the work of observation is organized are: Meteoric, solar, lunar, spectroscopic, and photographic, colored stars, variable stars, double stars, and Jupiter, each section being presided over by an amateur astronomer who has devoted special attention to the subject named. The first number of the Journal appeared in October, 1890, under the able editorship of Mr. E. W. Maunder.

Gesellschaft Urania.—The building forming the headquarters of the Gesellschaft Urania was completed in July, 1889, and is described at some length by Dr. M. W. Meyer in the February and March numbers of *Himmel und Erde.* The Gesellschaft is for the purpose of popularizing science. The chief astronomical instrument is a 12-inch refractor by Bamberg, the glass for which was made by Schott & Co., of Jena. There are also a 6-inch and a 4-inch refractor, a 6-inch reflector, a 2½-inch transit, and a 5-inch comet-seeker. These instruments are for the use of visitors, and for cloudy nights a collection of 700 lantern slides is provided.

The thirteenth meeting of the Astronomische Gesellschaft was held at Brussels, Septembr 10 to 12, 1889. The next meeting is at Munich in 1891.

Astronomical prizes.—The Lalande prize of the French Academy of Sciences was awarded for 1889 to M. Gonnessiat of the Lyons observatory, the Valz prize to Charlois, and the Janssen prize to Lockyer.

In 1890 the Lalande prize was awarded to Schiaparelli for his observations determining the rotation of Mercury and Venus, the Valz prize to Glasenapp for his determination of the orbits of double stars, and the Janssen prize to Young. The Damoiseau prize, for which but one memoir was presented, was continued for another year with the same subject: To perfect the theory of the inequalities of long period caused by the planets in the motion of the moon.

The Copley medal of the Royal Society was awarded on November 20, 1890, to Professor Simon Newcomb for his contributions to gravitational astronomy.

The first award of the Donohoe medal was made to Mr. W. R. Brooks for the discovery of a comet on March 19, 1890; the second to Mr. W. F. Denning for his comet of July 23, 1890, and the third to Monsieur Jérôme Coggia, astronomer of the observatory of Marseilles, for his discovery of a comet on July 18, 1890, this being the eighth comet discovered by M. Coggia.

A generous gift has been made in aid of astronomical research by Miss C. W. Bruce, of New York, who placed in the hands of Professor Pickering, director of the Harvard Observatory, $6,000. In answer to a circular

ssued by Professor Pickering, numerous requests were received for aid rom this fund, and various sums were awarded by Professor Pickering o as to aid as wide a range of astronomical subjects as possible, and to id investigators in all parts of the world.

Among new works of general interest to astronomers may be menioned Miss Clerke's "The System of the Stars;" a new edition of Chambers' Astronomy in three volumes. The first two volumes of an ble "Traité de mécanique céleste," the first containing the general heory of perturbations, and the second on the figures of rotation of elestial bodies; these are to be followed by a third volume on the lunar heory, theory of Jupiter's satellites, Hansen's method for the calcuation of perturbations, and other methods of recent date. Another vork which has been found useful as a text-book is Dziobek's *Die nathematischen Theorien der Planeton*–Bewegungen.

Dr. Scheiner has published a treatise on spectrum analysis which is ntended to form the first volume of complete work on astrophysics.

The first volume of the national edition of the works of Galileo has ppeared under the patronage of the King of Italy.

Dr. Dreyer has published a biography of Tycho Brahe upon which e has been at work for several years past.

A very interesting paper on Bowditch, who translated Laplace's "Mécanique Céleste," has been contributed by Prof. Joseph Lovering o the Proceedings of the American Academy of Sciences.

An index to the literature of spectroscopy, compiled by Mr. Alfred Tuckerman, has been published in the Smithsonian Miscellaneous Colections. It contains a bibliography of the history of the subjects; of ooks; of apparatus; of spectrum analysis in general; of qualitative nalysis; of quantitative analysis; of absorption spectra; of alkalies nd alkaloids; of astronomical spectroscopy; of carbon compounds, and f the spectra of metals; there is also a list of 799 authors. The numer of titles is 3,829.

Another useful contribution to astronomical bibliography is the catague of the Crawford Library at the Royal Observatory at Edinburgh, resented to the observatory by the Earl of Crawford, and formerly onstituting the library of the Dun Echt Observatory. The catalogue vas compiled by the present astronomer royal for Scotland, Mr. Copeand, and contains a number of rare works.

Reference should also be made to a new edition of M. Lancaster's useul little *Liste générale des observatoires*, appearing in 1890 with many dditions and corrections.

ASTRONOMICAL BIBLIOGRAPHY FOR 1889.

A brief bibliography of astronomy for the year 1890 having been conributed to the *Sidereal Messenger* for 1891, it seems unnecessary to over more than the year 1889 in the present review. The titles given elow include the most important books and journal articles of 1889, that

have come under the compiler's notice, some few titles having been ta
from reviews or catalogues, where the publications themselves have
been accessible.

In the reference to periodicals the volume and page are simply
arated by a colon; thus: Astron. Jour. 8:153 indicates volume 8, p
153, of the Astronomical Journal. The following less obvious abbre
tions occur:

Abstr. = Abstract.
Am. = American.
Bd. = Band.
d. = di, der, del, etc.
ed. = edition.
Hft. = Heft.
hrsg. = herausgegeben.
il. = illustrated.
j., jour. = journal.
k. k. = kaiserlich, königlich.
Lfg. = Lieferung.
M. = Marks.
n. d. = no date.
n. p. = no place of publication.
n F. = neue Folge.
n. s. = new series.
Not. = Notices.
Obsvy. = Observatory.
p. = page.
pl. = plates.
portr. = portrait.
pt. = part.
r. = reale.
Rev. = Review.
s. = series.
sc. = science, scientific.
vol. = volumes.

NECROLOGY OF ASTRONOMERS FOR 1889–'90.

Biographical sketches of most of the following astronomers are to
found in the columns of the *Astronomische Nachrichten*, in the *Vier
jahrschrift, der Astronomischen Gesellschaft*, or in the *Monthly Notice*
the Royal Astronomical Society.

ADOLPH (CARL). Born at Nordstemmen, Hanover, April 8, 1838; died Januar
1890.

CACCIATORE (GAETANO). Born at Palermo March 17, 1814; died at Palermo Jun
1889, *æt.* 75.

DE LARUE (WARREN). Born at Guernsey January 18, 1815; died April 19, 1
æt. 74.

ERCK (WENTWORTH). Born in Dublin, 1827; died at Sherrington, Wicklow, J
uary 15, 1890, *æt.* 63.

FEARNLEY, (CARL FREDERIK). Born at Frederiksbald December 19, 1818;
August 22, 1890, *æt* 72.

FIEVEZ (CHARLES). Died February 2, 1890, *æt* 46.

MONTIGNY (C. M. V.). Died at Schaerbeck, March 16, 1890, *æt* 71.

NEWALL (ROBERT STIRLING). Born in Dundee May 27, 1812; died April 21, 1
æt. 77.

OOM (FRÉDERICO AUGUSTO). Born at Lisbon December 4, 1830; died at Lisbon J
24, 1880, *æt* 60.

PERRY (STEPHEN JOSEPH). Born in London August 26, 1833; died at sea near D
arara, December 25, 1889, *æt.* 56.

PETERS (CHRISTIAN HEINRICH FRIEDRICH). Born at Coldenbüttel, Schleswig, S
tember 19, 1813; died at Clinton, New York, July 19, 1890, *æt.* 77.

RESPIGHI (LORENZO). Born at Cortemaggiore, Pracenza, October 7, 1824; died
Rome December 10, 1889, *æt.* 75.

SENBERGER (OTTO AUGUST). Born at Tukkum, Russia, August 10, 1800; died at Halle January 23, 1890, *æt.* 90.

IULTZ (HERMAN). Born at Nygvarn, Södermanland, July 7, 1823; died at Stockholm May 8, 1890, *æt.* 67.

MPEL (ERNST WILHELM LEBERECHT). Born at Nieder-Kunersdorf, Saxony, December 4, 1821; died at Arcetri March 16, 1889,* *æt.* 66.

LD (ALFRED). Born August 5, 1823; died at Grahamstown July 24, 1890, *æt.* 67.

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THE MATHEMATICAL THEORIES OF THE EARTH.*

By Robert Simpson Woodward.

The name of this section, which by your courtesy it is my duty to address to-day, implies a community of interest amongst astronomers and mathematicians. This community of interest is not difficult to explain. We can of course imagine a considerable body of astronomical facts quite independent of mathematics. We can also imagine a much larger body of mathematical facts quite independent of and isolated from astronomy. But we never think of astronomy in the large sense without recognizing its dependence on mathematics, and we never think of mathematics as a whole without considering its capital applications in astronomy.

Of all the subjects and objects of common interest to us, the Earth will easily rank first. The earth furnishes us with a stable foundation for instrumental work and a fixed line of reference, whereby it is possible to make out the orderly arrangement and procession of our solar system and to gain some inkling of other systems which lie within telescopic range. The earth furnishes us with a most attractive store of real problems; its shape, its size, its mass, its precession and nutation, its internal heat, its earthquakes, and volcanoes, and its origin and destiny, are to be classed with the leading questions for astronomical and mathematical research. We must of course recognize the claims of our friends the geologists to that indefinable something called the earth's crust, but considered in its entirety and in its relations to similar bodies of the universe, the Earth has long been the special province of astronomers and mathematicians. Since the times of Galileo and Kepler and Copernicus it has supplied a perennial stimulus to observation and investigation, and it promises to tax the resources of the ablest observers and analysts for some centuries to come. The mere mention of the names of Newton, Bradley, d'Alembert, Laplace, Fourier, Gauss, and Bessel, calls to mind not only a long list of inventions and discoveries, but the most

* Vice-presidential address before the section of Mathematics and Astronomy of the American Association for the Advancement of Science at the Toronto meeting, August, 1889. (From the *Proceedings Am. Assoc. Adv. Sci.*, vol. XXXVIII.)

important parts of mathematical literature. In its dynamical and physical aspects the Earth was to them the principal object of research, and the thoroughness and completeness of their contributions toward an explanation of the "system of the world" are still a source of wonder and admiration to all who take the trouble to examine their works.

A detailed discussion of the known properties of the earth, and of the hypotheses concerning the unknown properties, is no fit task for a summer afternoon; the intricacies and delicacies of the subject are suitable only for another season and a special audience. But it has seemed that a somewhat popular review of the state of our mathematical knowledge of the Earth might not be without interest to those already familiar with the complex details, and might also help to increase that general interest in science, the promotion of which is one of the most important functions of this association.

As we look back through the light of modern analysis, it seems strange that the successors of Newton, who took up the problem of the shape of the Earth, should have divided into hostile camps over the question whether our planet is elongated or flattened at the poles. They agreed in the opinion that the Earth is a spheroid, but they debated, investigated, and observed for nearly half a century before deciding that the spheroid is oblate rather than oblong. This was a critical question, and its decision marks perhaps the most important epoch in the history of the figure of the Earth. The Newtonian view of the oblate form found its ablest supporters in Huygens, Maupertuis, and Clairaut, while the erroneous view was maintained with great vigor by the justly distinguished Cassinian school of astronomers. Unfortunately for the Cassinians, defective measures of a meridional arc in France gave color to the false theory and furnished one of the most conspicuous instances of the deterring effect of an incorrect observation. As you well know, the point was definitely settled by Maupertuis's measurement of the Lapland arc. For this achievement his name has become famous in literature as well as in science, for his friend Voltaire congratulated him on having "flattened the poles and the Cassinis;" and Carlyle has honored him with the title of "Earth-flattener."*

Since the settlement of the question of the *form*—progress toward a knowledge of the *size* of the Earth has been consistent and steady, until now it may be said that there are few objects with which we have to deal whose dimensions are so well known as the dimensions of the Earth. But this is a popular statement, and like most such, needs to be explained in order not to be misunderstood. Both the size and shape of the Earth are defined by the lengths of its equatorial and polar axes; and, knowing the fact of the oblate spheroidal form, the lengths of the axes may be found within narrow limits from simple measure-

* Todhunter, *History of the Theories of Attraction and the Figure of the Earth.* London, 1873, vol. I, art. 195.

ments conducted on the surface quite independently of any knowledge of the interior constitution of the earth. It is evident in fact, without recourse to mathematical details, that the length of any arc, as a degree of latitude or longitude on the earth's surface, must depend on the lengths of those axes. Conversely, it is plain that the measurement of such an arc and the determination of its geographical position constitute an indirect measurement of the axes. Hence it has happened that scientific as distinguished from practical geodesy has been concerned chiefly with such linear and astronomical measurements, and the zeal with which the work has been pursued is attested by triangulations on every continent. Passing over the earlier determinations as of historical interest only, all of the really trustworthy approximations to the lengths of the axes have been made within the half century just passed. The first to appear of these approximations were the well-founded values of Airy,* published in 1830. These, however, were almost wholly overshadowed and supplanted eleven years later by the values of Bessel,† whose spheroid came to occupy a most conspicuous place in geodesy for more than a quarter of a century. Knowing as we now do that Bessel's values were considerably in error, it seems not a little remarkable that they should have been so long accepted without serious question. One obvious reason is found in the fact that a considerable lapse of time was essential for the accumulation of new data, but two other possible reasons of a different character are worthy of notice because they are interesting and instructive, whether specially applicable to this particular case or not. It seems not improbable that the close agreement of the values of Airy and Bessel, computed independently and by different methods—the greatest discrepancy being about 150 feet—may have been incautiously interpreted as a confirmation of Bessel's dimensions, and hence led to their too ready adoption. It seems also not improbable that the weight of Bessel's great name may have been too closely associated in the minds of his followers with the weights of his observations and results. The sanction of eminent authority, especially if there is added to it the stamp of an official seal, is sometimes a serious obstacle to real progress. We can not do less than accord to Bessel the first place amongst the astronomers and geodesists of his day, but this is no adequate justification for the exaggerated estimate long entertained of the precision of the elements of his spheroid.

The next step in the approximation was the important one of Clarke‡ in 1866. His new values showed an increase over Bessel's of about half a mile in the equatorial semi-axis and about three-tenths of a mile

* *Encyclopedia Metropolitana.*

† *Astronomische Nachrichten* No. 438, 1841.

‡ Comparison of Standards of Length, made at the ordnance office, Southampton, England, by Capt. A. R. Clarke, R. E. Published by order of the secretary of state for war, 1866.

in the polar semi-axis. Since 1866, General Clarke has kept pace with the accumulating data and given us so many different elements for our spheroid that it is necessary to affix a date to any of his values we may use. The later values, however, differ but slightly from the earlier ones, so that the spheroid of 1866, which has come to be pretty generally adopted, seems likely to enjoy a justly greater celebrity than that of its immediate predecessor. The probable error of the axes of this spheroid is not much greater than the hundred thousandth part,* and it is not likely that new data will change their lengths by more than a few hundred feet.

In the present state of science, therefore, it may be said that the first order of approximation to the form and dimensions of the Earth has been successfully attained. The question which follows naturally and immediately is, how much further can the approximation be carried? The answer to this question is not yet written, and the indications are not favorable for its speedy announcement. The first approximation, as we have seen, requires no knowledge of the interior density and arrangement of the earth's mass; it proceeds on the simple assumption that the sea surface is closely spheroidal. The second approximation, if it be more than a mere interpolation formula, requires a knowledge of both the density and arrangement of the constituents of the earth's mass, and especially of that part called the crust. "All astronomy," says Laplace, "rests on the stability of the earth's axis of rotation."† In a similar sense we may say all geodesy rests on the direction of the plumb line. The simple hypothesis of a spheroidal form assumes that the plumb line is everywhere coincident with the normal to the spheroid, or that the surface of the spheroid coincides with the level of the sea. But this is not quite correct. The plumb line is not in general coincident with the normal, and the actual sea level or geoid must be imagined to be an irregular surface lying partly above and partly below the ideal spheroidal surface. The deviations, it is true, are relatively small, but they are in general much greater than the unavoidable errors of observation and they are the exact numerical expression of our ignorance in this branch of geodesy. It is well known, of course, that deflections of the plumb line can sometimes be accounted for by visible masses, but on the whole it must be admitted that we possess only the vaguest notions of their cause and a most inadequate knowledge of their distribution and extent.

What is true of plumb-line deflections is about equally true of the deviations of the intensity of gravity from what may be called the spheroidial type. Given a closely spheroidal form of the sea level and it follows from the law of gravitation, as a first approximation, without

* Clarke, Col. A. R., Geodesy, Oxford, 1880, p. 319.

†"Toute l'Astronomie repose sur l'invariabilité de l'axe de rotation de la Terre á la surface du sphéroïde terrestre et sur l'uniformité de cette rotation." *Mécanique Céleste* (Paris, 1882), Tome V. p, 22.

any knowledge of the distribution of the earth's mass, that the increase of gravity varies as the square of the sine of the latitude in passing from the equator to the poles. This is the remarkable theorem of Stokes,* and it enables us to determine the form or ellipticity of the Earth by means of pendulum observations alone. It must be admitted, however, that the values of the ellipticity recently obtained in this way by the highest authorities, Clarke† and Helmert,‡ are far from satisfactory, whether we regard them in the light of their discrepancy or in the light of the different methods of computing them. In general terms we may say that the difficulty in the way of the use of pendulum observations still hinges on the treatment of local anomalies and on the question of reduction to sea level. At present, the case is one concerning which the doctors agree neither in their diagnosis nor in their remedies.

Turning attention now from the surface towards the interior, what can be said of the earth's mass as a whole, of its laws of distribution, and of the pressures that exist at great depths? Two facts, namely, the mean density and the surface density, are roughly known; a third fact, namely, the precession constant, or the ratio of the difference of the two principal moments of inertia to the greater of them, is known with something like precision. These facts lie within the domain of observation and require only the law of gravitation for their verification. Certain inferences, also, from these facts and others, have long been and still are held to be hardly less cogent and trustworthy, but before stating them it will be well to recall briefly the progress of opinion concerning this general subject during the past century and a half.

The conception of the earth as having been primitively fluid was the prevailing one among mathematicians before Clairaut published his Théorie de la Figure de la Terre in 1743. By the aid of this conception Clairaut proved the celebrated theorem which bears his name, and probably no idea in the mechanics of the earth has been more suggestive and fruitful. It was the central idea in the elaborate investigations of Laplace and received at his hands a development which his successors have found it about equally difficult to displace or to improve. From the idea of fluidity spring naturally the hydrostatical notions of pressure and level surfaces, or the arrangement of fluid masses in strata of uniform density. Hence follows, also, the notion of continuity of increase in density from the surface toward the center of the Earth. All of the principal mechanical properties and effects of the earth's mass, viz, the ellipticity, the surface density, the mean density, the precession constant, and the lunar inequalities, were correlated by Laplace §

* Stokes, G. G., *Mathematical and Physical Papers*, Cambridge University Press, 1880, vol. II.

† Geodesy, Chap. xiv.

‡ Helmert, Dr. F. R., *Die Mathematischen und Physikalischen Theorieen der Höheren Geodäsie*, Leipzig, 1880, 1884, II Teil.

§ *Mécanique Céleste*, Tome V, Livre xi.

in a single hypothesis, involving only one assumption in addition to that of original fluidity and the law of gravitation. This assumption relates to the compressibility of matter and asserts that the ratio of the increment of pressure to the increment of density is proportional to the density. Many interesting and striking conclusions follow readily from this hypothesis, but the most interesting and important are those relative to density and pressure, especially the latter, whose dominance as a factor in the mechanics of celestial masses seems destined to survive whether the hypothesis stands or falls. The hypothesis requires that, while the density increases slowly from something less than 3 at the surface to about 11 at the center of the Earth, the pressure within the mass increases rapidly below the surface, reaching a value surpassing the crushing strength of steel at the depth of a few miles and amounting at the center to no less than 3,000,000 atmospheres. The inferences, then, as distinguished from facts, are that the mass of the Earth is very nearly symmetrically disposed about its center of gravity, that pressure and density except near the surface are mutually dependent, and that the earth in reaching this stage has passed through the fluid or quasi-fluid state.

Later writers have suggested other hypotheses for a continuous distribution of the earth's mass, but none of them can be said to rival the hypothesis of Laplace. Their defects lie either in not postulating a direct connection between density and pressure or in postulating a connection which implies extreme or impossible values for these and other mechanical properties of the mass.

It is clear, from the positiveness of his language in frequent allusions to this conception of the earth, that Laplace was deeply impressed with its essential correctness. "Observations," he says, "prove incontestably that the densities of the strata (couches) of the terrestrial spheroid increase from the surface to the center,"* and "the regularity with which the observed variation in length of a second's pendulum follows the law of squares of the sines of the latitudes proves that the strata are arranged symmetrically about the center of gravity of the earth."† The more recent investigations of Stokes, to which allusion has already been made, forbid our entertaining anything like so confident an opinion of the earth's primitive fluidity or of a symmetrical and continuous arrangement of its strata. But, though it must be said that the sufficiency of Laplace's arguments has been seriously impugned, we can hardly think the probability of the correctness of his conclusions has been proportionately diminished.

* "Enfin il (Newton) regarde la terre comme homogène, ce qui est contraire aux observations, qui prouvent incontestablement que les densités des couches du sphéroïde terrestre croissent de la surface au centre." *Mécanique Céleste*, Tome v, p. 9.

† "La régularité avec laquelle la variation observée des longueurs du pendule à secondes suit la loi du carré du sinus de la latitude prouve que ces couches sont disposées régulièrement autour du centre de gravité de la terre et que leur forme est à peu près elliptique et de révolution." *Ibid.*, p. 17.

Suppose, however, that we reject the idea of original fluidity. Would not a rotating mass of the size of the earth assume finally the same aspects and properties presented by our planet? Would not pressure and centrifugal force suffice to bring about a central condensation and a symmetrical arrangement of strata similar at least to that required by the Laplacian hypothesis? Categorical answers to these questions can not be given at present. But, whatever may have been the antecedent condition of the earth's mass, the conclusion seems unavoidable that at no great depth the pressure is sufficient to break down the structural characteristics of all known substances, and hence to produce viscous flow whenever and wherever the stress difference exceeds a certain limit, which can not be large in comparison with the pressure. Purely observational evidence, also, of a highly affirmative kind in support of this conclusion, is afforded by the remarkable results of Tresca's experiments on the flow of solids and by the abundant proofs in geology of the plastic movements and viscous flow of rocks. With such views and facts in mind the fluid stage, considered indispensable by Laplace, does not appear necessary to the evolution of a planet, even if it reach the extreme refinement of a close fulfillment of some such mathematical law as that of his hypothesis. If, as is here assumed, pressure be the dominant factor in such large masses, the attainment of a stable distribution would be simply a question of time. The fluid mass might take on its normal form in a few days or a few months, whereas the viscous mass might require a few thousand or a few million years.

Some physicists and mathematicians, on the other hand, reject both the idea of existence of great pressures within the earth's mass, and the notion of an approach to continuity in the distribution of density. As representing this side of the question the views of the late M. Roche, who wrote much on the constitution of the earth, are worthy of consideration. He tells us that the very magnitude of the central pressure computed on the hypothesis of fluidity is itself a peremptory objection to that hypothesis.* According to his conception, the strata of the earth from the center outwards are substantially self-supporting and unyielding. It does not appear, however, that he had submitted this conception to the test of numbers, for a simple calculation will show that no materials of which we have any knowledge would sustain the stress in such shells or domes. If the crust of the earth were self-supporting, its crushing strength would have to be about thirty times that of the best cast steel, or five hundred to one thousand times that of granite. The views of Roche on the distribution of the terrestrial densities appear equally extreme.† He prefers to consider the mass as

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* *Mémoire sur l'état intérieur du globe terrestre,* par M. Édouard Roche; Memoires de la section des sciences de l'Académie des Sciences et Lettres de Montpellier, 1880–1884 Tome x.

† *Ibid.*

made up of two distinct parts, an outer shell or crust whose thickness is about one-sixth of the earth's radius, and a solid nucleus having little or no central condensation. The nucleus is conceived to be purely metallic, and to have about the same density as iron. To account for geological phenomena, he postulates a zone of fusion separating the crust from the nucleus. The whole hypothesis is consistently worked out in conformity with the requirements of the ellipticity, the superficial density, the mean density, and precession; so that to one who can divest his mind of the notion that pressure and continuity are important factors in the mechanics of such masses, the picture which Roche draws of the constitution of our planet will present nothing incongruous.

In a field so little explored and so inaccessible, though hedged about as we have seen by certain sharply limiting conditions, there is room for a wide range of opinion and for great freedom in the play of hypothesis; and although the preponderance of evidence appears to be in favor of a terrestrial mass in which the reign of pressure is well-nigh absolute, we should not be surprised a few decades or centuries hence to find many of our notions on this subject radically defective.

If the problem of the constitution and distribution of the earth's mass is yet an obscure and difficult one after two centuries of observation and investigation, can we report any greater degree of success in the treatment of that still older problem of the earth's internal heat; of its origin and effects? Concerning phenomena always so impressive and often so terribly destructive as those intimately connected with the terrestrial store of heat, it is natural that there should be a considerable variety of opinion. The consensus of such opinion, however, has long been in favor of the hypothesis that heat is the active cause of many and a potent factor in most of the grander phenomena which geologists assign to the earth's crust; and the prevailing interpretation of these phenomena is based on the assumption that our planet is a cooling sphere whose outer shell or crust is constantly cracked and crumpled in adjusting itself to the shrinking nucleus.

The conception that the earth was originally an intensely heated and molten mass appears to have first taken something like definite form in the minds of Leibnitz and Descartes.* But neither of these philosophers was armed with the necessary mathematical equipment to subject this conception to the test of numerical calculation. Indeed, it was not fashionable in their day, any more than it is with some philosophers in ours, to undertake the drudgery of applying the machinery of analysis to the details of an hypothesis. Nearly a century elapsed before an order of intellects capable of dealing with this class of questions appeared. It was reserved for Joseph Fourier to lay the foundation and

*Protogée, ou de la formation et des révolutions du globe, par Leibnitz, ouvrage traduite - - - avec une introduction et des notes par le Dr. Bertrand de Saint-Germain, Paris, 1859.

build a great part of the super-structure of our modern theory of heat diffusion, his avowed desire being to solve the great problem of terrestrial heat. "The question of terrestrial temperatures," he says, "has always appeared to us one of the grandest objects of cosmological studies, and we have had it principally in view in establishing the mathematical theory of heat."* This ambition however was only partly realized. Probably Fourier under-estimated the difficulties of his problem, for his most ingenious and industrious successors in the same field have made little progress beyond the limits he attained. But the work he left is a perennial index to his genius. Though quite inadequately appreciated by his contemporaries, the Analytical Theory of Heat, which appeared in 1820, is now conceded to be one of the epoch-making books. Indeed, to one who has caught the spirit of the extraordinary analysis which Fourier developed and illustrated by numerous applications in this treatise, it is evident that he opened a field whose resources are still far from being exhausted. A little later Poisson took up the same class of questions and published another great work on the mathematical theory of heat.† Poisson narrowly missed being the foremost mathematician of his day. In originality, in wealth of mathematical resources, and in breadth of grasp of physical principles he was the peer of the ablest of his contemporaries. In lucidity of exposition it would be enough to say that he was a Frenchman, but he seems to have excelled in this peculiarly national trait. His contributions to the theory of heat have been somewhat overshadowed in recent times by the earlier and perhaps more brilliant researches of Fourier, but no student can afford to take up that enticing, though difficult, theory without the aid of Poisson as well as Fourier.

It is natural, therefore, that we should inquire what opinions these great masters in the mathematics of heat diffusion held concerning the earth's store of heat. I say opinions, for, unhappily, this whole subject is still so largely a matter of opinion that, in discussing it, one may not inappropriately adopt the famous caution of Marcus Aurelius, "Remember that all is opinion." It does not appear that Fourier reached any definite conclusion on this question, though he seems to have favored the view that the Earth in cooling from an earlier state of incandescence reached finally through convection a condition in which there was a uniform distribution of heat throughout its mass. This is the *consistentior status* of Leibnitz, and it begins with the formation of the earth's crust, if not with the consolidation of the entire mass. It thus affords an initial distribution of heat and an epoch from which analysis may start, and the problem for the mathematician is to assign the subse-

* "La question des températures terrestres nous a toujours paru un des plus grands objets des études cosmologiques, et nous l'avions principalement en vue en établissant la théorie mathématique de la chaleur." *Annales de Chimie et de Physique*, 1824, tome XXVII, p. 159.

† *Théorie Mathématique de la Chaleur*, Paris, 1835.

quent distribution of heat and the resulting mechanical effects. But no great amount of reflection is necessary to convince one that the analysis can not proceed without making a few more assumptions. The assumptions which involve the least difficulty, and which for this reason, partly, have met with most favor, are that the conductivity and thermal capacity of the entire mass remain constant, and that the heat conducted to the surface of the earth passes off by the combined process of radiation, convection, and conduction, without producing any sensible effect on surrounding space. These or similar assumptions must be made before the application of theory can begin. In addition, two data are essential to numerical calculations, namely, the diffusivity, or ratio of the conductivity of the mass to its thermal capacity, and the initial uniform temperature. The first of these can be observed, approximately, at least; the second can only be estimated at present. With respect to these important points which must be considered after the adoption of the *consistentior status*, the writings of Fourier afford little light. He was content perhaps to invent and develop the exquisite analysis requisite to the treatment of such problems.

Poisson wrote much on the whole subject of terrestrial temperatures and carefully considered most of the troublesome details which lay between his theory and its application. While he admitted the nebular hypothesis and an initial fluid state of the Earth, he rejected the notion that the observed increase of underground temperature is due to a primitive store of heat. If the Earth was originally fluid by reason of its heat, a supposition which Poisson regarded quite gratuitous, he conceived that it must cool and consolidate from the center outwards; * so that according to this view the crust of our planet arrived at a condition of stability only after the supply of heat had been exhausted. But Poisson was not at a loss to account for the observed temperature gradient in the earth's crust. Always fertile in hypotheses, he advanced the idea that there exists by reason of interstellar radiations, great variations in the temperature of space, some vast regions being comparatively cool and others intensely hot, and that the present store of terrestrial heat was acquired by a journey of the solar system through one of the hotter regions. "Such is," he says, "in my opinion, the true cause of the augmentation of temperature which occurs as we descend below the surface of the globe."† This hypotheisis was the result of Poisson's mature reflection, and as such is well worthy of attention. The notion that there exist hot foci in space was advanced also in another form in 1852 by Rankine, in his interesting speculation on the re-concentration of energy. But whatever we may think of the hypothesis as a whole it does not appear to be adequate to the case of the

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* *Théorie Mathématique de la Chaleur*, Supplément de, Paris, 1837.

†"Telle est, dans mon opinion, la cause véritable de l'augmentation de température qui a lieu sur chaque verticale à mesure que l'on s'abaisse au-dessous de la surface du globe."—*Théorie Mathématique de la Chaleur*, Supplément de, p. 15.

Earth unless we suppose the epoch of transit through the hot region exceedingly remote and the temperature of that region exceedingly high. The continuity of geological and paleontological phenomena is much better satisfied by the Leibnitzian view of an earth long subject to comparatively constant surface conditions but still active with the energy of its primitive heat.

Notwithstanding the indefatigable and admirable labors of Fourier and Poisson in this field, it must be admitted that they accomplished little more than the preparation of the machinery with which their successors have sought and are still seeking to reap the harvest. The difficulties which lay in their way were not mathematical but physical. Had they been able to make out the true conditions of the earth's store of heat, they would undoubtedly have reached a high grade of perfection in the treatment of the problem. The theory as they left it was much in advance of observation, and the labors of their successors have therefore necessarily been directed largely towards the determination of the thermal properties of the earth's crust and mass.

Of those who in the present generation have contributed to our knowledge and stimulated the investigation of this subject, it is hardly necessary to say that we owe most to Sir William Thomson. He has made the question of terrestrial temperatures highly attractive and instructive to astronomers and mathematicians, and not less warmly interesting to geologists and paleontologists. Whether we are prepared to accept his conclusions or not, we must all acknowledge our indebtedness to the contributions of his master hand in this field as well as in most other fields of terrestrial physics. The contribution of special interest to us in this connection is his remarkable memoir on the secular cooling of the Earth.* In this memoir he adopts the simple hypothesis of a solid sphere whose thermal properties remain invariable while it cools by conduction from an initial state of uniform temperature, and draws therefrom certain striking limitations on geologic time. Many geologists were startled by these limitations, and geologic thought and opinion have since been widely influenced by them. It will be of interest therefore to state a little more fully and clearly the grounds from which his arguments proceed. Conceive a sphere having a uniform temperature initially, to cool in a medium which instantly dissipates all heat brought by conduction to its surface, thus keeping the surface at a constant temperature. Suppose we have given the initial excess of the sphere's temperature over that of the medium. Suppose also that the capacity of the mass of the sphere for the diffusion of heat is known, and known to remain invariable during the process of cooling. This capacity is called diffusivity, and is a constant which can be observed. Then from these data the distribution of temperature at any future time can be assigned, and hence also the rate of temperature increase, or the tem-

* *Transactions of the Royal Society of Edinburgh*, 1862. Thomson and Tait's Natural Philosophy, vol. I, Part 2, Appendix D.

perature gradient, from the surface towards the center of the sphere can be computed. It is tolerably certain that the heat conducted from the interior to the surface of the Earth does not set up any re-action which in any sensible degree retards the process of cooling. It escapes so freely that, for practical purposes, we may say it is instantly dissipated. Hence, if we can assume that the Earth had a specified uniform temperature at the initial epoch, and can assume its diffusivity to remain constant, the whole history of cooling is known so soon as we determine the diffusivity and the temperature gradient at any point. Now, Sir William Thomson determined a value for the diffusivity from measurements of the seasonal variations of under-ground temperatures, and numerous observations of the increase of temperature with depth below the earth's surface gave an average value for the temperature gradient. From these elements, and from an assumed initial temperature of 7000° Fahr., he infers that geologic time is limited to something between twenty million and four hundred million years. He says: "We must allow very wide limits in such an estimate as I have attempted to make; but I think we may with much probability say that the consolidation can not have taken place less than 20 million years ago, or we should have more underground heat than we actually have, nor more than 400 million years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that Leibnitz's epoch of emergence of the *consistentior status* was probably between those dates." These conclusions were announced twenty-seven years ago and were re-published without modification in 1883. Recently, also, Professor Tait, reasoning from the same basis, has insisted with equal confidence on cutting down the upper limit of geologic time to some such figures as ten million or fifteen million years.* As mathematicians and astronomers, we must all confess to a deep interest in these conclusions and the hypothesis from which they flow. They are very important if true. But what are the probabilities? Having been at some pains to look into this matter, I feel bound to state that, although the hypothesis appears to be the best which can be formulated at present, the odds are against its correctness. Its weak links are the unverified assumptions of an initial uniform temperature and a constant diffusivity. Very likely these are approximations, but of what order we can not decide. Futhermore, if we accept the hypothesis, the odds appear to be against the present attainment of trustworthy numerical results, since the data for calculation, obtained mostly from observations on continental areas, are far too meagre to give satisfactory average values for the entire mass of the earth. In short, this phase of the case seems to stand about where it did twenty years ago, when Huxley warned us that the perfection of our mathematical mill is no guaranty of the quality of the grist, adding that, "as the grandest mill will not

* Recent Advances in Physical Science, London, 1876.

extract wheat flour from peascods, so pages of formulæ will not get a definite result out of loose data."*

When we pass from the restricted domain of quantitative results concerning geologic time to the freer domain of qualitative results of a general character, the contractional theory of the earth may be said still to lead all others, though it seems destined to require more or less modification if not to be relegated to a place of secondary importance. Old, however, as is the notion that the great surface irregularities of the earth are but the outward evidence of a crumpling crust, it is only recently that this notion has been subjected to mathematical analysis on anything like a rational basis. About three years ago Mr. T. Mellard Reade† announced the doctrine that the earth's crust from the joint effect of its heat and gravitation should behave in a way somewhat analogous to a bent beam, and should possess at a certain depth a "level of no strain" corresponding to the neutral surface in a beam. Above the level of no strain, according to this doctrine, the strata will be subjected to compression and will undergo crumpling, while below that level the tendency of the strata to crack and part is overcome by pressure which produces what Reade calls "compressive extension," thus keeping the nucleus compact and continuous. A little later the same idea was worked out independently by Mr. Charles Davison,‡ and it has since received elaborate mathematical treatment at the hands of Darwin,§ Fisher,‖ and others. The doctrine requires for its application a competent theory of cooling, and hence can not be depended on at present to give anything better than a general idea of the mechanics of crumpling and a rough estimate of the magnitudes of the resulting effects. Using Thomson's hypothesis, it appears that the stratum of no strain moves downward from the surface of the earth at a nearly constant rate during the earlier stages of cooling, but more slowly during later stages; its depth is independent of the initial temperature of the earth; and if we adopt Thomson's value of the diffusivity, it will be about two and a third miles below the surface in a hundeed million years from the beginning of cooling, and a little more than fourteen miles below the surface in seven hundred million years. The most important inference from this theory is that the geological effects of secular cooling will be confined for a very long time to a comparatively thin crust. Thus, if the earth is a hundred million years old, crumpling should not extend much deeper than two miles. A test to which the theory has been sub-

* Geological Reform (The Anniversary Address to the Geological Society for 1869).

† Reade, T. Mellard, Origin of Mountain Ranges, London, 1886.

‡ On the Distribution of Strain in the Earth's Crust resulting from Secular Cooling with special reference to the growth of continents and the formation of mountain chains. By Charles Davison, with a note by G. H. Darwin. *Philosophical Transactions*, vol. 178 (1887), A, pp. 231–249.

§ *Ibid.*

‖ Fisher, Rev. Osmond, Physics of the Earth's Crust, second edition, London, 1889, Chapter VIII.

jected, and one which some* consider crucial against it, is the volumetric amount of crumpling shown by the Earth at the present time. This is a difficult quantity to estimate, but it appears to be much greater than the theory can account for.

The opponents of the contractional theory of the Earth, believing it quantitatively insufficient, have recently revived and elaborated an idea first suggested by Babbage† and Herschel in explanation of the greater folds and movements of the crust. This idea figures the crust as being in a state bordering on hydrostatic equilibrium, which can not be greatly disturbed without a re-adjustment and consequent movement of the masses involved. According to this view the transfer of any considerable load from one area to another is followed sooner or later by a depression over the loaded area and a corresponding elevation over the unloaded one, and in a general way it is inferred that the elevation of continental areas tends to keep pace with erosion. The process by which this balance is maintained has been called isostasy,‡ and the crust is said to be in an isostatic state. The dynamics of the superficial strata with the attendant phenomena of folding and faulting are thus referred to gravitation alone, or to gravitation and whatever opposing force the rigidity of the strata may offer. In a mathematical sense, however, the theory of isostasy is in a less satisfactory state than the theory of contraction. As yet we can see only that isostasy is an efficient cause if once set in action, but how it is started and to what extent it is adequate remain to be determined. Moreover, isostasy does not seem to meet the requirements of geological continuity, for it tends rapidly towards stable equilibrium, and the crust ought therefore to reach a state of repose early in geologic time. But there is no evidence that such a state has been attained, and but little if any evidence of diminished activity in crustal movements during recent geologic time. Hence we infer that isostasy is competent only on the supposition that it is kept in action by some other cause tending constantly to disturb the equilibrium which would otherwise result. Such a cause is found in secular contraction, and it is not improbable that these two seemingly divergent theories are really supplementary.

Closely related to the questions of secular contraction and the mechanics of crust movements are those vexed questions of earthquakes, volcanism, the liquidity or solidity of the interior, and the rigidity of the earth's mass as a whole;—all questions of the greatest interest, but still lingering on the battle-fields of scientific opinion. Many of the "thrice slain" combatants in these contests would fain risk being slain again; and whether our foundation be liquid or solid, or, to speak more

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* Notably, Rev. Osmond Fisher. See his Physics of the Earth's Crust, chapter viii.

† Appendix to the Ninth Bridgewater Treatise (by C. Babbage), second edition, London, 1838.

‡ Dutton, Capt. C. E. On some of the Greater Problems of Physical Geology, *Bulletin Philosophical Society of Washington*, vol. XI, pp. 51–64.

precisely, whether the Earth may not be at once highly plastic under the action of long-continued forces and highly rigid under the action of periodic forces of short period, it is pretty certain that some years must elapse before the arguments will be convincing to all concerned. The difficulties appear to be due principally to our profound ignorance of the properties of matter subject to the joint action of great pressure and great heat. The conditions which exist a few miles beneath the surface of the earth are quite beyond the reach of laboratory tests as hitherto developed, but it is not clear how our knowledge is to be improved without resort to experiments of a scale in some degree comparable with the facts to be explained. In the mean time, therefore, we may expect to go on theorizing, adding to the long list of dead theories which mark the progress of scientific thought with the hope of attaining the truth not so much by direct discovery as by the laborious process of eliminating error.

When we take a more comprehensive view of the problems presented by the Earth, and look for light on their solution in theories of cosmogony, the difficulties which beset us are no less numerous and formidable than those encountered along special lines of attack. Much progress has recently been made, however, in the elaboration of such theories. Roche,* Darwin,† and others have done much to remove the nebulosity of Laplace's nebular hypothesis. Poincaré ‡ and Darwin § have gone far towards bridging the gaps which have long rendered the theory of rotating fluid masses incomplete. Poincaré has, in fact, shown us how a homogeneous rotating mass might, through loss of heat and consequent contraction, pass from the spheroidal form to the Jacobian ellipsoidal form, and thence, by reason of its increasing speed of rotation, separate into two unequal masses. Darwin, starting with a swarm of meteorites and gravitation as a basis, has reached many interesting and instructive results in the endeavor to trace out the laws of evolution of a planetary system.|| But notwithstanding the splendid researches of these and other investigators in this field, it must be said that the real case of the solar system, or of the earth and moon, still defies analysis; and that the mechanics of the segregation of a planet from the sum, or of a satellite from a planet, if such an event has ever happened, or the

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* Essai sur la Constitution et l'origine du système solaire, par M. Édouard Roche. *Mémoires de l'Académie des Sciences et Lettres de Montpellier*, Tome VIII, 1873.

† On the Precession of a Viscous Spheroid and on the remote History of the Earth, *Phil. Trans.*, Part II, 1879. On the secular changes in the Elements of the Orbit of a Satellite revolving about a tidally distorted Planet, *Phil. Trans.*, Part II, 1880. On the Tidal Friction of a Planet attended by several Satellites, and on the Evolution of the Solar System, *Phil. Trans.*, Part II, 1881.

‡ Sur l'équilibre d'une masse fluide animée d'un mouvement de rotation. *Acta Mathematica*, vol. 7, 1885.

§ On figures of Equilibrium of Rotating Masses of Fluid, *Phil. Trans.*, vol. 178, 1887.

|| On the Mechanical Conditions of a Swarm of Meteorites and on Theories of Cosmogony, *Phil. Trans.*, vol. 180, 1889.

mechanics of the evolution of a solar system from a swarm of meteorites, are still far from being clearly made out.

Time does not permit me to make anything but the briefest allusion to the comparatively new science of mathematical meteorology with its already considerable list of well-defined theories pressing for acceptance or rejection. Nor need I say more with reference to those older mathematical questions of the tides and terrestial magnetism than that they are still unsettled. These and many other questions, old and new, might serve equally well to illustrate the principal fact that this address has been designed to emphasize, namely, that the mathematical theories of the earth already advanced and elaborated are by no means complete, and that no mathematical Alexander need yet pine for other worlds to conquer.

Speculations concerning the course and progress of science are usually untrustworthy if not altogether fallacious. But, being delegated for the hour to speak to and for mathematicians and astronomers, it may be permissible to offer, in closing, a single suggestion, which will perhaps help us to orient ourselves aright in our various fields of research. If the curve of scientific progress in any domain of thought could be drawn, there is every reason to believe that it would exhibit considerable irregularities. There would be marked maxima and minima in its general tendency towards the limit of perfect knowledge; and it seems not improbable that the curve would show throughout some portions of its length a more or less definitely periodic succession of maxima and minima. Races and communities as well as individuals, the armies in pursuit of truth as well as those in pursuit of plunder, have their periods of culminating activity and their periods of placid repose. It is a curious fact that the history of the mathematical theories of the earth presents some such periodicity. We have the marked maximum of the epoch of Newton near the end of the seventeenth century, with the equally marked maximum of the epoch of Laplace near the end of the eighteenth century; and, judging from the recent revival of geodesy and astronomy in Europe, and from the well-nigh general activity in mathematical and geological research, we may hope, if not expect, that the end of the present century will signalize a similar epoch of productive activity. The minima periods which followed the epochs of Newton and Laplace are less definitely marked but not less noteworthy and instructive. They were not periods of placid repose; to find such one must go back into the night of the middle ages; but they were periods of greatly diminished energy, periods during which those who kept alive the spirit of investigation were almost as conspicuous for their isolation as for their distinguished abilities. Many causes, of course, contributed to produce these minima periods, and it would be an interesting study in philosophic history to trace out the tendency and effect of each cause. It is desired here, however, to call attention to only one cause which contributed to the somewhat general apathy of the periods mentioned,

and which always threatens to dampen the ardor of research immediately after the attainment of any marked success or advance. I refer to the impression of contentment with and acquiescence in the results of science, which seems to find easy access to trained as well as untrained minds before an investigation is half completed or even fairly begun. That some such tacit persuasion of the completeness of the knowledge of the earth has at times pervaded scientific thought, there can be no doubt. This was notably the case during the period which followed the remarkable epoch of Laplace. The profound impression of the sufficiency of the brilliant discoveries and advances of that epoch is aptly described by Carlyle in the half humorous, half sarcastic language of Sartor Resartus. "Our theory of gravitation," he says, "is as good as perfect: Lagrange, it is well known, has proved that the planetary system, on this scheme, will endure forever; Laplace, still more cunningly, even guesses that it could not have been made on any other scheme. Whereby, at least, our nautical logbooks can be better kept; and water transport of all kinds has grown more commodious. Of geology and geognosy we know enough; what with the labors of our Werners and Huttons, what with the ardent genius of their disciples, it has come about that now, to many a royal society, the creation of a world is little more mysterious than the cooking of a dumpling; concerning which last, indeed, there have been minds to whom the question—*How the apples were got in*—presented difficulties." This was written nearly sixty years ago, about the time the sage of Ecclefechan abandoned his mathematics and astronomy for literature to become the seer of Chelsea; but the force of its irony is still applicable, for we have yet to learn, essentially, "*How the apples were got in*" and what kind they are.

As to the future, we can only guess, less or more vaguely, from our experience in the past and from our knowledge of present needs. Though the dawn of that future is certainly not heralded by rosy tints of overconfidence amongst those acquainted with the difficulties to be overcome, the prospect, on the whole, has never been more promising. The converging lights of many lines of investigation are now brought to bear on the problems presented by our planet. There is ample reason to suppose that our day will witness a fair average of those happy accidents in science which lead to the discovery of new principles and new methods. We have much to expect from the elaborate machinery and perfected methods of the older and more exact sciences of measuring and weighing—astronomy, geodesy, physics, and chemistry. We have more to expect, perhaps, from geology and meteorology, with their vast accumulation of facts not yet fully correlated. Much, also, may be anticipated from that new astronomy which looks for the secrets of the earth's origin and history in nebulous masses or in swarms of meteorites. We have the encouraging stimulus of a very general and rapidly growing popular concern in the objects of our inquiries,

and the freest avenues for the dissemination of new information; so that we may easily gain the advantage of a concentration of energy without centralization of personal interests. To those, therefore, who can bring the pre-requisites of endless patience and unflagging industry, who can bear alike the remorseless discipline of repeated failure and the prosperity of partial success, the field is as wide and as inviting as it ever was to a Newton or a Laplace.

ON THE PHYSICAL STRUCTURE OF THE EARTH.*

By HENRY HENNESSY, F. R. S.

The structure of the Earth, as a mechanical and physical question, is closely connected with the origin and formation of its satellite, and of the planets and satellites belonging to the same solar system. The brilliant results obtained during the present and preceding century by the aid of mathematical analysis, whereby the motions of those bodies have been brought within the grasp of dynamical laws may have led to the notion that by similar methods many obscure problems relating to the planet we inhabit might be accurately solved. But although the general configuration of the Earth and planets has been treated mathematically, with results which leave little to be desired, when applications of analytical methods are attempted to questions of detail in terrestrial structure, the complication of the conditions is so great as to impose the necessity on some investigators of so altering these conditions as to make their results perfectly inapplicable to the real state of the Earth. Physical geology presents problems the solution of which undoubtedly calls for mechanical and physical considerations; but these may in general, under the complex nature of the phenomena, be often better reasoned out without the employment of the symbolical methods of analysis. In most cases the conditions are totally unlike those above alluded to, which admit of precise numerical computations. The heterogeneous character of the rocks composing the Earth's crust, and the probably varied nature of the matter composing its interior, render mathematical applications rarely possible, and sometimes misleading Such views seem to be gradually gaining strength among geologists who pay attention to questions of a general nature, and no one has beeter expressed them in recent times than Prof. M. E. Wadsworth.†

The principle upon which I have ventured to found all my researches on terrestrial physics is this: to reason on the matter composing the globe from our knowledge of the physical and mechanical properties

* From the *L. E. D. Philosophical Magazine*, September and October, 1886, vol. XXII, pp. 233–251 and 328–331.

† "Lithological Studies." *Memoirs of Harvard College Museum*, vol. I, p. 3, and *American Naturalist*, June, 1884, p. 587.

of its materials which come under our notice. Of these properties the most important are density, compressibility, and contraction or dilation from changes of temperature. Newton and other philosophers have already adopted the same principle to a limited extent, when assuming for the mass of fluid composing the Earth in its primitive condition those specific properties which have been assigned to all kinds of fluids observed at the surface. It is impossible to frame any statement more erroneous and misleading than that I have endeavored to render the question more hypothetical than it was. On the contrary, I have discarded the invariable assumption of mathematicians who treated the question, namely, the hypothesis of the invariability of positions of the particles composing the solidifying earth. The speculations of all rational inquirers upon the Earth's internal structure must necessarily start from the same general principle as above. Some investigators have disregarded that principle and made the problem thereby a purely mathematical exercise.

In order to reason upon the Earth's figure, we must assume that the laws of fluid equilibrium apply to the inner portions of the fluid as well as the outer. There is nothing hypothetical in reasonings as to the formation of the solid shell and the law of increase of ellipticity of its inner surface as a result of the transition of the formerly fluid matter to the state of solidity. On the contrary, the assumptions of Mr. Hopkins and other mathematicians, that this transition created no change in the law of density of the matter composing the Earth and in the ellipticity of the strata of equal pressure, are not merely hypothetical; they are directly opposed to well-established physical and mechanical laws.

On the other hand, those who have concluded that nothing can be known of the form of the fluid nucleus seem to deny that the recognized laws of matter apply to the internal condition of the Earth. The shape of the nucleus and the figures of its strata of equal density follow from physical and mechanical laws, just as the forms of the isothermal surfaces within the spheroid follow from the known laws of conduction of heat. Some of the mechanical reasonings regarding the strata of the nucleus and the structure of the solid shell can be presented without employing mathematical symbols, and in what follows I have, as far as possible, avoided the use of such symbols.

This course, moreover, possesses the advantage of making many parts of reasonings more clear to geologists and observers of the stratigraphical features of the Earth, who are in reality the ultimate judges of the matter, and not mathematicians. The necessity under which the latter are constrained when dealing with problems, of throwing the preliminary propositions into simple, well-defined shapes, admitting of definite deductions, obliges them to overlook the most essential conditions of the very questions at issue, and they thus arrive at results which may be precise, but which are totally inconclusive with reference to the Earth's structure.

THE MECHANICAL AND PHYSICAL PROPERTIES OF THE MATTER COMPOSING THE EARTH.

(1) The materials of the Earth must manifestly influence its general structure, and no inquiries with this structure can be usefully made if the physical properties of these materials are not kept in view. If the interior of the Earth is in a fluid state it is reasonable to believe that the fluid is not the ideal substance called by mathematicians a perfect liquid, namely, a substance not only endowed with perfect mobility among its particles, but also absolutely incompressible. It is more reasonable to believe that the fluid in question resembles the liquid outpourings of volcanoes, or at least some real and tangible liquid whose properties have been experimentally studied. I have already shown that by overlooking this simple principle certain untenable conclusions, which assert the exclusively solid character of the Earth, have been deduced. Here I propose to develop some additional arguments relative to one of the properties of liquids which has an essential bearing upon the internal structure of the Earth.

(2) In a former paper, on the limits of hypotheses regarding the properties of matter composing the Earth's interior,* I find that having referred to published statements where the facts were not clearly put forward, I underrated the compressibility of liquids as compared with solids. The influence of the imperfect experiments of the Academia del Cimento has long injuriously operated in defining liquid and solid matter, and has produced a remarkable conflict of opinions.

On taking the results of the best experimental investigations it appears that, although liquids are but slightly compressible as compared with gases, they are highly compressible as compared with solids. In many treatises on physics and mechanics which have a high reputation, matter is divided into solids, elastic fluids or gases, and incompressible fluids or liquids. Hence the erroneous inference seems to have arisen that liquids are incompressible, not only in comparison with gases, but also in comparison with solid bodies. I was surprised to find this remarkably misleading proposition formally stated, long after the decisive experiments of Oersted, Colladon, and Sturm, Regnault, Wertheim, and Grassi, in such a work as Pouillet's *Eléments de Physique*, and also in the German translation by Müller. The great compressibility of liquids as compared with solids is seldom affirmed as a distinct general proposition in books on physics. It occurs, however, in Deschanel's treatise, both in the original and in the English edition. Daguin states, in vol. I of his *Traité de Physique*, 2d edition, p. 40, that the compressibility of liquids was long considered doubtful, but nevertheless they are more compressible than solids.

Lamé also pointed out the great compressibility of liquids as com-

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* *Philosophical Magazine* for October, 1878, p. 265.

pared with solids. I have before now referred to the statement of the same proposition in the comprehensive work of the late Prof. C. F. Naumann, the *Lehrbuch der Geognosie*, vol. I, p. 269, 2d edition.*

Although in many physical questions the compressibility of liquids may be neglected as well as the compressibility of solids, we are not entitled to assume at any time that the latter are relatively more compressible than the former. In questions where the pressure of columns of liquid of great magnitude comes under consideration we can no longer treat the liquid as incompressible. In the problem of oceanic tides the incompressibility of the water has been assumed, but if a planet were covered with water to a depth of 100 miles it would be scarcely correct to make such an assumption. The compressibility is negligible in a small mass of water, but it can not be neglected in a large mass. Such an assumption is equally unwarrantable with regard to properties of matter which, though negligible in some problems, are not in others. Thus in the common hydraulic questions liquids are assumed to be incompressible; it would be more correct to say the compressibility is neglected. In small problems connected with limited portions of the atmosphere the compressibility of air may be also neglected, but we could not neglect it for a high column of the atmosphere. If, as before remarked, the Earth were surrounded with an ocean 100 miles deep, the compressibility of the water could not be well overlooked in tidal questions; then, *à fortiori*, compressibility can not be neglected in such a problem as the tides of a liquid spheroid having a radius nearly equal to that of the earth. This is immediately made manifest by expressing the compressibilities of liquids, not in terms of the amount due to a single atmosphere of pressure, as is done in most tabulated groups of results, but by some very much greater standard, such as one or two thousand atmospheres. In the experiments of Perkins† the highest pressure employed was 2,000 atmospheres, and with this he reduced a column of water by nearly one-twelfth of its volume. The results of experiments with great pressures such as this are highly illustrative of the force by which a fluid may be compressed in the Earth's interior. The actual coefficients of cubical compressibility, on which calculations could be based, may be partly obtained from the more exact researches of Regnault, Grassi, and other recent experiments, or from special investigations on fluid matter conducted with precautions such as these observers have employed. By then comparing the moduli of compressibilities calculated from pressures of 1,000 or 10,000 atmospheres there could be no possibility of overlooking the consequences as to the relations of liquids and solid bodies in any case where they could be subjected to pressures of abnormal magnitude.

(3) The propagation of sound in liquids and solids gives further proof of the greater compressibility of liquids.

* "Flüssige Körper sind aber mit einer weit starkeren Compressibilitat begabt, also starre Körper."

† *Phil. Trans.* 1826, p. 541.

The rate v of transmission of sound in solids and liquids is a function of their compressibilities. In solids,

$$v=\sqrt{\frac{gE}{\rho}},$$

where E is the modulus of elasticity and ρ the density. In liquids,

$$v_1=\sqrt{\frac{gHa}{\mu\,\rho_1}},$$

where μ is the coefficient of cubic compressibility, H the pressure of the atmosphere, and a the density of mercury. But as in solids the modulus of elasticity is inversely as the compressibility k, we have

$$\frac{v}{v_1}=\sqrt{\frac{\mu\,\rho_1}{k\rho Ha}}$$

Both in solids and liquids the velocity of sound is inversely as the square roots of the densities and compressibilities. Although such solids as metals and rocks are denser than most liquids, the limits of their elastic compressibility are so much less that sound is propagated far more quickly through such solids than through liquids. In steel and metals generally this has been long since established. In rocks the velocity of sound has been computed from direct experiment by Mallet, and has been found to be greater in continuous homogeneous rock than the velocities observed in liquids.*

(4) If we had not the results of direct experiment on the compressibilities of liquids and solids to assure us that these properties in liquids are in excess of those obtained for solids we might fairly infer this conclusion from the relative dilatability of such substances under differences of temperature.† The construction of our common thermometers is based on the greatly superior dilatability of the liquids inclosed in the thermometer-tube over the material of the tube itself. The dynamical theory of heat clearly establishes that the expansion of solids and liquids is a mechanical action as much as their compression under the action of force, and the substances which contract least by

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* See *Philosophical Transactions* for 1861 and 1862.

† *Expansions of metals and glass for* 1° *C., according to Dulong and Petit, at different temperatures T.*

SOLIDS.							T.	LIQUID.
Platinum.	T.	Iron.	T.	Copper.	T.	Glass.		Mercury.
	°		°		°		°	
$\frac{1}{37,700}$	100	$\frac{1}{28,200}$	100	$\frac{1}{19,400}$	100	$\frac{1}{38,700}$	100	$\frac{1}{5,550}$
						$\frac{1}{36,800}$	213	$\frac{1}{5,425}$
$\frac{1}{38,300}$	311	$\frac{1}{22,700}$	372	$\frac{1}{17,700}$	328	$\frac{1}{32,000}$	353	$\frac{1}{5,300}$

cooling are precisely those which contract least under pressure. Gases which contract most by pressure are also the most dilatable by heat. Liquids occupy an intermediate place between solids and gases in relation both to the dynamical effect of pressure and the action of loss of heat. If, instead of the experiment of the Academia del Cimento with globes of porous metal, an experiment with equally strong but impervious vessels had been made, the deformation of each globe would have been unaccompanied by the exudation of the liquid, and the totally false statement that solids are more compressible than liquids would not have so long injuriously influenced physical science.

THE ROTATION OF THE EARTH CONSIDERED AS PARTLY FLUID AND PARTLY SOLID.

(1) The problem of the precessional motion of the Earth considered as a solid shell filled with liquid devoid of viscidity and friction has been elaborately investigated by Mr. Hopkins, in his "Researches on Physical Geology," in the *Philosophical Transactions* for 1839, 1840, and 1842, and the result obtained by him has been often quoted as extremely remarkable. Before treating the same question, it may be necessary to state that on the continent of Europe the application made by Mr. Hopkins of his result to geology is not generally admitted, and views such as I have always firmly upheld seem to be more generally adopted; but some confusion appears to exist as to Mr. Hopkins's results and those to which I have been led. Thus in a recent treatise on systematic geology the author says, with reference to the thickness of the solid crust of the earth, there are plainly only four possibilities to be thought of:

1. The Earth is through and through solid.
2. The Earth is through and through fluid, with a solid crust.
3. The Earth has a solid nucleus and a solid crust, with fluid stratum lying between.
4. The Earth is solid, but furnished with cavities which are filled with fluid.

The first and last of these possibilities are not admissible, according to astronomical observations. According to the investigators of Hopkins the action exercised by the sun and moon on the position of the Earth's axis in space, by which precession and nutation are produced, would be different according to the structure we attribute to the earth. *The values established by observation compel us to regard the earth as for the most part in a fluid state, in order that the results may harmonize with calculation* (Pfaff, Grundriss der Geologie). This is the reverse of what Hopkins has concluded, and is precisely what I have long since enunciated, which I have always continued to maintain, and which forms the cumulative result of the investigations in the text of this paper. In a report to the Royal Irish Academy on "Experiments on the Influence of the Molecular Influence of Fluids on their Motion

when in Rotation," p. 57,* I referred to a proof obtained by me of the result alluded to, and I now may be allowed to submit this proof to those interested in the question.

(2) Let us suppose the earth to consist of a solid spheroidal shell composed of nearly similar spheroidal strata of equal density, and having the ellipticities of the inner and outer surfaces small and nearly equal. The shell is supposed to be full of liquid and to rotate around its polar axis. Under these conditions the attraction of an exterior body would tend to produce pressure between the fluid nucleus and the inner surface of the shell. Whatever may be the direction of this pressure, it can be resolved into a force normal to the shell's surface and into forces in its tangent plane. The normal force might be effective in causing a deformation of the shell, or, if the latter were rigid, it would be destroyed by the shell's resistance.

If friction existed between the materials of the shell and the fluid of the nucleus, the resolved forces in the tangent plane would tend to change the motion of the shell from the motion it would have if empty. But if no friction and no adhesion existed between the particles of the liquid and the shell's nearly spherical surface, and if the particles of the liquid are free from viscidity and internal friction among themselves. this purely tangential component could exercise no influence on the motions of the shell. If the solid envelope containing fluid was bounded by planes such as a prismatic vessel or box, it is manifest that unequal normal pressures on the faces of such prism would tend to produce couples, and thus possible rotations. Such a case has been considered by Professor Stokes, and he has shown that a rectangular prism filled with fluid will have the same motion as if the fluid was replaced by a solid having the same mass, center of gravity, and principal axis, but with much smaller moments of inertia corresponding to these axes. But in a continuously curved and nearly spherical vessel the normal pressure arising from the disturbance of the liquid could not produce the same results. The tangential components of the forces acting at the surface of the liquid could, in this case, be alone effective, and if no friction or viscidity existed at this surface such tangential action would totally disappear. The conclusion of Mr. Hopkins's first memoir is, that if the ellipticity of the inner and outer surfaces of the solid shell were the same, precession would be unaffected by the fluid, and any small inequality of nutation would be totally inappreciable to observation (p. 423, *Phil. Trans.*, 1839). This may be rendered more manifest by recalling the general equations for the surface of a fluid obtained by Poisson, Navier, Meyer, and other mathematicians when the internal friction of the fluid is taken into account. If α, β, γ, be the angles made by the normal to the curved surface of the fluid, X, Y, Z the components parallel to the rectangular axes of x, y, and z, it appears that we shall have at the fluid surface, when nearly spherical,

* *Proceedings of R. I. A.*, 2d series, vol. III, Science.

$$X = hk^2\left[\ 2\frac{du}{dx}\cos\alpha + \left(\frac{du}{dy} + \frac{dv}{dx}\right)\cos\beta + \left(\frac{du}{dz} + \frac{dv}{dx}\right)\cos\ \gamma\ \right],$$

$$Y = hk^2\left[\left(\frac{dv}{dx} + \frac{du}{dy}\right)\cos\ \alpha + 2\frac{dv}{dy}\cos\ \beta + \left(\frac{dv}{dz} + \frac{dw}{dy}\right)\cos\gamma\ \right],$$

$$Z = hk^2\left[\left(\frac{dw}{dx} + \frac{du}{dz}\right)\cos\alpha + \left(\frac{dw}{dy} + \frac{dv}{dz}\right)\cos\ \beta + 2\frac{dw}{dz}\cos\ \gamma\ \right],$$

where u, v, w are components of velocity parallel to the coördinate axes, and where k is a coefficient depending on friction and viscidity. If no viscidity and no friction exists we must have $k=0$, and hence also

$$X=0,\ Y=0,\ Z=0.$$

Now, as X, Y, and Z are the effective components with which the nearly spherical mass of fluid acts at its surface when each of them is separately equal to zero, it follows that the fluid can do no work at the surface, and the motions of the shell would take place quite independently of the contained mass of fluid when the latter is totally devoid of friction and viscidity.

(3) It has long since been clearly shown that the motion of the axis of the Earth, considered as a solid body, may be determined by the differential equations

$$\frac{d\psi}{dt} = -\frac{1}{Cn\ \sin\ \theta}\frac{dV}{d\theta}$$

$$\frac{d\theta}{dt} = \frac{1}{Cn\ \sin\ \theta}\frac{dV}{d\psi}.$$

V is the potential of the rotating solid, C its maximum moment of inertia, θ and ψ direction angles of the axis of rotation. In the case of the Earth, θ has a particular value when it becomes the obliquity of the ecliptic, and ψ the longitude of the first point of Aries. It follows that the determination of ψ and θ at any time depends upon C and V.

By analytical transformations, which are fully given by Poisson in his memoir *Sur la Rotation de la Terre autour de son centre de Gravité*, and by other writers, it finally appears that the variations of θ and ψ depend on equations in which a factor enters of the form

$$\frac{2\ C - A - B}{C},$$

where A, B, C, are the three principal moments of inertia of the Earth. In a spheroid of revolution $A = B$, and the factor becomes $\frac{2\ (C - A)}{C}$.

As precession depends essentially on the variation of the angle ψ, it follows that the complete expression of the factor $\frac{C-A}{C}$ is of primary importance.

(4) Mathematicians, during the past two centuries, have devoted much attention to the question of the figure of a rotating mass of fluid, with especial reference to the explanation of the spheroidal figures of the earth and her sister planets. Solutions of this problem have been presented, especially by Clairaut, Legendre, Laplace, Gauss, Ivory, Jacobi, and Airy; and it is not a little remarkable that in applying these solutions to the case of the Earth every one of these investigators has not only supposed the Earth to have been originally in a fluid state, but that the particles of the mass retained the same positions after solidification had taken place. This tacit or openly expressed assumption of the unchangeable position of the particles of the original fluid mass on their passage to a complete or partial state of solidity lies at the root of the whole question of the Earth's structure. For the first time in the treatment of the physico-mathematical problem, I distinctly discarded this assumption and I affirmed that the position of the particles of matter, on passing from the state of fluidity to solidity, must assume positions in conformity with mechanical and physical laws. In this way the hypothesis of the Earth's primitive fluidity became more simple and much more rational; for it was as manifestly absurd to assume that the particles of the fluid mass, on passing into a solid state of consistence, retained their original positions, as it would be to assume that if the whole Earth became liquefied the positions of its particles would be unchanged. The corrected and simplified hypothesis is also fruitful in important results; but it is singular that, as far as I am aware, no mathemetician seems to have understood or appreciated its bearing on the physical structure of the Earth, except M. Plana, by a remark in a memoir published by him towards the close of his career.

(5) Before presenting my conclusions on the shape of the inner surface of the solidified shell and Plana's remark relative to the same subject, it is necessary to recall some results established by Clairaut and frequently put forward by mathematical investigators of the Earth's figure. It seems to be universally admitted that if a mass of heterogeneous fluid composed of strata of equal density, each increasing in density from the surface of the mass to its center, is set in rotation, the several strata will be spheroidal, but their ellipticities will not be equal. The ellipticities will decrease from the outer surface toward the center. This law of decrease of ellipticity toward the center is not a hypothetical result, but a necessary deduction from the properties of fluids. As all known fluids are compressible, such an arrangement of strata of equal density as that referred to must follow from the supposition of the existence of any mass of fluid of such magnitude as the whole Earth. The increase

of the Earth's density from its surface to its center is, moreover, a fact clearly revealed by the mean density of the Earth being double that of the materials composing the outside of its solid shell.

If the increase of density in going from the surface to the center of a large mass of fluid is due to compression exercised by the outer upon the inner strata, it follows that the greater the total quantity of fluid the greater will be the difference between the density at its surface and its center, and the less the quantity of fluid the less will be this difference. With a small spheroid of compressible fluid the variation of density might be neglected and the mass regarded as homogeneous. Suppose such a small mass of fluid to be set in rotation, its surface will become spheroidal, and it will have the well-known ellipticity $\frac{5}{4}m$, where m is the ratio of centrifugal force to gravity at the equator of the spheroid. If now this original spheroid be supposed to be overlaid with masses of the fluid, one after another, the inner portions will be sensibly compressed, and the whole mass will begin to vary in density in going from center to surface. The outer surface will now present an ellipticity less that $\frac{5}{4}m$. If fresh layers of fluid are continually applied to the outer surface, the variation of density will continue, and the difference between the density at the center and surface will increase. The ellipticity of the outer stratum of fluid will at the same time diminish to a value corresponding to the law of density. Let us now reverse this operation and suppose a great mass of liquid in rotation; its outer stratum will be less dense than those beneath, and its greatest density must be at the center. Let the outer strata of equal density be successively removed, so as to leave a succession of free fluid surfaces, until a spheroid is reached in which the difference of density is insensible. It is manifest that with each successive removal of the upper stratum of liquid the compression in the remaining strata becomes reduced, and also the variation in density from surface to center, until this variation becomes altogether extinguished. With the same velocity of rotation, the ellipticities of the surfaces of liquid thus successively exposed would increase up to the limiting value, $\frac{5}{4}m$.

If at any time of the Earth's solidification we suppose a nucleus of fluid to be inclosed within the solid shell, the successive increasing of thickness of the shell, from the congelation of the fluid matter of the nucleus, must be accompanied by the removal of successive outer strata from the nucleus. From what has been seen already, the nucleus will tend to acquire an increase of ellipticity, and therefore to mould the semifluid pasty matter about to pass into a solid state into a shape different from what it would have if no change whatever in the position of the particles had taken place. As the nucleus is supposed to be in a state of fusion from heat, the successive additions to the inner surface of the shell from the matter of the nucleus must proceed at a very slow rate. The congelation of the surface stratum of the nucleus must be a process of the same order of slowness as the flow of heat through the shell; and the mathematical theory of conduction established by Fourier

shows that this can not proceed otherwise than slowly. The changes in shape of the surface of the nucleus would be correspondingly slow and gradual. When once a comparatively rigid outer crust had been formed, the process of molding additional strata of solidified matter against the inner surface of the crust from the nucleus would proceed in a slow and gradual order, so that the resulting solid strata would conform to the shape impressed upon them by the molding forces. A remarkable illustration of the way in which fused matter ejected from the Earth's interior may, while turning on its center and at the same time cooling, mold itself against a solid crust formed upon it has been adduced by Charles Darwin, and has already been quoted by me on a former occasion. From these considerations I have been led to conclude, *that the ellipticity of the shell's inner surface may exceed but can not be less than the ellipticity of its outer surface;** and referring to the same question, Plana used the words, "La loi des ellipticités a subi dans le passage de l'état liquide à l'état solide une alteration sensible par laquelle toutes les couches se sont constitueés de manière a avoir *un même* applatissement et plus grand que le précédent." M. Plana has further stated his views in the same volume of the *Astronomische Nachrichten* for 1852, thus: "Il est permis de penser que ces couches (de la fluide intérieuse) en se consolidant, ont subi des modifications à la verité fort petites, mais assez grandes pour nous empêcher de pouvoir dériver, avec tout l'exactitude que l'on pourrait souhaiter, l'état de la Terre solid de son état antérieure de fluidité."

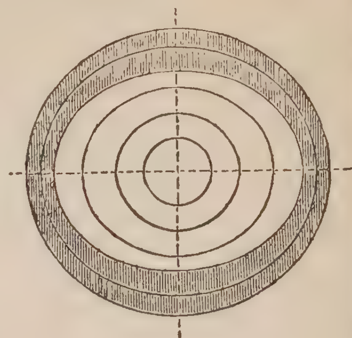
This paragraph gives a distinct adhesion to the improved form of the hypothesis of the original fluidity of the Earth; and this concurrence on the part of M. Plana is the more important, as it is possible that he had formed his conclusions independently. He refers to a letter written by him on the subject to Humboldt; and it is remarkable that, in the fifth and last volume of "Cosmos," published not long before the author's death, some adjacent notes allude to Plana's views, and contain references to the investigations of Mr. Hopkins and to my early researches.

At this period Humboldt could scarcely have had time to examine the mechanical and physical reasonings, and he merely quoted the papers in the *Philosophical Transactions* as if he had seen them for the first time. I am not aware of any evidence as to whether Plana had known their contents; and it is possible that his conclusions as to the forms of the strata of the shell and nucleus had been formed independently, though published a short time after my investigations.

The annexed figure may assist in making clear the results of the preceding paragraph. The outer ellipse represents the outline of the exterior surface of the Earth's crust, which is shaded and bounded inwardly by a surface slightly more elliptical. The fluid nucleus included within the shell is represented with strata decreasing in ellipticity towards the

* See the subjoined representation of a section of the shell and nucleus.

center. This arrangement is necessarily followed by a mass of fluid under such conditions as the nucleus, or under the conditions of the entirely fluid Earth. If the matter composing the Earth underwent no change in passing from the fluid to the solid state, instead of the arrangement here represented, the inner surface of the shell would have a smaller ellipticity than its outer surface, and the strata of the shell, as well as those of the nucleus, would be less oblate in going from the outer surface.



(6) It is important to distinctly bear in mind that the constitution of the shell and nucleus indicated by the foregoing reasonings is not based on any hypothesis of a specific law of density of the interior strata of the Earth. It is a deduction from the established properties of fluids quite as vigorous as the conclusions regarding the spheroidal shape of a mass of rotating liquid. On the other hand, the supposition tacitly or openly made by Mr. Hopkins and his followers, that the ellipticity of the inner stratum of the solid shell is precisely the same as that which this stratum had when fluid, is not merely a hypothesis—it is an assumption which is directly contradicted by the recognized physical properties of all known liquids, and even contradicted by the fundamental principles of hydrodynamics. Upon this assumption was based the calculation of the ratios of the inner and outer ellipticities of the shell which would correspond to the observed value of the precession of the Earth's axis, and hence the limiting value of the thickness of the shell. But when the fundamental assumption on which this ratio is calculated is shown to be in contradiction to physical and mechanical laws, the whole of the conclusions drawn from such a calculation must fall to the ground.

In the *Mecanique Céleste*, Laplace, following Clairaut, proved that if the density in a fluid spheroid decreases from the center to the surface, the ellipticity of the strata of equal density must decrease from the surface towards the center. This result forms the groundwork of some of the arguments employed in the present inquiry. Legendre and Laplace also deduced a law of density from the properties of compressible fluids, and from this law the latter unfolded a law of ellipticity of the strata of equal density. The results arrived at in my present inquiry are manifestly totally independent of the law of density $\rho = \frac{A \sin qa}{a}$, deduced by Legendre and Laplace. In order to apply this law to the strata of the solidified shell, the assumption must necessarily be made that the particles of the fluid underwent no change in position on passing to the solid state. This was assumed

by Mr. Hopkins and Archdeacon Pratt; and, as we have seen, such an assumption is not only unwarranted, but is absolutely contradicted by the established laws of hydrodynamics. My conclusions are not only in harmony with those laws, but necessarily require them to be kept constantly in view throughout the whole investigation.

(7) The result obtained in section (3) allows of an immediate and easy application to the inquiry before us, if we admit that the strata of equal density in the shell have all equal ellipticities—an admission which has been already shown to be a particular case of a rigorous and exact deduction from hydrodynamical principles. In this case let us consider the ratio of the difference of the moments of inertia of any spheroidal stratum to its greatest moments of inertia. It will readily appear that the difference of the greatest and least moments of inertia, of all the strata, divided by the sum of the greatest moments of inertia, will be the same as that for a homogeneous shell whose inner and outer elipticities are equal.

If ρ be the density of any spheroidal stratum of equal density, then for that stratum

$$\frac{C_1-A_1}{C_1}=\frac{\int\rho\,(x^2+y^2)\,dx\,dy\,dz-\int\rho\,(z^2+y^2)\,dx\,dy\,dz}{\int\rho\,(x^2+y^2)\,dx\,dy\,dz},$$

and as ρ may be placed outside the sign of integration, it disappears both from numerator and denominator. As we shall see presently,

$$\frac{C_1-A_1}{C_1}=\frac{1}{2}\left(1-\frac{b_1^2}{a_1^2}\right),$$

where b_1 and a_1 are the semi-axes of the stratum; and for all other strata of equal density we would have

$$\frac{C_2-A_2}{C_2}=\frac{1}{2}\left(1-\frac{b_2^2}{a_2^2}\right),$$

$$\frac{C_3-A_3}{C_3}=\frac{1}{2}\left(1-\frac{b_3^2}{a_3^2}\right),\ \ldots\ \frac{C_n-A_n}{C_n}=\frac{1}{2}\left(1-\frac{b_n^2}{a_n^2}\right).$$

Now if these strata are all similar, and have equal ellipticities,

$$\frac{b_1}{a_1}=\frac{b_2}{a_2}=\frac{b_3}{a_3}=\ldots\frac{b_n}{a_n};$$

and hence

$$\frac{C_1-A_1}{C_1}=\frac{C_2-A_2}{C_2}=\frac{C_3-A_3}{A_3}=\ldots\frac{C_n-A_n}{C_n}=\frac{1}{2}\left(1-\frac{b^2}{a^2}\right),$$

where b and a are the outer semiaxes of the shell composed of all the strata of equal density. But

$$\frac{1}{2}\left(1-\frac{b^2}{a_2}\right)=\frac{C-A}{C}=\frac{C_1+C_2+\ldots+C_n-(A_1+A_2+\ldots+A_n)}{C_1+C_2+\ldots+C_n}.$$

This is the symbolical form of the proposition just stated.

In a homogeneous solid of revolution the general expression for the moment of inertia is

$$\pi\int y^2 x dx;$$

and from the ordinary treatises on mechanics it readily appears that from a spheroid,

$$C=\frac{8}{15}\pi a^2 b,\quad A=B=\frac{4}{15}\pi a^2 b\,(a^2+b^2),$$

where b is the semi-polar and a the semi-equatorial axis. Hence we have

$$\frac{C-A}{C}=\frac{2a^4b-a^4b-a^2b^3}{2a^4b}=\frac{a^4b-a^2b^3}{2a^4b}=\frac{(a^2-b^2)}{2a^4b}a^2b=\frac{a^2-b^2}{2a^2}=\frac{1}{2}\left(1-\frac{b^2}{a^2}\right),$$

and

$$\frac{2\,(C-A)}{C}=\left(1-\frac{b^2}{a^2}\right).$$

In a spheroidal shell for whose inner surface the semi-axes are b_1 and a_1, we have the moments of inertia with respect to the axes by taking the moments for the inner spheroid bounded by b_1 and a_1 from those of the outer spheroid.

Calling the former C_1 and A_1, we have as before,

$$C_1=\frac{8}{15}\pi a_1{}^4 b_1,\quad A_1=\frac{4}{15}\pi a_1{}^2 b_1(a_1{}^2+b_1{}^2).$$

Calling C_1 and A_1 the moments of inertia of the shell, we have therefore,

$$C_1=\frac{8}{15}\pi\,(a^4b-a_1{}^4b_1),\quad A_1=\frac{4}{15}\pi\,[a^2b\,(a^2+b^2)-a_1{}^2b_1(a_1{}^2+b_1{}^2)];$$

and hence

$$\frac{C_1-A_1}{C_1}=\frac{a^2b(a^2-b^2)-a_1{}^2b_1(a_1{}^2b-b_1{}^2)}{2\,(a^4b-a_1{}^4b_1)}=\frac{a^4b\left(1-\frac{b^2}{a^2}\right)-a_1{}^4b_1\left(1-\frac{b_1{}^2}{a_1{}^2}\right)}{2\,(a^4b-a_1{}^4b_1)}.$$

If e and e_1 be the outer and inner ellipticities of the shell,

$$e=1-\frac{b}{a},\quad e_1=1-\frac{b_1}{a_1},\text{ and if } e=e_1,\ \frac{b}{a}=\frac{b_1}{a_1}.$$

In this case $\dfrac{C_1-A_1}{C_1}=\dfrac{(a^4b-a_1{}^4b_1)\left(1-\dfrac{b^2}{a^2}\right)}{2\,(a^4b-a_1{}^4b_1)}=\frac{1}{2}\left(1-\dfrac{b^2}{a^2}\right),$

or

$$\frac{C_1-A_1}{C_1}=\frac{C-A}{C}.$$

Consequently the precessional motion of such a shell would be the same as that of a homegeneous spheroid of the same ellipticity. If $e = \frac{1}{300}$, it appears that the value of precession for such a spheroid would be 57″, while its observed value is 50″·1.* Now, as it is impossible to admit such a difference where the result of observation is so well established, we must conclude that the solid shell of the Earth, composed of nearly equi-elliptic strata, can not extend to its center—in other words, that the Earth can not be altogether a solid from its surface to its center. On the other hand, the fluid nucleus contained within the shell can not be devoid of friction and viscidity, but must possess these properties in common with all fluids that have ever been observed on the Earth's surface. These properties of the liquid may, as I have long since announced, cause the shell and liquid nucleus to rotate together as one solid mass. The same conclusion was afterward put forward by M. Delauney; and experiments made under his direction, and afterward, at the instance of the Royal Irish Academy, by me, show that in rotating glass vessels filled with water the amount of friction and viscidity is such as to render any difference of slow motion between the liquid and its containing vessel insensible. With liquids so viscid that water is in comparison limpid, such as pitch, honey, and especially volcanic lava in a fused state, the results would be absolutely decisive. To this class of liquids the fluid matter of the Earth's interior, so far as it has come under observation, undoubtedly belongs; and hence the overwhelming certainty of our general conclusions as to the connection between the Earth's structure and its rotation.

(8) If the tendency of the solid crust is to become more elliptical at its inner surface as it increases in thickness, some interesting consequences appear to follow. If the shell were unaccompanied by the nucleus, or if no friction existed at their surfaces, the changes in the relations of the principal moments of inertia of the shell might be supposed to cause its rotation to become unstable, so as to bring about conditions which might result in a change of the axis of rotation. It is easy to show on the most favorable suppositions that this could not occur. The increasing ellipticity of the inner surface of the shell would be due to the increasing oblateness of the surface of the fluid nucleus, and this would be at its maximum if the neucleus approached a state of homogeneity; but the fluid can not approach this state unless the radius of the nucleus is so small that the variation in density due to pressure becomes insensible, whence all its strata would possess the same density. This condition with a certain thickness of the solid shell

* A revision of the numerical data from recent astronomical results leads me to conclude that the precession for the solid spheroid would be a little less, and about 55″ instead of 57″. This I propose to prove in a short paper, entitled "Note on the annual precession calculated on the hypothesis of the earth's solidity." This note [appended to this article] leaves the general conclusions of the present paper unaltered.

may bring about equality in the two principal moments of inertia of the shell. The most favorable case would be for a homogeneous shell. Hence we have only to solve the very simple problem: Given the thickness of a homogeneous spheroidal shell at its pole, required its thickness at the equator, so as to make its principal moments of inertia equal. We have from the expressions for C_1 and A_1 in (7),

$$a^2 b\,(a^2-b^2)=a_1^2\, b_1\,(a_1^2-b_1^2), \qquad \text{or } a_1^4-a_1^2\, b_1^2=\frac{a^2\, b\,(a^2\, b^2)}{b_1},$$

which gives

$$a_1=\frac{1}{\sqrt{2}}\sqrt{b_1^2+\sqrt{\frac{4\, a^2\, b\,(a^2-b^2)}{b_1}+b_1^4}}.$$

This may be written

$$\frac{a_1}{b_1}=\frac{1}{\sqrt{2}}\sqrt{1+\sqrt{\frac{4\, a^2\, b\,(a^2-b^2)}{b_1^5}+1}}.$$

If we take $e=\frac{1}{293}$ for the outer ellipticity of the shell, and $e_1=\frac{1}{230}$ for its maximum inner ellipticity, we can easily find the values of $\frac{a}{b}$ and $\frac{a_1}{b_1}$; from whence it appears that in order to have equal moments of inertia the thickness of the shell should be .047 of its equatorial semi-axis, and the mean radius of the nucleus would thus be reduced from the original value when the whole mass was fluid by a fraction less than one-twentieth. Under these conditions the ellipticity of $\frac{1}{230}$, corresponding to homogeneity, could not exist; and hence it may be concluded that, whether the shell is thin or whether the Earth has become almost altogether solid, the moment of the inertia of the shell with respect to its polar axis must be always greater than the moment of inertia for its equatorial axis.

The tendency of the fluid nucleus to increase in ellipticity might produce a result worthy of examination by volcanologists, namely, a possible increase in the development of volcanic phenomena in equatorial as compared to polar regions with the progressive solidification of the Earth up to a certain point. Until the thickness of the shell has become very great, recent periods should exhibit a greater development of volcanic energy towards the equator than toward the poles as compared to remote epochs.

NOTE.

On the annual precession calculated on the hypothesis of the Earth's solidity.

In discussing the influence of the internal structure of the Earth upon precession it has been frequently assumed that with the ellipticity $\frac{1}{300}$ the annual precession of a homogeneous solid shell or completely

solid spheroid would be 57″. This was the result of Mr. Hopkins's calculations; and the difference, amounting to between six and seven seconds between it and the observed value, formed the basis of all his conclusions relative to the Earth's internal condition. Hitherto I have not seen any reason for doubting the above numerical result; but on looking more closely into the question it appears probable that we must reduce the precession for the hypothetical solid spheroid to about 55″. If the Earth were a spheroid perfectly rigid, the amount of precession can be calculated from formulæ given in Airy's *Tracts*, Pratt's *Mechanical Philosophy*, Pontecoulant's *Théorie Analytique du Système du Monde*, or Resal's *Traité de Mécanique Celeste.* In the two latter works Poisson's memoir on the rotation of the Earth about its center of gravity is very closely followed, and the formulæ are those which I have generally employed. From these writings we have

$$P_1 = \frac{3m^2}{4n} \frac{(2C-A-B)}{C} (1+\gamma) \cos I;$$

where I is the inclination of the equator to the ecliptic, γ the ratio of the Moon's action on the Earth compared to that of the Sun, m the Earth's mean motion around the Sun, $\frac{m}{n}$ the ratio of this mean motion to the Earth's rotation, and A, B, C the three principal movements of the inertia of the Earth. When the Earth is supposed to be a spheroid of revolution, $A=B$, and the above becomes

$$(1)\quad P = \frac{3m^2}{2n} \frac{C-A}{C} . \quad (1+\gamma) \cos I.$$

Pratt gives the formula

$$(2)\quad P = \frac{3n^1}{2n} \left(\frac{C-A}{A}\right) \left\{ 1 + \frac{n^2}{n^1} \frac{1-\frac{3}{2}\sin^2 i}{1+\gamma} \right\} 180°;$$

where i is the inclination of the Moon's orbit to the ecliptic, γ the ratio of the Earth's mass to that of the Moon.

In all these formulæ, or in any others by which the precession can be calculated, the Moon's mass entérs directly or indirectly. When Mr. Hopkins made his calculation more than forty years ago, he appears to have taken the value of the Moon's mass and all his other numerical data from the early editions of Airy's *Tracts*. He uses 366.26 for the Earth's period, 27.32 for the Moon's. He makes $I=23° \ 28'$, $i=5° \ 8' \ 50''$, and the Moon's mass $\frac{1}{70}$ of the Earth's mass. All of these values require revision, and it may be remarked that Sir George Airy has more recently expressed the opinion that $\frac{1}{80}$ may be taken as the value of the Moon's mass.* On this question I may be permitted to remark

***Monthly Notices* of the Royal Astronomical Society, December, 1878, p. 140.

that there are three different phenomena from which the Moon's mass has been determined: (1) The perturbations of the Earth's motion in its orbit around the Sun by the action of the moon; (2) the tides; and (3) the nutation of the Earth's axis. The largest mass, or $\frac{1}{70}$ nearly, has been obtained from the first, and the smallest from nutation. But the values obtained from nutation are not very accordant, and moreover the close connection between nutation and precession makes it a doubtful matter to calculate the amount of one from a quantity depending on the other. The moon's mass obtained from the tides is that which has been employed by Laplace, Poisson, and other mathematicians as the most probable. It appears that a recent discussion of the tides in the United States, made by Mr. Ferrel, has given the same value as that found by Laplace. This circumstance, as well as the fact that the value so obtained lies between the values found by the other methods, gives us reason to place much confidence in the result. If we call P_1 the precession for a homogeneous spheroid whose ellipticity is E, then from (1)

$$P_1=\frac{3m^2}{2n}E(1+\gamma)\cos I.$$

If we take the value of the Moon's mass given by the tides, or rather the ratio of the Moon's action to that of the Sun thus given, we shall use the value of γ employed by Poisson, Pontécoulant, and Resal; if we also employ for E the value which Colonel Clarke shows good ground for deeming the most probable,* that is $\frac{1}{293\cdot46}$ instead of $\frac{1}{300}$ or even smaller fractions hitherto accepted, I find that P_1 becomes $56''\cdot05$. By Pratt's formula and the numerical values he employs, except for E, I find

$$P_1=54''\cdot879.$$

If we take $\frac{1}{80}$ for the Moon's mass in Poisson's formula, γ becomes 2·2062, and

$$P_1=53''\cdot574.$$

If we change γ to 80 in Pratt's formula with

$$E=\frac{1}{293\cdot46},\ P_1=52''\cdot95.$$

The value for the observed precession now generally admitted is $50''\cdot37$. It is therefore manifest that the difference between this and the precession of a homogeneous equi-elliptic spheroid can not be admitted to be as great as Mr. Hopkins has declared it to be. From the values of P_1 which I have calculated we should have

$$P_1-P=5''\cdot68 \text{ and } 4''\cdot507, \text{ with the Moon's mass}=\tfrac{1}{75};$$

* See Colonel Clarke's paper in the *Philosophical Magazine* for August, 1878, where he maintains that recent geodetical results tend to increase the value of the Earth's ellipticity and to make the measured value approach to that obtained from pendulum observations.

$$P_1-P=\frac{3''\cdot204}{3''\cdot617}, \text{ and } 2''.58, \text{ if we take the Moon's mass}=\tfrac{1}{80}.$$

On calculating P with the Moon's mass$=\frac{1}{80}$, Sun's mass 354936, γ is 2·25395. If we take for I its value in 1852, or 23° 27′ 32″, and make

$$m=359^\circ\cdot9931,\ \frac{m}{n}=\cdot0027303,\ E=\tfrac{1}{293\cdot46},$$

the following calculations can be made.

$$\begin{aligned}
&\log m &&=2\cdot5562965,\\
&\log(1+\gamma) &&=0\cdot5124109,\\
&\log\cos I &&=9\cdot9625322,\\
&\log\frac{m}{n} &&=\frac{-3+4362104,}{.4674500}\\
&\log\frac{3}{2}[60\times60] &&=\frac{3\cdot7323937}{4\cdot1998437},\\
&\log P_1 &&=\frac{2\cdot4675439}{1\cdot7322948}=\log 53''.988,\\
&\text{or } P_1=54'' \text{ nearly}, && P_1-P=3''\cdot617.
\end{aligned}$$

Consequently instead of admitting Mr. Hopkins's result of 7″ for the difference between the precession of a homogeneous spheroid with the Earth's ellipticity and the precession actually observed, we may affirm that this difference is probably not more than 4″ or 5″.

With the best values for the numerical elements the difference is, however, too well ascertained to be overlooked, and it leads to the conclusion that the Earth can not consist of an entirely solid mass composed of equi-elliptic strata, and that it is therefore partly composed of a solid shell bounded by surfaces such as I have elsewhere indicated, with an interior mass of viscid liquid, such as is seen flowing from the volcanic openings of the shell, arranged in strata conforming to the laws of hydrostatics, or in other words, with strata of equal density decreasing in ellipticity toward the Earth's center.

GLACIAL GEOLOGY.*

By Prof. JAMES GEIKIE, F. R. S.

The results obtained by geologists, who have been studying the peripheral areas of the drift-covered regions of our continent, are such as to satisfy us that the drifts of those regions are not iceberg-droppings, as we used to suppose, but true morainic matter and fluvio-glacial detritus. Geologists have not jumped to this conclusion; they have only accepted it after laborious investigations of the evidence. Since Dr. Otto Torell, in 1875, first stated his belief that the "diluvium" of north Germany was of glacial origin a great literature on the subject has sprung up, a perusal of which will show that with our German friends glacial geology has passed through much the same succession of phases as with us. At first icebergs are appealed to as explaining everything—next we meet with sundry ingenious attempts at a compromise between floating ice and a continuous ice-sheet. As observations multiply, however, the element of floating ice is gradually eliminated, and all the phenomena are explained by means of land ice and "schmelz-wasser" alone. It is a remarkable fact that the iceberg hypothesis has always been most strenuously upheld by geologists whose labors have been largely confined to the peripheral areas of drift-covered countries. In the upland and mountainous tracts, on the other hand, that hypothesis has never been able to survive a moderate amount of accurate observation. - - -

The notion of a general ice-sheet having covered a large part of Europe, which a few years ago was looked upon as a wild dream, has been amply justified by the labors of those who are so assiduously investigating the peripheral area of the "great northern drift." And perhaps I may be allowed to express my own belief that the drifts of middle and southern England, which exhibit the same complexity as the "lower diluvium" of the continent, will eventually be generally acknowledged to have had a similar origin.

I now pass on to review some of the general results obtained by con-

* Presidential address before the Geological Section of the British Association Adv. Sci. at Newcastle, September, 1889. (*Report of the British Association*, 1889, vol. LIX, pp. 552–564.)

tinental geologists as to the extent of area occupied by inland ice during the last great extension of glacier ice in Europe. It is well known that this latest ice-sheet did not overflow nearly so wide a region as that underneath which the lowest bowlder clay was accumulated. Gerard de Geer has given a summary* of the general results obtained by himself and his fellow-workers in Sweden and Norway; and these have been supplemented by the labors of Berendt, Geinitz, Hunchecone, Klockmann, Keilhack, Schröder, Wahnschaffe, and others in Germany, and by Sederholm in Finland. From them we learn that the end-moraines of the ice-circle round the southern coasts of Norway, from whence they sweep southeast by east across the province of Gottland in Sweden, passing through the lower ends of Lakes Wener and Wetter, while similar moraines mark out for us the terminal front of the inland ice in Finland at least two parallel frontal moraines passing inland from Hango head on the Gulf of Finland through the southern part of that province to the north of Lake Ladoga. Further northeast than this they have not been traced; but, from some observations by Helmersen, Sederholm thinks it probable that the terminal ice-front extended northeast by the north of Lake Onega to the eastern shores of the White Sea. Between Sweden and Finland lies the basin of the Baltic, which at the period in question was filled with ice, forming a great Baltic glacier which overflowed the Aland Islands, Gottland and Oland, and which, fanning out as it passed toward the southwest, invaded, on the south side, the Baltic provinces of Germany, while, on the north, it crossed the southern part of Scania in Sweden and the Danish islands to enter upon Jutland. - - -

The general conclusion arrived at by those who are at present investigating the glacial accumulations of northern Europe may be summarized as follows:

(1) Before the invasion of northern Germany by the inland ice the low grounds bordering on the Baltic were overflowed by a sea which contained a boreal and arctic fauna. These marine conditions are indicated by the presence, under the lower bowlder clay of more or less well-bedded fossiliferous deposits. On the same horizon occur also beds of sand, containing fresh-water shells, and now and again mammalian remains, some of which imply cold and other temperate climatic conditions. Obviously all these deposits may pertain to one and the same period, or more properly to different stages of the same period—some dating back to a time when the climate was still temperate, while others clearly indicate the prevalence of cold conditions, and are therefore probably somewhat younger.

(2) The next geological horizon in ascending order is that which is marked by the "Lower Diluvium"—the glacial and fluvioglacial detritus of the great ice-sheet which flowed south to the foot of the Harz Mountains. The bowlder clay on this horizon now and again contains

* *Zeitschrift d. deutsch. geolog. Ges.* Bd. XXXVII, p. 177.

marine, fresh-water, and terrestrial organic remains, derived undoubtedly from the so-called preglacial beds already referred to. These latter, it would appear, were plowed up and largely incorporated with the old ground moraine.

(3) The interglacial beds which next succeed contain remains of a well-marked temperate fauna and flora, which point to something more than a mere partial or local retreat of the inland ice. The geographical distribution of the beds and the presence in these of such forms as *Elephas antiquus*, *Cervus elephas*, *C. megaceros*, and a flora comparable to that now existing in northern Germany, justify geologists in concluding that the inter-glacial epoch was one of long duration, and characterized in Germany by climatic conditions apparently not less temperate than those that now obtain. One of the phases of that inter-glacial epoch, as we have seen, was the overflowing of the Baltic provinces by the waters of the North Sea.

(4) To this well-marked inter-glacial epoch succeeded another epoch of arctic conditions, when the Scandinavian inland ice once more invaded Germany, plowing through the inter-glacial deposits, and working these up in its ground moraine. So far as I can learn, the prevalent belief among geologists in north Germany is that there was only one inter-glacial epoch; but, as already stated, doubt has been expressed whether all the facts can be thus accounted for. There must always be great difficulty in the correlation of widely separated inter-glacial deposits, and the time does not seem to me to have yet come when we can definitely assert that all these inter-glacial beds belong to the same geological horizon.

I have dwelt upon the recent work of geologists in the peripheral areas of the drift-covered regions of northern Europe, because I think the results obtained are of great interest to glacialists in this country. And for the same reason I wish next to call attention to what has been done of late years in elucidating the glacial geology of the Alpine lands of central Europe, and more particularly of the low grounds that stretch out from the foot of the mountains. Any observations that tend to throw light upon the history of the complex drifts of our own peripheral areas can not but be of service. The only question concerning the ground moraines that has recently given rise to much discussion is the origin of the materials themselves. It is obvious that there are only three possible modes in which those materials could have been introduced to the ground moraine; either they consist of superficial morainic débris which has found its way down to the bottom of the old glaciers by crevasses; or they may be made up of the rock rubbish, shingle, gravel, etc., which doubtless strewed the valleys before these were occupied by ice; or, lastly, they may have been derived in chief measure from the underlying rocks themselves by the action of the ice that overflowed them. The investigations of Penck, Blaas, Böhm, and Brückner appear to me to have demonstrated that the ground moraines

are composed mostly of materials which have been detached from the underlying rocks by the erosive action of the glaciers themselves. Their observations show that the regions studied by them in great detail were almost completely buried under ice, so that the accumulation of superficial moraines was, for the most part, impossible; and they advance a number of facts which prove positively that the ground moraines were formed and accumulated under the ice. These geologists do not deny that some of the material may occasionally have come from above, nor do they doubt that preëxisting masses of rock rubbish and alluvial accumulations may have been incorporated with the ground moraines; but the enormous extent of the latter and the direction of transport and distribution of the erratics which they contain can not be thus accounted for, while all the facts are readily explained by the action of the ice itself, which used its subglacial débris as tools with which to carry on the work of erosion.

Professor Heim and others have frequently asserted that glaciers have little or no eroding power, since at the lower ends of existing glaciers we find no evidence of such erosion being in operation. But the chief work of a glacier cannot be carried on at its lower end, where motion is reduced to a minimum, and where the ice is perforated by sub-glacial tunnels and arches, underneath which no glacial erosion can possibly take place; and yet it is upon observations made in just such places that the principal arguments against the erosive action of glaciers have been based. - - - If we wish to learn what glacier-ice can accomplish, we must study in detail some wide region from which the ice has completely disappeared. Following this plan, Dr. Blaas has been led by his observations on the glacial formation of the Inn Valley to recant his former views, and to become a formidable advocate of the very theory which he formerly opposed. To his work and the memoirs by Penck, Brückner, and Böhm, already cited, and especially to the admirable chapter on glacier erosion by the last-named author, I would refer those who may be anxious to know the last word on this much-debated question.

The evidence of inter-glacial conditions within the Alpine lands continues to increase. These are represented by alluvial deposits of silt, sand, gravel, conglomerate, breccia, and lignites. Penck, Böhm, and Brückner find evidence of two interglacial epochs, and maintain that there have been three distinct and separate epochs of glaciation in the Alps. No mere temporary retreat and re-advance of the glaciers, according to them, will account for the phenomena presented by the inter-glacial deposits and associated morainic accumulations. During interglacial times the glaciers disappeared from the lower valleys of the Alps; the climate was temperate and probably the snow-fields and glaciers approximated in extent to those of the present day. All the evidence conspires to show that an interglacial epoch was of prolonged duration. Dr. Brückner has observed that the moraines of the last

glacial epoch rest here and there upon loess, and he confirms Penck's observations in South Bavaria that this remarkable formation never overlies the morainic accumulations of the latest glacial epoch. According to Penck and Brückner therefore the loess is of interglacial age. There can be little doubt, however, that loess does not belong to any one particular horizon. Wahnschaffe* and others have shown that throughout wide areas in north Germany it is the equivalent in age of the "Upper Diluvium," while Schumacher† points out that in the Rhine valley it occurs on two separate and distinct horizons. Professor Andreæ has likewise shown that there is an upper and lower löss in Alsace, each characterized by its own special fauna.‡

There is still considerable difference of opinion as to the mode of formation of this remarkable accumulation. By many it is considered to be an aqueous deposit; others, following Richthofen, are of opinion that it is a wind-blown accumulation, while some incline to the belief that it is partly the one and partly the other. Nor do the upholders of these various hypotheses agree amongst themselves as to the precise manner in which water or wind has worked to produce the observed results. Thus, amongst the supporters of the aqueous origin of the loess, we find this attributed to the action of heavy rains washing over and re-arranging the material of the bowlder clays.§ Many, again, have held it probable that loess is simply the finest loam distributed over the low grounds by the flood waters that escaped from the northern inland ice and the *mers de glace* of the Alpine lands of central Europe. Another suggestion is that much of the material of the loess may have been derived from the denudation of the bowlder clays by flood water during the closing stages of the last cold period. It is pointed out that in some regions at least the lœss is underlaid by a layer of erratics, which are believed to be the residue of the denuded bowlder clay. We are reminded by Klockmann‖ and Wahnschaffe¶ that the inland ice must have acted as a great dam, and that the wide areas in Germany, etc., would be flooded, partly by water derived from the melting inland ice and partly by waters flowing north from the hilly tracts of middle Germany. In the great basins thus formed there would be a commingling of fine silt material derived from north and south, which would necessarily come to form a deposit having much the same character throughout.

From what I have myself seen of the loess in various parts of Germany, and from all that I have gathered from reading and in conversation with those who have worked over loess-covered regions I incline

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* *Abhandl. z. geolog. Specialkarte v. Preussen, etc.*, Bd. VII, Heft 1: *Zeitschr. d. deutsch. geolog. Gesellsch.*, 1885, p. 904; 1886, p. 367.

† *Hygienische Topographie von Strassburg i. E.*, 1885.

‡ *Abhandl. z. geolog. Specialkarte v. Elsass-Lothringen*, Bd. VII, Heft 2.

§ Laspeyres: *Erläuterungen z. geolog. Specialkarte v. Preuessn, etc., Blatt Gröbzig, Zörbig und Petersberg.*

‖ Klockmann: *Jahrb. d. k. preuss. geolog. Landesanstalt für* 1883, p. 262.

¶ Wahnschaffe: *Op. cit.* and *Zeitschr. d. deutsch. geolog. Ges.*, 1886, p. 367.

to the opinion that loess is for the most part of aqueous origin. In many cases this can be demonstrated, as by the occurrence of bedding and the intercalation of layers of stones, sand, gravel, etc., in the deposit; again, by the not infrequent appearance of fresh-water shells; but perhaps chiefly by the remarkable uniformity of character which the loess displays. It seemed to me reasonable also to believe that the flood waters of glacial times must needs have been charged with finely divided sediment, and that such sediment would be spread over wide regions in the low grounds—in the slack waters of the great rivers and in the innumerable temporary lakes which occupied or partly occupied many of the valleys and depressions of the land. There are different kinds of loess or loess-like deposits, however, and all need not have been formed in the same way. Probably some may have been derived, as Wahnschaffe has suggested, from the denudation of bowlder clay. Possibly, also, some loess may owe its origin to the action of rain upon the stony clays, producing what we in this country would call "rain-wash." There are other accumulations, however, which no aqueous theory will satisfactorily explain. Under this category comes much of the so-called *Berglöss*, with its abundant land shells and its generally unstratified character. It seems likely that such loess is simply the result of sub aerial action, and owes its origin to rain, frost, and wind acting upon the superficial formations and re-arranging their finer-grained constituents. And it is quite possible that the upper portion of much of the loess of the lower grounds may have been re-worked in the same way. But I confess I can not yet find in the facts adduced by German geologists any evidence of a dry-as-dust epoch having obtained in Europe during any stage of the Pleistocene period. It is obvious, however, that after the flood waters had disappeared from the low grounds of the continent sub-aërial action would come into play over the wide regions covered by glacial and fluvio-glacial deposits. Thus, in the course of time these deposits would become modified, just as similar accumulations in these islands have been top-dressed, as it were, and to some extent even re-arranged.

I am strengthened in these views by the conclusion arrived at by M. Falsan, the eminent French glacialist. Covering the plateaux of the Dombs, and widely spread throughout the valleys of the Rhone, the Ain, the Isère, etc., in France, there is a deposit of loess, he says, which has been derived from the washing of the ancient moraines. At the foot of the Alps, where black schists are largely developed, the loess is dark gray; but west of the secondary chain the same deposit is yellowish and composed almost entirely of silicious materials, with only a very little carbonate of lime. This *limon*, or loess, however, is very generally modified towards the top by the chemical action of rain, the yellow loess acquiring a red color. Sometimes it is crowded with calcareous concretions; at other times it has been deprived of its calcareous element and converted into a kind of pulverulent silica or quartz. This, the true

loess, is distinguished from another, *lehm*, which Falsan recognizes as the product of atmospheric action, formed, in fact, in place from the disintegration and decomposition of the subjacent rocks. Even this lehm has been modified by running water, dispersed or accumulated locally, as the case may be.*

All that we know of the loess and its fossils compels us to include this accumulation as a product of the Pleistocene period. It is not of postglacial age, even much of what one may call the "remodified loess" being of Late Glacial or Pleistocene age. I can not attempt to give here a summary of what has been learned within recent years as to the fauna of the loess. The researches of Nehring and Liebe have familiarized us with the fact that at some particular stage in the Pleistocene period a fauna like that of the alpine steppe lands of western Asia was indigenous to middle Europe, and the recent investigations of Woldrich have increased our knowledge of this fauna. At what horizon, then, does this steppe fauna make its appearance? At Thiede Dr. Nehring discovered in so-called loess three successive horizons, each characterized by a special fauna. The lowest of these faunas was decidedly arctic in type; above that came a steppe fauna, which last was succeeded by a fauna comprising such forms as mammoth, woolly rhinoceros, *Bos*, *Cervus*, horse, hyæna, and lion. Now, if we compare this last fauna with the forms which have been obtained from true postglacial deposits, those deposits, namely, which overlie the younger bowlder clays and flood accumulations of the latest glacial epoch, we find little in common. The lion, the mammoth, and the rhinoceros are conspicuous by their absence from the postglacial beds of Europe. In place of them we meet with a more or less arctic fauna, and a high alpine and arctic flora, which, as we all know, eventually gave place to the flora and fauna with which Neolithic man was contemporaneous. As this is the case throughout northwestern and central Europe, we feel justified in assigning the Thiede beds to the Pleistocene period, and to that interglacial stage which preceded and gradually merged into the last glacial epoch. - - -

If the student of the Pleistocene fauna has certain advantages in the fact that he has to deal with forms many of which are still living, he labors at the same time under disadvantages which are unknown to his colleagues who are engaged in the study of the life of far older periods. The Pleistocene period was distinguished above all things by its great oscillations of climate, the successive changes being repeated and producing correlative migrations of floras and faunas. We know that arctic and temperate faunas and floras flourished during interglacial times, and a like succession of life forms followed the final disappearance of glacial conditions. A study of the organic remains met with in any particular deposit will not necessarily, therefore, enable us to assign these to their proper horizon. The geograph-

* Falsan: *La Période glaciaire*, p. 81.

ical position of the deposit and its relation to Pleistocene accumulations elsewhere must clearly be taken into account. Already, however, much has been done in this direction, and it is probable that ere long we shall be able to arrive at a fair knowledge of the various modifications which the Pleistocene floras and faunas experienced during the protracted period of climatic changes of which I have been speaking. We shall even possibly learn how often the arctic, steppe, prairie, and forest faunas, as they have been defined by Woldrich, replaced each other. Even now some approximation to this better knowledge has been made. Dr. Pohlig,* for example, has compared the remains of the Pleistocene faunas obtained at many different places in Europe, and has presented us with a classification which, although confessedly incomplete, yet serves to show the direction in which we must look for further advances in this department of inquiry.

During the last twenty years the evidence of interglacial conditions both in Europe and America has so increased that geologists generally no longer doubt that the Pleistocene period was characterized by great changes of climate. The occurrence at many different localities on the continent of beds of lignite and fresh-water alluvia, containing remains of Pleistocene mammalia, intercalated between separate and distinct bowlder clays, has left us no alternative. The interglacial beds of the Alpine lands of Central Europe are paralleled by similar deposits in Britain, Scandinavia, Germany, and France. But opinions differ as to the number of glacial and interglacial epochs, many holding that we have evidence of only two cold stages and one general interglacial stage. This, as I have said, is the view entertained by most geologists who are at work on the glacial accumulations of Scandinavia and North Germany. On the other hand, Dr. Penck and others, from a study of the drifts of the German alpine lands, believe that they have met with evidence of three distinct epochs of glaciation and two epochs of interglacial conditions. In France, while some observers are of opinion that there have been only two epochs of general glaciation, others, as for example, M. Tardy, find what they consider to be evidence of several such epochs. Others again, as M. Falsan, do not believe in the existence of any interglacial stages, although they readily admit that there were great advances and retreats of the ice during the glacial period. M. Falsan, in short, believes in oscillations, but he is of the opinion that these were not so extensive as others maintained. It is, therefore, simply a question of degree, and whether we speak of oscillations or of epochs we must needs admit the fact that through all the glaciated tracts of Europe fossiliferous deposits occur intercalated among glacial accumulations. The successive advance and retreat of

*Pohlig: *Sitzungsb. d. Niederrheinischen Gesellschaft zu Bonn*, 1884: *Zeitschr. d. deutsch. geolog. Ges.*, 1887, p. 798. For a very full account of the diluvial European and Northern Asiatic mammalian faunas by Woldrich, see *Mém. de l'Acad. des Sciences de St.-Petersbourg*, 1887, 7e sér., tom. XXXV.

the ice, therefore, was not a local phenomenon, but characterized all the glaciated areas. And the evidence shows that the oscillations referred to were on a gigantic scale.

The relation borne to the glacial accumulations by the old river alluvia which contain relics of paleolithic man early attracted attention. From the fact that these alluvia in some places overlie glacial deposits the general opinion (still held by some) was that paleolithic man must needs be of postglacial age. But since we have learned that all bowlder clay does not belong to one and the same geological horizon—that, in short, there have been at least two, and probably more, epochs of glaciation—it is obvious that the mere occurrence of glacial deposits underneath paleolithic gravel does not prove these latter to be postglacial. All that we are entitled in such a case to say is simply that the implement-bearing beds are younger than the glacial accumulations upon which they rest. Their horizon must be determined by first ascertaining the relative position in the glacial series of the underlying deposits. Now, it is a remarkable fact that the bowlder clays which underlie such old alluvia belong, without exception, to the earlier stages of the glacial period. This has been proved again and again, not only for this country but for Europe generally. I am sorry to reflect that some twenty years have now elapsed since I was led to suspect that the paleolithic gravels and cave deposits were not of post-glacial but of glacial and inter-glacial age. In 1871–'72 I published a series of papers in the *Geological Magazine*, in which I set forth the views I had come to form upon this interesting question. In these papers it was maintained that the alluvia and cave deposits could not be of post-glacial age, but must be assigned to pre-glacial and inter-glacial times, and in chief measure to the latter. Evidence was adduced to show that the latest great development of glacier ice in Europe took place after the southern pachyderms and paleolithic man had vacated England; that during this last stage of the glacial period, man lived contemporaneously with a northern and alpine fauna in such regions as southern France; and, lastly, that paleolithic man and the southern mammalia never re-visited northwestern Europe after extreme glacial conditions had disappeared. These conclusions were arrived at after a somewhat detailed examination of all the evidence then available, the remarkable distribution of the paleolithic and ossiferous alluvia having, as I have said, particularly impressed me. I colored a map to show at once the areas covered by the glacial and fluvio-glacial deposits of the last glacial epoch, and the regions in which the implement-bearing and ossiferous alluvia had been met with, when it became apparent that the latter never occurred at the surface within the regions occupied by the former. If ossiferous alluvia did here and there appear within the recently glaciated areas, it was always either in caves or as infra- or inter-glacial deposits. Since the date of these researches our knowledge of the geographical distribution of Pleisto-

cene deposits has greatly increased, and implements and other relics of paleolithic man have been recorded from many new localities throughout Europe. But none of this fresh evidence contradicts the conclusions I had previously arrived at; on the contrary, it has greatly strengthened my general argument. - - -

Thus as years advance the picture of Pleistocene times becomes more and more clearly developed. The conditions under which our old paleolithic predecessors lived—the climatic and geographical changes of which they were the witnesses—are gradually being revealed with a precision that only a few years ago might well have seemed impossible. This of itself is extremely interesting, but I feel sure that I speak the conviction of many workers in this field of labor when I say that the clearing up of the history of Pleistocene times is not the only end which they have in view. One can hardly doubt that when the conditions of that period and the causes which gave rise to these have been more fully and definitely ascertained we shall have advanced some way towards the better understanding of the climatic conditions of still earlier periods. - - - It would almost seem as if all one had to do to ascertain the climatic condition of any particular period was to prepare a map depicting with some approach to accuracy the former relative position of land and sea. With such a map could our meteorologists infer what the climatic conditions must have been? Yes, provided we could assure them that in other respects the physical conditions did not differ from the present. Now, there is no period in the past history of our globe the geological conditions of which are better known than the Pleistocene. And yet when we have indicated these upon a map we find that they do not give the results which we might have expected. The climatic conditions which they seem to imply are not such as we know did actually obtain. It is obvious, therefore, that some additional and perhaps exceptional factor was at work to produce the recognized results. What was this disturbing element, and have we any evidence of its interference with the operation of the normal agents of climatic changes in earlier periods of the world's history? We all know that various answers have been given to such questions. Whether amongst these the correct solution of the enigma is to be found time will show. Meanwhile, as all hypothesis and theory must starve without facts to feed on, it behooves us as working geologists to do our best to add to the supply. The success with which other problems have been attacked by geologists forbids us to doubt that ere long we shall have done much to dispel some of the mystery which still envelopes the question of geological climates.

THE HISTORY OF THE NIAGARA RIVER.*

By G. K. Gilbert.

The Niagara River flows from Lake Erie to Lake Ontario. The shore of Erie is more than 300 feet higher than the shore of Ontario; but if you pass from the higher shore to the lower, you do not descend at a uniform rate. Starting from Lake Erie and going northward, you travel upon a plain—not level, but with only gentle undulations—until you approach the shore of Lake Ontario, and then suddenly you find yourself on the brink of a high bluff or cliff overlooking the lower lake, and separated from it only by a narrow strip of sloping plain. The bird's-eye view in Plate I is constructed to show the relations of these various features, the two lakes, the broad plateau lying a little higher than the shore of Lake Erie, the cliff, which geologists call the Niagara Escarpment, and the narrow plain at its foot.

Where the Niagara River leaves Lake Erie at Buffalo and enters the plain, a low ridge of rock crosses its path, and in traversing this its water is troubled; but it soon becomes smooth, spreads out broadly, and indolently loiters on the plain. For three-fourths of the distance it can not be said to have a valley, it rests upon the surface of the plateau; but then its habit suddenly changes. By the short rapid at Goat Island and by the cataract itself the water of the river is dropped 200 feet down into the plain, and thence to the cliff at Lewiston it races headlong through a deep and narrow gorge. From Lewiston to Lake Ontario there are no rapids. The river is again broad, and its channel is scored so deeply in the littoral plain that the current is relatively slow, and the level of its water surface varies but slightly from that of the lake.

The narrow gorge that contains the river from the Falls to Lewiston is a most peculiar and noteworthy feature. Its width rarely equals the fourth of a mile, and its depth to the bottom of the river ranges from 200 to 500 feet. Its walls are so steep that opportunities for climbing up and down them are rare, and in these walls one may see the

* This essay contains the substance of a lecture read to the American Association for the Advancement of Science at its Toronto meeting, August, 1889. (From the Sixth Annual Report of the Commissioners of the State Reservation at Niagara, 1888–'89. Transmitted to the legislature January 22, 1890. pp. 61–84.)

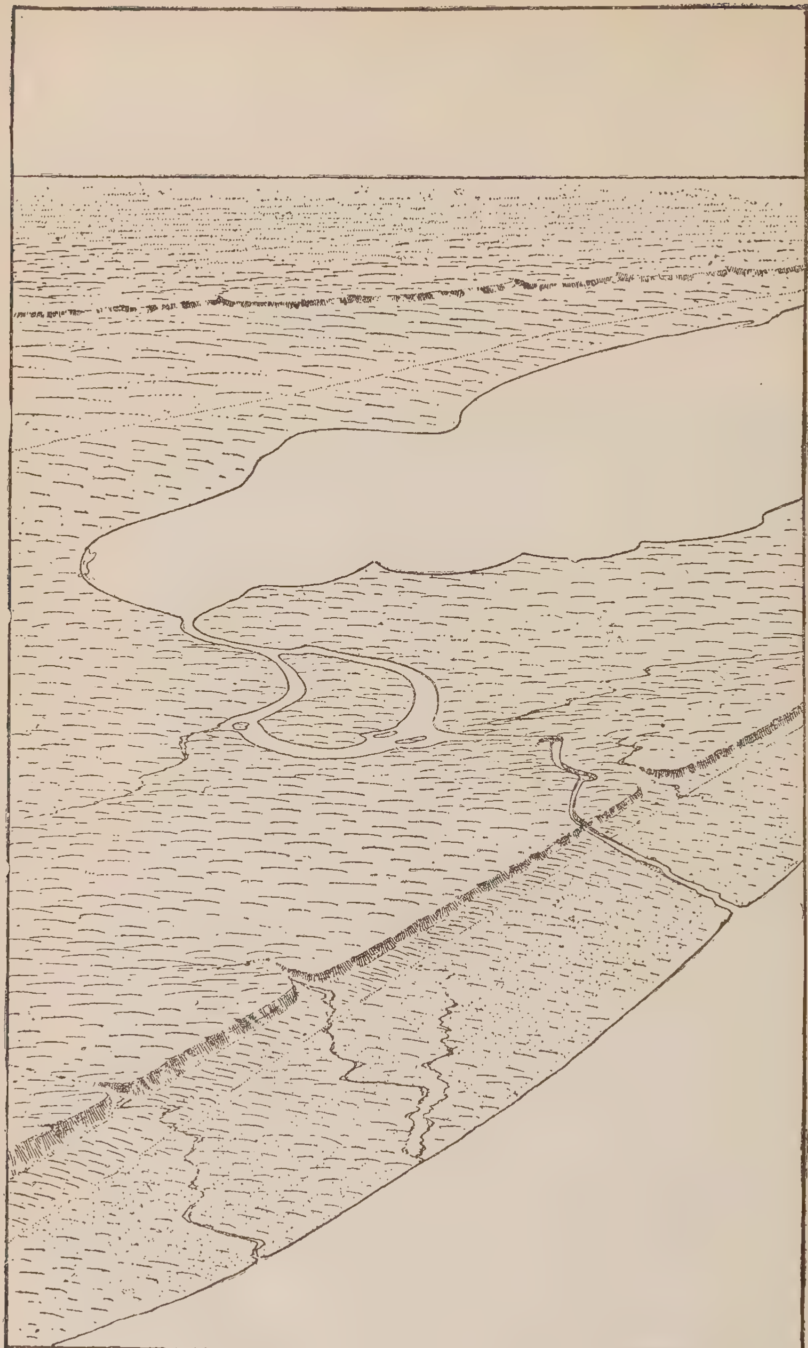


PLATE I.—BIRD'S-EYE VIEW OF NIAGARA RIVER.

geologic structure of the plateau. They are constituted of bedded rocks—limestone, shale, and sandstone—lying nearly horizontal, and a little examination shows that the same strata occur in the same order on both sides. So evenly are they matched, and so uniform is the general width of the gorge, that one might suspect, after a hasty examination, the two sides had been cleft asunder by some Plutonic agency. But those who have made a study of the subject have reached a different and better conclusion—the conclusion that the trench was excavated by running water, so that the strata of the two sides are alike because they are parts of continuous sheets, from each of which a narrow strip has here been cut.

The contour of the cataract is subject to change. From time to time blocks of rock break away, falling into the pool below, and new shapes are then given to the brink over which the water leaps Many such falls of rock have taken place since the white man occupied the banks of the river, and the breaking away of a very large section is still a recent event. By such observation we are assured that the extent of the gorge is increasing at its end, that it is growing longer, and that the cataract is the cause of its extension.

This determination is the first element in the history of the river. A change is in progress before our eyes. The river's history, like human history, is being enacted, and from that which occurs we can draw inferences concerning what has occurred, and what will occur. We can look forward to the time when the gorge now traversing the fourth part of the width of the plateau will completely divide it, so that the Niagara will drain Lake Erie to the bottom. We can look back to the time when there was no gorge, but when the water flowed on the top of the plain to its edge, and the Falls of Niagara were at Lewiston.

We may think of the river as laboring at a task—the task of sawing in two the plateau. The task is partly accomplished. When it is done the river will assume some other task. Before it was begun what did the river do?

How can we answer this question? The surplus water discharge from Lake Erie can not have flowed by this course to Lake Ontario without sawing at the plateau. Before it began the cutting of the gorge it did not flow along this line. It may have flowed somewhere else, but if so it did not constitute the Niagara River. The commencement of the cutting of the Niagara gorge is the beginning of the history of the Niagara River. We have accomplished somewhat of our purpose if we have discovered that our river had a beginning.

We are so accustomed to think of streams, and especially large streams, as permanent, as flowing on forever, that the discovery of a definite beginning to the life of a great river like the Niagara is important and impressive. But that discovery does not stand alone. Indeed, it is but one of a large class of similar facts familiar to students

of geology. Let us consider for a moment the tendency of stream histories and the tendency of lake histories. Wherever streams fall over rocky ledges in rapids or in cataracts, their power of erosion is greatly increased by the rapid descent, and they deepen their channels. If this process continues long enough, the result must be that each stream will degrade its channel through the hard ledges until the descent is no more rapid there than in other parts of its course. It follows that a stream with cascades and water-falls and numerous rapids is laboring at an unfinished task. It is either a young stream, or else nature has recently put obstructions in its path.

Again, consider what occurs where a lake interrupts the course of a stream. The lower part of the stream, the outflowing part, by deepening its channel continually tends to drain the lake. The upper course, the inflowing stream, brings mud and sand with it and deposits them in the still water of the lake, thus tending to fill its basin. Thus, by a double process, the streams are laboring to extinguish the lakes that lie in their way, and given sufficient time, they will accomplish this.

Now, if you will study a large map of North America, you will find that the region of the Great Lakes is likewise a region of small lakes. A multitude of lakes, lakelets, ponds, and swamps where ponds once were, characterize the surface from the Great Lakes northward to the Arctic Ocean, and for a distance southward into the United States. In the same region waterfalls abound, and many streams consist of mere alternations of rapids and pools. Further south, in the region beyond the Ohio River, lakes and cataracts are rare. The majority of the streams flow from source to mouth with regulated course, their waters descending at first somewhat steeply, and gradually becoming more nearly level as they proceed. At the south the whole drainage system is mature; at the north it is immature. At the south it is old; at the north, young.

The explanation of this lies in a great geologic event of somewhat recent date—the event known as the age of ice. Previous to the ice age our streams may have been as tame and orderly as those of the Southern States, and we have no evidence that there were lakes in this region. During the ice age the region of the Great Lakes was somewhat in the condition of Greenland. It was covered by an immense sheet of ice and the ice was in motion. In general it moved from north to south. It carried with it whatever lay loose upon the surface. It did more than this, for just as the soft water of a stream, by dragging sand and pebbles over the bottom, wears its channel deeper, so the plastic ice, holding grains of sand and even large stones in its under surface, dragged these across the underlying rock, and in this way not only scoured and scratched it, but even wore it away.

In yet other ways the moving ice mass was analogous to a river. Its motion was perpetual, and its form changed little, but that which moved was continually renewed. As a river is supplied by rain, so the

glacier was supplied by snow falling upon regions far to the north. To a certain extent the glacier discharged to the ocean like a river, breaking up into icebergs and floating away; but its chief discharge was upon the land, through melting. The climate at its southern margin was relatively warm, and into this warm climate the sheet of ice steadily pushed and was as steadily dissolved.

Whatever stones and earth were picked up or torn up by the ice moved with it to its southern margin and fell to the ground as the ice melted. If the position of the ice margin had been perfectly uniform its continuously deposited load might have built a single high wall; but as the seasons were cold or warm, wet or dry, the ice margin advanced and retreated with endless variation, and this led to the deposition of irregular congeries of hills, constituting what is known as the "drift deposit." Eventually the warm climate of the south prevailed over the invader born of a cold climate, compelling it to retreat. The motion of the ice current was not reversed, but the front of the glacier was melted more rapidly than it could be renewed, and thus its area was gradually restricted. During the whole period of retrenchment the deposition of drift proceeded at the margin of the ice, so that the entire area that it formerly occupied is now diversified by irregular sheets and heapings of earth and stone.

The ancient configuration of the country was more or less modified by the erosive action of the ice, and it was further modified by the deposits of drift. The destructive and constructive agencies together gave to the land an entirely new system of hills and valleys. When the ice was gone the rain that fell on the land could no longer follow the old lines of drainage. Some of the old valleys had perhaps been obliterated; others had been changed so that their descent was in a different direction, and all were obstructed here and there by the heaps of drifts. The waters were held upon the surface in innumerable lakes, each overflowing at the lowest side of its basin, and thus giving birth to a stream that descended to some other lake. Often the new lines of descent—the new water courses—crossed regions that before had had no streams, and then they were compelled to dig their own channels. Thus it was that the whole water system of a vast region was refashioned, and thus it has come to pass that the streams of this region are young.

Like every other stream of the district of the Great Lakes, the Niagara was born during the melting of the ice, and so we may begin our chronicle with the very beginning of the river.

If you will again call to mind the features of a general map of the United States and Canada, and consider the direction in which the streams flow, you will perceive that there is a continuous upland, a sort of main divide, separating the basin of the Great Lakes from the basin of the Mississippi.* It is not a mountain range. In great part it is a

* A part of its course appears as a broken line on the maps in Figs. 3 and 4.

region of hills. In places it is only the highest part of the plain; but it is nevertheless a continuous upland, else the waters would not be parted along its course. When the ice had its greatest extent it passed over this upland, so that the waters produced by its melting fell into the Ohio and other tributaries of the Mississippi, as well as into streams that discharged to Delaware and Chesapeake Bays. Afterward, when the glacier gradually fell back, there came a time when the ice front lay in the main to the north of the great water parting, but had not yet receded from the Adirondack Mountains, so that the water that flowed from the melting glacier could not escape by way of the St. Lawrence River, but gathered as a lake between the upland divide and the ice front. In fact, it formed not one but many lakes, each discharging across the divide by some low pass, and as the great retreat progressed these lakes were varied in number and extent, so that their full history is exceeding complex.

The surfaces of these lakes were stirred by the winds, and waves beat upon their shores. In places they washed out the soft drift and carved clifts; elsewhere they fashioned spits and bars. These cliffs and spits and other monuments of wave work survive to the present time, and have made it possible to trace out and map certain of the ancient lakes. The work of surveying them is barely begun, but from what is known we may add a chapter to the history of our river.

There was a time when one of these lakes occupied the western portion of the basin of Lake Erie, and discharged across the divide at the point where the city of Fort Wayne now stands, running into the Wabash River and thence into the Ohio. The channel of this discharge is so well preserved that its meaning can not be mistaken, and the associated shore lines have been traced for many miles eastward into Ohio and northward into Michigan. Afterward this lake found some other point of discharge, and a new shore line was made 25 feet lower. Twice again the point of discharge was shifted and other shore lines were formed. The last and lowest of the series has been traced eastward across the States of Ohio and Pennsylvania and into western New York, where it fades away in the vicinity of the town of Careyville. At each of the stages represented by these four shore lines the site of the Niagara was either buried beneath the ice or else submerged under the lake bordering the ice. There was no river.

The next change in the history of the lakes was a great one. The ice, which had previously occupied nearly the whole of the Ontario basin, so far withdrew as to enable the accumulated water to flow out by way of the Mohawk Valley. The level of discharge was thus suddenly lowered 550 feet, and a large district previously submerged became dry land. Then for the first time Lake Erie and Lake Ontario were separated, and then for the first time the Niagara River carried the surplus water of Lake Erie to Lake Ontario.

The waves of the new-born Lake Ontario at once began to carve

about its margin a record of its existence. That record is wonderfully clear, and the special training of the geologist has not been necessary to the recognition of its import. The earliest books of travel in western New York describe the Ridge road, and tell us that the ridge of sand and gravel which it follows was even then recognized by all residents as an ancient beach of the lake.* In the Province of Ontario, the beach was examined and described by the great English geologist, Charles Lyell, during his celebrated journey in America,† and it afterward received more careful study by Mr. Sandford Fleming,‡ and by the geologists of the Canadian Survey.§ In western New York it was traced out by the great American geologist, James Hall, during his survey of the geology of the fourth district of the State.‖ Within a few years more attention has been given to detail. Prof. J. W. Spencer has traced the line continuously from the head of the lake at Hamilton, past Toronto, Windsor, and Grafton, in the vicinity of Belleville,¶ beyond which point it is hard to follow. South of the lake, I myself have traced it from Hamilton to Queenstown and Lewiston, thence to Rochester, and all about the eastern end of the basin to Watertown, beyond which point it is again difficult to trace. Southeast of the present margin of Lake Ontario there was a great bay, extending as far south as Cayuga Lake, and including the basin of Oneida Lake, and it was from this bay that the discharge took place, the precise point of overflow being the present site of the city of Rome. For this predecessor of Lake Ontario Professor Spencer has proposed the name of Iroquois.

Putting together the results of his survey and of my own, I have been able to prepare a map (Pl. II) exhibiting with a fair amount of detail the outline of the old lake. It will be observed that the northeastern portion of the shore is not traced out. In fact it is not traceable. The water was contained on that side by the margin of the glacier, and with the final melting of the ice all record of its shore vanished.

The form and extent of Lake Iroquois, and the form and extent of each other lake that bordered the ice front, were determined partly by the position of the pass over which the discharge took place, and by the contour of the land; but they were also determined to a great extent by the peculiar attitude of the land.

* C. Schultz, jr. Travels on an Inland Voyage - - - in the years 1807 and 1808, New York, 1810, p. 85.

De Wit Clinton. Discourse before the New York Historical Society, 1811, p. 58.

Francis Hall. Travels in Canada and the United States in 1816 and 1817, Boston, 1818, p. 119.

† Travels in North America in the years 1841–'42. New York, 1845, vol. 2, pp. 86, 87.

‡ Sandford Fleming. Notes on the Davenport gravel drift. *Canadian Journal*, new series, vol. 6, pp. 247–253.

§ Geological Survey of Canada, report to 1863, pp. 914, 915.

‖ Natural History of New York. Geology, Part IV, pp. 348–354.

¶ Communicated to the Philosophical Society of Washington, to be published in vol. 11 of the Bulletin of the Society.

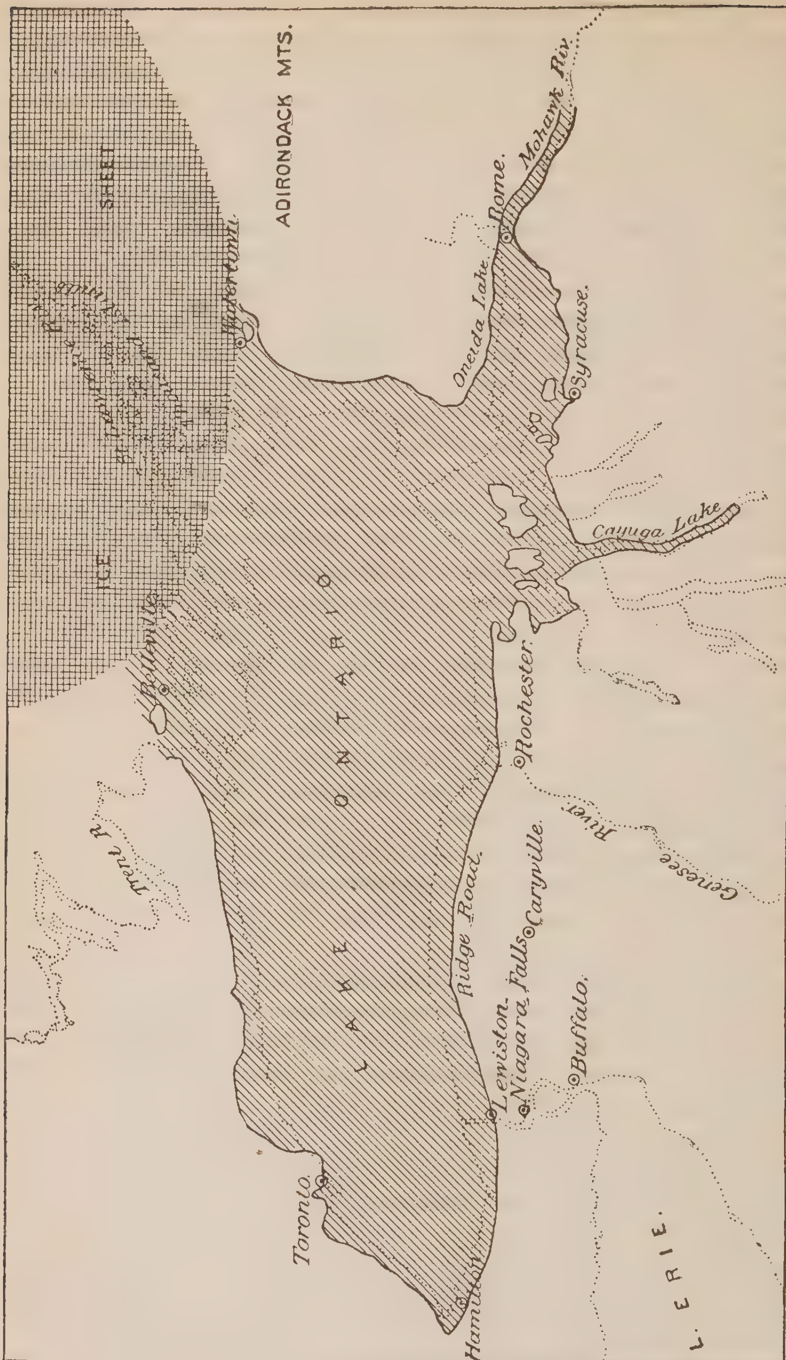


PLATE II.—MAP OF LAKE IROQUOIS.

Perhaps a word of general explanation is necessary in speaking of the attitude of the land. Geologists are prone to talk of elevation and subsidence—of the uprising of the earth's crust at one place or at one time, and of its down-sinking at another place or another time. Their language usually seems to imply the rise or fall of an area all together, without any relative displacement of its parts; but you will readily see that, unless a rising or sinking tract is torn asunder from its surroundings, there must be all about it a belt in which the surface assumes an inclined position, or, in other words, where the attitude of the land is changed. If the district whose attitude changes is a lake basin, the change of attitude will cause a change in the position of the line marked about the slopes of the basin by the water margin, and it may even cause the overflow of the basin to take a new direction.

The Ontario basin has been subjected to a very notable change of attitude, and the effect of this change has been to throw the ancient shore line out of level. When the shore line was wrought by the waves, all parts of it must have lain in the same horizontal plane, and had there been no change in the attitude of the basin, every point of the shore line would now be found at the level of the old outlet at Rome. Instead of this, we find that the old gravel spit near Toronto—the Davenport ridge—is 40 feet higher than the contemporaneous gravel spit on which Lewiston is built; at Belleville, Ontario, the old shore is 200 feet higher than at Rochester, New York; at Watertown 300 feet higher than at Syracuse; and the lowest point, in Hamilton, at the head of the lake, is 325 feet lower than the highest point near Watertown. From these and other measurements we learn that the Ontario basin with its new attitude inclines more to the south and west than with the old attitude.

The point of discharge remained at Rome as long as the ice was crowded high against the northern side of the Adirondack Mountains, but eventually there came a time when the water escaped eastward between the ice and the mountain slope. The line of the St. Lawrence was not at once opened, so that the subsidence was only partial. The water was held for short times at various intermediate levels, recorded at the east in a series of faint shore lines. Owing to the attitude of the land, these shores are not traceable all about the basin, but pass beneath the present water level at various points.

Finally the ice blockade was raised in the St. Lawrence Valley, and the present outlet was established. During the period of final retreat the attitude of the land had slowly changed, so that it was not then so greatly depressed at the north as before; but it had not yet acquired its present position, and for a time Lake Ontario was smaller than now, its western margin lying lower down on the slope of the basin.

An attempt has been made in Pl. III to exhibit diagramatically the relations of ice dams and basin attitudes to one another and to the river. The various elements are projected, with exaggeration of heights, on a

PLATE III.—DIAGRAM TO ILLUSTRATE THE RELATIONS OF WATER LEVELS IN THE ONTARIO BASIN TO ATTITUDES OF THE LAND AND TO OUTLETS.

vertical plane running a little west of south, or parallel to the direction of greatest inclination of old water-planes. At N is represented the Niagara escarpment and the associated slope of the lake basin; at A the Adirondack Mountains. R and T are the passes at Rome and at the Thousand Islands. Successive positions of the ice front are marked at I^1, I^2, and I^3. The straight line numbered 1 represents the level of lake water previous to the origin of the Niagara River; 2 gives the first position of the water level after the establishment of the Rome outlet; and the level gradually shifted to 3; 4 is the first of the series of temporary water levels when the water escaped between the mountain slope and the ice front; 5 represents the first position of the water level after the occupation of the Thousand Island outlet; and 6, the present level of Lake Ontario.

It should be added parenthetically that the shore of Lake Iroquois as mapped in Pl. II is not quite synchronous. Between 2 and 3 of Pl. III there was a continuous series of water levels, but it was not easy to map any one except the highest. The northern part of the map delineates the margin of water level 2 and the southern part the margin of water level 3.

It is easy to see that these various changes contribute to modify the history of the Niagara River. In the beginning, when the cataract was at Lewiston, the margin of Lake Ontario, instead of being 7 miles away, as now, was only 1 or 2 miles distant, and the level of its water was about 75 feet higher than at present. The outlet of the lake was at Rome, and while it there continued there was a progressive change in the attitude of the land, causing the lake to rise at the mouth of the Niagara until it was 125 feet higher than now. It fairly washed the foot of the cliff at Queenston and Lewiston. Then came a time when the lake fell suddenly through a vertical distance of 250 feet, and its shore retreated to a position now submerged. Numerous minor oscillations were caused by successive shiftings of the point of discharge, and by progressive changes in the attitude of the land, until finally the present outlet was acquired, at which time the Niagara River had its greatest length. It then encroached 5 miles on the modern domain of Lake Ontario, and began a delta where now the lead-line runs out 30 fathoms.

While the level of discharge was lower than now, the river had different powers as an eroding agent. The rocks underlying the low plain along the margin of the lake are very soft, and where a river flows across yielding rocks the depth to which it erodes is limited chiefly by the level of its point of discharge. So when the point of discharge of the Niagara River—the surface of the lake to which it flowed—was from 100 to 200 feet lower than now, the river carved a channel far deeper than it could now carve. When afterward the rise of land in the vicinity of the outlet carried the water gradually up to its present position in the basin this channel was partly filled by sand and

other débris brought by the current; but it was not completely filled and its remarkable present depth is one of the surviving witnesses of the shifting drama of the Ontario. Near Fort Niagara 12 fathoms of water are shown on the charts.

Mr. Warren Upham has made a similar discovery in the basin of the Red River of the North. That basin held a large lake, draining southward to the Mississippi—a lake whose association with the great glacier Upham appropriately signalized by naming it after the apostle of "the glacial theory," Louis Agassiz. The height of the old Agassiz shore has been carefully measured by Mr. Upham, through long distances, and it is found to rise continuously, though not quite uniformly, toward the north. Similar discoveries have been made in the basins of Erie, Huron, and Michigan, and the phenomena all belong approximately to the same epoch. So, while the details remain to be worked out, the general fact is already established that during the epoch of the ice retreat the great plain constituting the Laurentian basin was more inclined to the northward than at present.

It was shown, first in the case of Lake Agassiz, and afterward, as already stated, in the case of Lake Ontario, that the change from the old attitude of the land to the present attitude was in progress during the epoch of the ice retreat. The land was gradually rising to the north or northeast. In each lake basin the water either retreated from its northern margin, so as to lay bare more land, or encroached on its southern margin, or else both these changes occurred together; and in some cases we have reason to believe that the changes were so extensive that the outlets of lakes were shifted from northerly passes to more southerly passes.

To illustrate the effect of the earlier system of land slopes upon the distribution of water in the region of the Great Lakes I have constructed the map in Pl. IV. It does not postulate the system of levels most divergent from the present system, but a system such as may have existed at the point of time when the last glacial ice was melted from the region. The modern system of drainage is drawn in broken lines; the hypothetic system in full lines, with shading for the lake areas; and a heavier broken line toward the bottom of the map marks the position of the present water-parting at the southern edge of the Laurentian basin.

In the ancient system of drainage, Georgian Bay, instead of being a dependency of Lake Huron, is itself the principal lake, and receives the overflow from Huron. It expands toward the northeast so as to include the basin of Lake Nipissing, and its discharge is across a somewhat low pass at the east end of Lake Nipissing, and thence down the Ottawa River to the St. Lawrence. Lake Michigan, instead of communicating with Lake Huron by a strait, forms a tributary lake, discharging its surplus through a river. Lake Superior has the same relations as now, but its overflow traverses a greater distance before

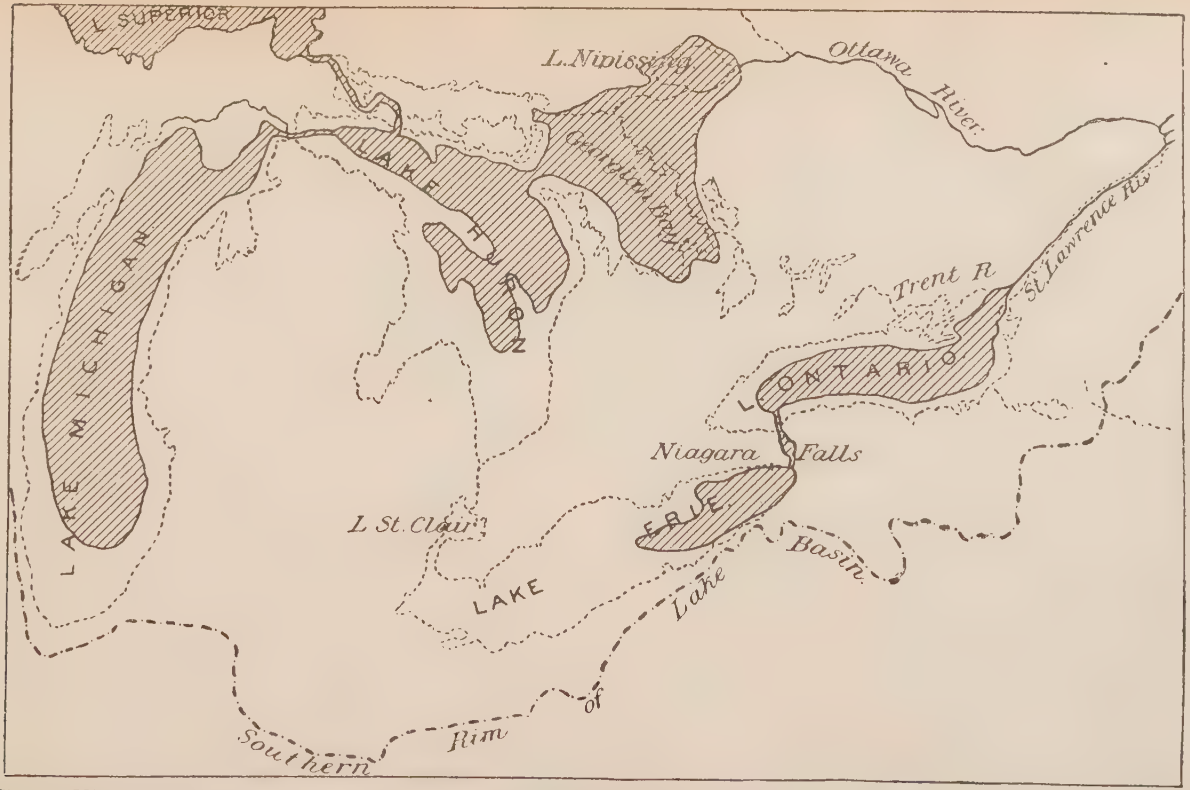


PLATE IV.—HYPOTHETIC HYDROGRAPHY AT A DATE AFTER THE MELTING OF THE GREAT GLACIER FROM THE ST. LAWRENCE VALLEY.

EXPLANATION.—Water-parting in heavy broken line. Modern hydrography in light broken lines. Ancient rivers in full lines. Ancient lakes shaded.

reaching Lake Huron. Superior, Michigan, Huron, and Georgia constitute a lake system by themselves, independent of Erie and Ontario, and the channel of the Detroit River is dry. Lake Erie and Lake Ontario, both greatly reduced in size, constitute another chain, but their connecting link, the Niagara River, is a comparatively small stream, for the diversion of the upper lakes robs the river of seven-eighths of its tributary area.

Whether this hypothetic state of drainage ever existed, whether the ice retreated from the Nipissing pass while still the changing attitude of the land was such as to turn the Georgian outlet in that direction, are questions not yet answered. But such data as I have at present incline me to the belief that for a time the upper lakes did discharge across the Nipissing pass.

Professor Spencer has decribed a channel by which Georgian Bay once drained across a more southerly pass to the valley of the Trent River, and thence to Lake Ontario.* He states that there is an ancient shore line about Georgian Bay associated with this outlet, and that he has traced this line westward and southward until it comes down to the shore of Lake Huron, demonstrating that during the existence of that outlet also, the Detroit River ran dry. The Trent pass is much higher than the Nipissing pass, so that it appears necessary to assume that during the history of the Trent outlet for the upper lakes the great glacier still occupied the region of Lake Nipissing, preventing the escape of the water in that direction.

The map in Pl. V represents the system of lakes and outlets at that time. It is largely theoretic, but at the same time I believe its general features consistent with our present knowledge of the facts.

Unless I have misunderstood Professor Spencer, Lake Ontario was at high stage in the first part of the epoch of the Trent Valley outlet, and was afterwards at low stage. I have selected as the date of my map the epoch of the high stage, with the outlet of Ontario at Rome, and have indicated an ice sheet so extensive as to block the way not only at Lake Nipissing but at the pass of the Thousand Islands. The date of this map is earlier than the other; it belongs to a time when the northward depression of the land was greater. Lake Erie is represented as less in extent, for its basin in that position would hold less water. Huron and Ontario would likewise be smaller were their waters free to escape over the lowest passes; but the ice blocks the way, and so their waters are raised to the level of higher passes. Of the contemporaneous relations of the upper lakes we know nothing at present. They are drawn as though communicating with Lake Huron, but it is equally possible that they fell into some other drainage system. Here again the Detroit channel was not in use, and the Niagara River was outlet only for the waters of the Erie basin.

* *Proceedings Am. Assoc. Adv. Sci.*, 37th Meeting (Cleveland), pp. 198–199.

PLATE V.—HYPOTHETIC HYDROGRAPHY AT A DATE BEFORE THE MELTING OF THE GREAT GLACIER FROM THE ST. LAWRENCE VALLEY.

EXPLANATION.—Water-parting in heavy broken line. Modern hydrography in light broken lines. Ancient rivers in full lines. Ancient lakes shaded. Ice sheet cross-shaded.

Graphic methods are ill adapted to the communication of qualified or indefinite statements. By the aid of a map one can indicate definitely the relation of Albany to other places and things, but he cannot say indefinitely that Albany is somewhere in eastern New York, nor can he say, with qualification, that it is probably on the Mohawk River. For this reason I have decided to publish these two maps only after hesitation, because I should greatly regret to produce the impression that the particular configuration of lakes and outlets here delineated has been actually demonstrated. The facts now at command are suggestive rather than conclusive, and when the subject shall have been fully investigated it is to be expected that the maps representing these epochs will exhibit material differences from those I have drawn. The sole point that I wish to develop at this time is the probability that during a portion of the history of the Niagara River its drainage district—that area from which its water was supplied—was far less than it is at the present time. There is reason to believe that during an epoch which may have been short or long—we can only vaguely conjecture—the Niagara was a comparatively small river.

The characters of the gorge are in general remarkably uniform from end to end. Its width does not vary greatly; its course is flexed but slightly; its walls exhibit the same alternation of soft and hard rocks. But there is one exceptional point. Midway, its course is abruptly bent at right angles. On the outside of the angle there is an enlargement of the gorge, and this enlargement contains a deep pool, called the Whirlpool. At this point, and on this side only, the material of the wall has an exceptional character. At every other point there is an alternation of shales, sandstones, and limestones, capped above by an unequal deposit of drift. At this point limestones, sandstones, and shales disappear, and the whole wall is made of drift. Here is a place where the strata that floor the plateau are discontinuous, and must have been discontinuous before the last occupation of the region of the glacier, for the gap is filled by glacial drift.

Another physiographic feature was joined to this by Lyell and Hall. They observed that the cliff limiting the plateau has, in general, a very straight course, with few indentations. But at the town of St. David's, a few miles west of Queenston. a wide flaring gap occurs. This gap is partly filled by drift, and although the glacial nature of the drift was not then understood, it was clearly perceived by those geologists that the drift-filled break marked the position of a line of erosion established before the period of the drift. Putting together the two anomalies, they said that the drift-filled gap at the Whirlpool belonged to the same line of ancient erosion with the drift-filled gap at St. David's.* Their conclusion has been generally accepted by subsequent investigators, but the interpretation of the phenomena was carried

* Travels in North America. By Charles Lyell. New York, 1845. Vol. II, pp. 77–80. Natural History of New York. Geology, Part IV. By James Hall, pp. 389–390.

little further until the subject was studied by Dr. Julius Pohlman.* He pointed out that the upper course of the ancient gorge could not have lain outside the modern gorge. If the course of one gorge lay athwart the course of the other, we should have two breaks in the continuity of the strata, instead of the single one at the Whirlpool. The upper part of the ancient gorge necessarily coincides with a part of the modern gorge; and so when the cataract, in the progressive excavation of the cañon, reached a point at the Whirlpool where it had no firm rock to erode, it had only to clear out the incoherent earth and bowlders of glacial drift. To whatever distance the gorge of the earlier stream extended, the modern river found its laborious task performed in advance.

Let us put together what we have learned of the Niagara history. The river began its existence during the final retreat of the great ice sheet, or, in other words, during the series of events that closed the age of the ice in North America. If we consider as a geologic period the entire time that has elapsed since the beginning of the age of ice, then the history of the Niagara River covers only a portion of that period. In the judgment of most students of glacial geology, and, I may add, in my own judgment, it covers only a small portion of that period.

During the course of its history the length of the river has suffered some variation by reason of the sucessive fall and rise of the level of Lake Ontario. It was at first a few miles shorter than now; then it became suddenly a few miles longer, and its present length was gradually acquired.

With the change in the position of its mouth there went a change in the height of its mouth; and the rate at which it eroded its channel was affected thereby. The influence on the rate of erosion was felt chiefly along the lower course of the river, between Lewiston and Fort Niagara.

The volume of the river has likewise been inconstant. In early days, when the lakes levied a large tribute on the melting glacier, the Niagara may have been a larger river than now; but there was a time when the discharge from the upper lakes avoided the route by Lake Erie, and then the Niagara was a relatively small stream.

The great life work of the river has been the digging of the gorge through which it runs from the cataract to Lewiston. The beginning of its life was the beginning of that task. The length of the gorge is in some sense a measure of the river's age. In the main the material dug has been hard limestone and sandstone, interbedded with a coherent though softer shale; but for a part of the distance the material was incoherent drift.

The geologic age of the earth—the time during which its surface has been somewhat as now, divided into land and ocean, subject to endless waste on the land and to endless accumulation of sediment in the

Proceedings Am. Assoc. Adv. Sci., 35th meeting (Buffalo), pp. 221–222.

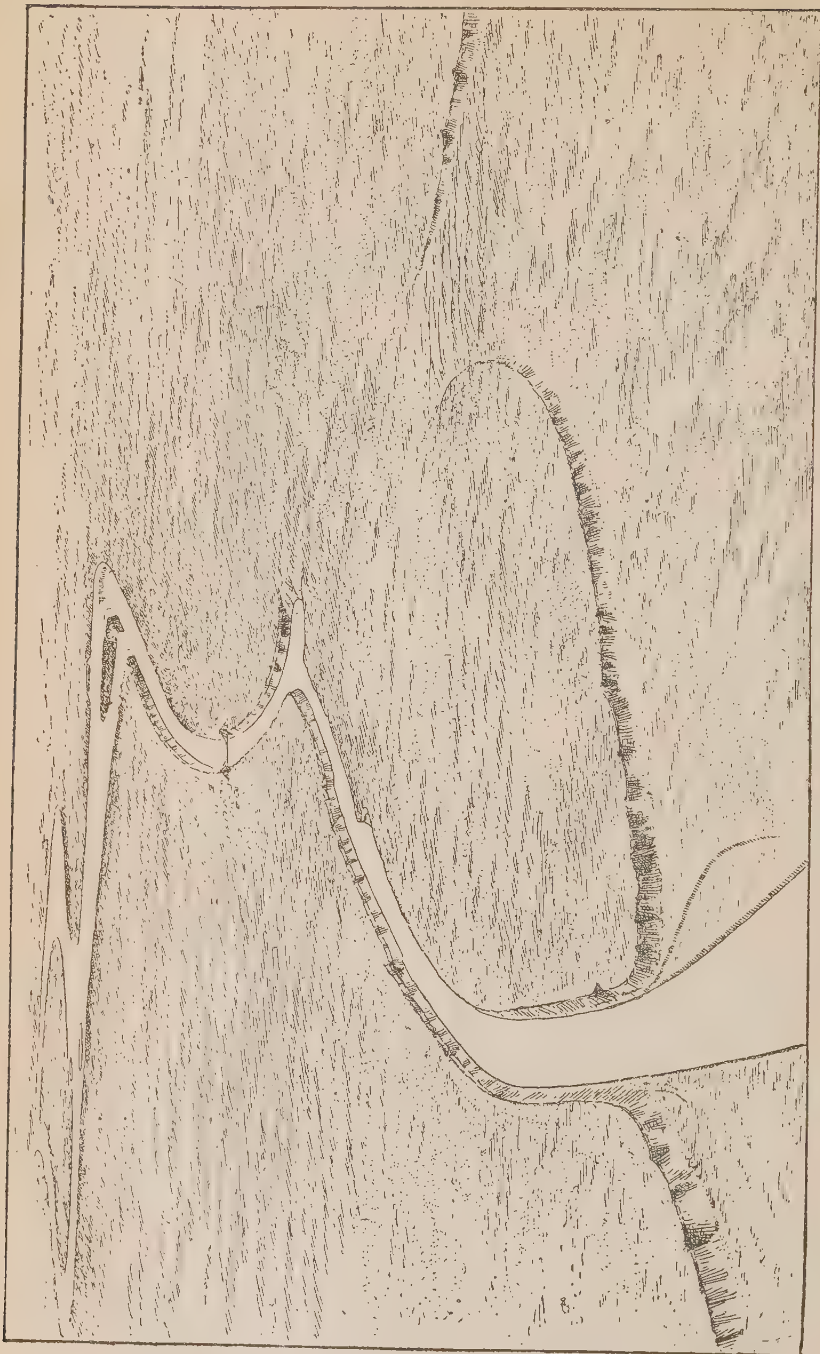


PLATE VI.—BIRD'S-EYE VIEW OF THE NIAGARA GORGE.

ocean, green with verdure and nourishing the varied forms of animal life—this time is of immense duration. Even the units into which geologists divide it, the periods and epochs of their chronology are themselves of vast duration. Human history is relatively so short, and its units of centuries and years are so exceedingly brief, that the two orders of time are hardly commensurate. Over and over again the attempt has been made to link together the two chronologies, to obtain for the geologic units some satisfactory expression in the units of human history. It can not in fairness be said that all these attempts have failed, for some of them are novel and untested; but, however successful or unsuccessful they may have been, the interest in the subject remains, and no discussion of the history of the Niagara River would be complete without some allusion to its value as a geologic chronometer. It is true we know but little of the ratio the river epoch bears to the extent of the glacial period, or to any longer geologic unit; but yet were we able to determine, even approximately, the time consumed by the river in cutting its gorge, we should render less hazy and vague our conception of the order of magnitude of the units of the earth's geologic history. The problem has been attacked by numerous writers, and the resulting estimates have ranged from three or four thousand years to three or four million years.

The method of reaching a time estimate has been, first, to estimate the present rate of recession—the rate at which the cataract is increasing the length of the gorge; second, to compute, with the aid of this estimate and the known length of the gorge, the time necessary for the entire excavation; and, third, some writers have modified their result by giving consideration to various conditions affecting the rate of erosion during earlier stages of the excavation. The enormous range of the resulting estimates of time has depended chiefly upon the imperfection of data with reference to the present rate of recession of the falls. It is but a few years since measurement of the rate of recession was substituted for bald guessing.

This measurement consists in making surveys and maps of the falls at different times, so that the amount of change in the interval between surveys can be ascertained by comparison of the maps. In 1842 Professor Hall made a survey of the outlines of the falls, and he published, for the use of future investigators, not only the map resulting from the survey, but also the bearings taken with the surveying instrument in determining the principal points of the map.* He likewise left upon the ground a number of well-marked monuments to which future surveys could be referred. Thirty-three years later a second survey was made by the United States Army Engineers, and they added still further to the series of bench marks available for future reference. Three years ago my colleage, Mr. R. S. Woodward, executed a third survey.†

* Natural History of New York, Geology, Part IV, pp. 402, 403.

† *Science*, vol. VIII, 1886, p. 205.

Plate VII exhibits the outline of the crest of the falls, together with the brink of the cliff in the vicinity of the falls, as determined by Mr. Woodward in 1886, and also shows a part of the same outline as determined by Professor Hall 44 years earlier.* If both were precise, the area included between the two lines would exactly represent the recession of the Horseshoe and American falls in 44 years, and the retreat of the cliff face at Goat Island in the same time. I regret to say that there is internal evidence pointing to some defect in one or both surveys, for there are some points at which the Woodward outline projects farther towards the gorge than the Hall outline, and yet we can not believe that any additions have been made to the face of the cliff. Nevertheless, a critical study, not merely of these bare lines on the chart, but also of the fuller data in the surveyors' notes, leads to the belief that the rate of recession in the central part of the Horseshoe Fall is approximately determined, and that it is somewhere between 4 and 6 feet per annum. The amount fallen away at the sides of the Horseshoe is not well determined, but this is of less importance, for such falling away affects the width of the gorge rather than its length, and it is the length with which we are concerned.

The surveys likewise fail to afford any valuable estimate of the rate of retreat of the American Fall, merely telling us that its rate is far less than that of the Horseshoe—a result that might be reached independently by going back in imagination to the time when the two falls were together at the foot of Goat Island, and considering how much greater is the distance through which the Horseshoe Fall has since retreated. The rate of retreat of the central portion of the Horseshoe is the rate at which the gorge grows longer.

Now if we were to divide the entire length of the gorge by the space through which the Horseshoe Fall retreats in a year, we might regard the resulting quotient as expressing the number of years that the falls have been occupied with their work. This is precisely the procedure by which the majority of time estimates have been deduced, but in my judgment it is not defensible. It implies that the rate of retrogression has been uniform, or, more precisely, that the present rate of retrogression does not differ from the average rate, and this implication is open to serious question. I conceive that future progress in the discussion of the time problem will consist chiefly in determining in what ways the conditions or circumstances that affect the rate of retrogression have varied in past time. In order to discuss intelligently these conditions, it is necessary to understand just what is the process by which the river increases the length of its gorge.

There can be no question that the cataract is the efficient engine, but what kind of an engine is it? What is the principle on which it works?

* The south side of this chart is placed uppermost (in violation of the conventional rule) so that it may accord with the bird's-eye views.

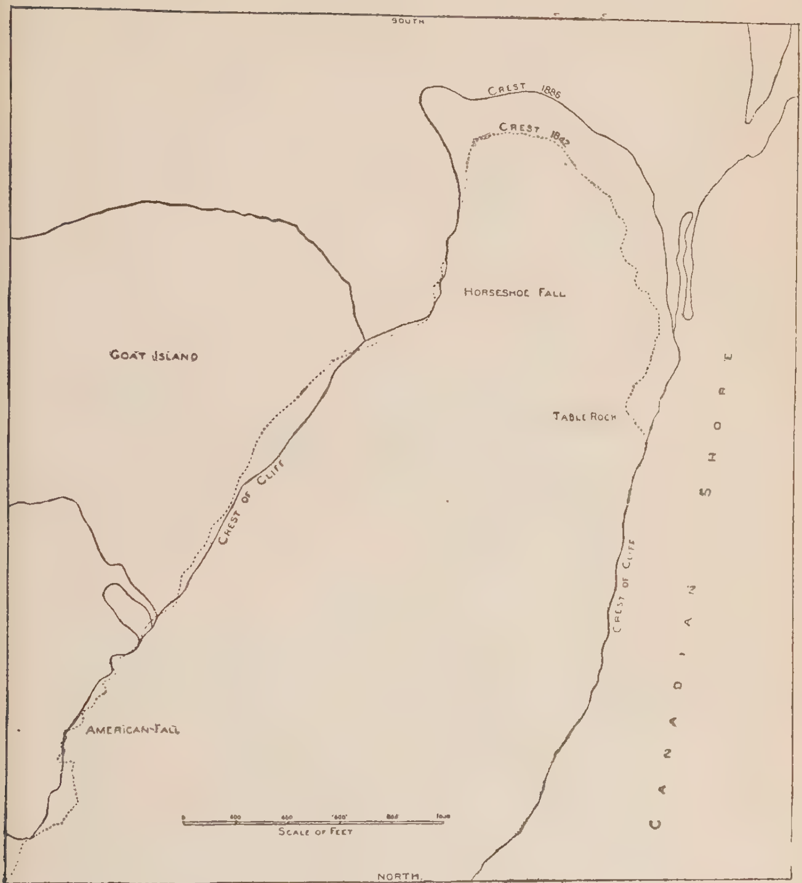


PLATE VII.—CHART OF THE CLIFF LINE AT THE HEAD OF THE NIAGARA GORGE, COMPILED TO SHOW THE RECESSION FROM 1842 TO 1886.

EXPLANATION.—Broken line, crest of falls and cliff as mapped by N. Y. State Geol. Survey in 1842. Full line, crest of falls as mapped by the U. S. Geol. Survey in 1886, with other features as mapped by the U. S. Lake Survey in 1875.

It has already been stated that the rocks at the falls lie in level layers. The order of succession of the layers has much to do with the nature of the cataract's work. Above all is a loose sheet of drift, but this yields so readily to the wash of the water that we need pay no attention to it at present. Under that is a bed of strong limestone. This is called the Niagara limestone, and in thickness is 80 feet. Beneath it is a shale, called the Niagara shale, with a thickness of 50 feet; and then for 35 feet there is an alternation of limestone, shale, and sandstone, known collectively as the Clinton group. This reaches down very nearly to the water's edge. Beneath it and extending downward for several hundred feet is a great bed of soft, sandy shale, interrupted, so far as we know, by a single hard layer, a sandstone ledge, varying in thickness from 10 to 20 feet. These are the Medina shales and the Medina sandstone. The profile in the figure indicates that the hard layers project as shelves or steps, and that the softer layers are eaten back. I have been led so to draw them by considerations of analogy only, for underneath the center of the great cataract no observations have been made. We only know that the river leaps from the upper surface of the Niagara limestone and strikes upon the water of the pool. The indicated depth of the pool, too, is a mere surmise, for in that commotion of waters direct observation is out of the question. But where the United States Engineers were able to lower their plummet, a half a mile away, a depth was discovered of nearly 200 feet, and I have assumed that the cataract is scouring as deeply now as it scoured at the time when that part of the gorge was dug.

It is a matter of direct observation that from time to time large blocks of the upper limestone fall away into the pool, and there seems no escape from the inference that this occurs because the erosion of the shale beneath deprives the limestone of its support. Just how the shale is eroded and what is the part played by the harder layers beneath are questions in regard to which we are much in doubt. In the Cave of the Winds, where one can pass beneath and behind one of the thinner segments of the divided fall, the air is filled with spray and heavier masses of water that perpetually dash against the shale, and though their force in that place does not seem to be violent, it is possible that their continual beating is the action that removes the shaly rock. The shale is of the variety known as calcareous, and as its calcareous element is soluble, it may be that solution plays its part in the work of undermining. What goes on beneath the water of the pool must be essentially different. The Niagara River carries no sediment, and therefore can not scour its channel in the manner of most rivers, but the fragments of the limestone bed that fall into the pool must be moved by the plunging water, else they would accumulate and impede its work, and being moved, we can understand that they become powerful agents of excavation. Water plunging into a pool acquires a gyratory motion, and, carrying detritus about with it, sometimes bores deep

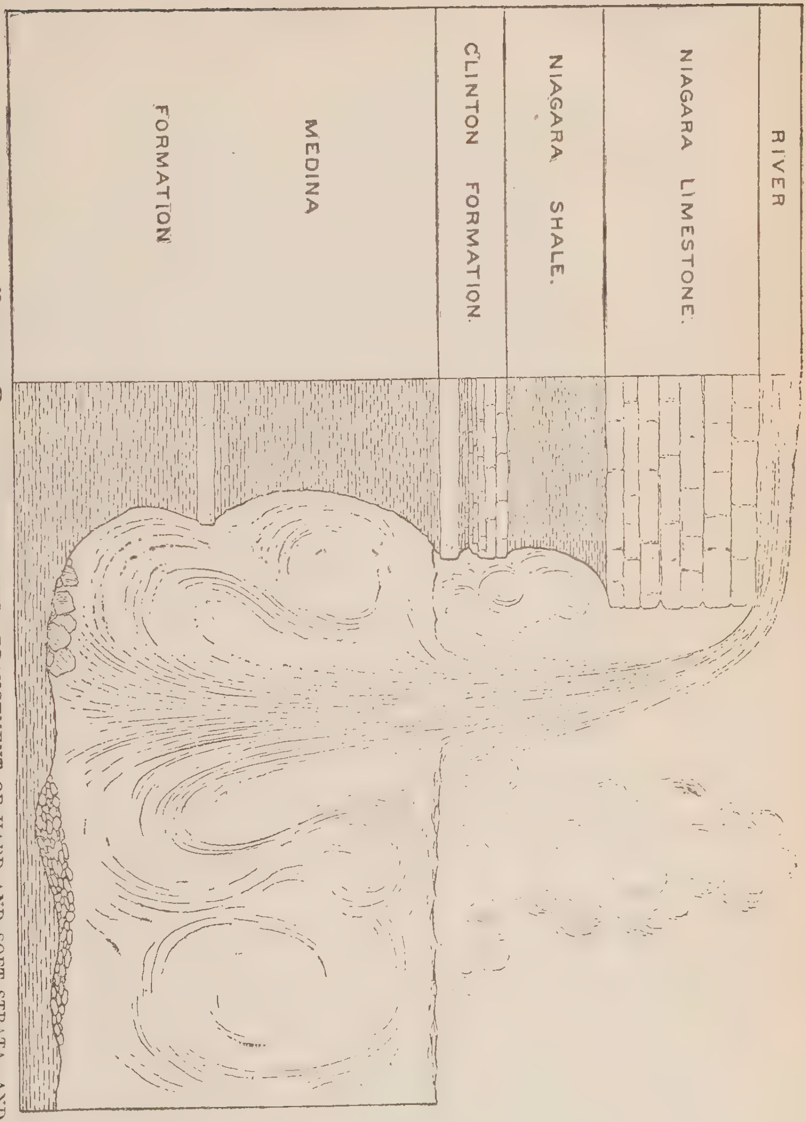


PLATE VIII.—SECTION OF NIAGARA FALLS, SHOWING THE ARRANGEMENT OF HARD AND SOFT STRATA, AND ILLUSTRATING A THEORY OF THE PROCESS OF EROSION.

holes, even in rocks that are hard. These holes are called technically "pot-holes," and there is much to commend the suggestion that the excavation within the pool is essentially pot-hole work.*

The process which I have described is that which takes place in the central part of the Horseshoe Fall, where the greatest body of water is precipitated. At the margin of the Horseshoe, and also at the American Fall, in which places the body of falling water is much less, the process is different. There is there no pot-hole action and no pool. The fallen blocks of limestone form a low talus at the foot of the cliff, and upon them the force of the descending water is broken and spent. Such of you as have made the excursion through the Cave of the Winds will recall that though for a few steps you traveled upon an undisturbed rock stratum, one of the layers of the Clinton group, the greater part of the journey lay across large fallen blocks of limestone, irregularly heaped. Where, then, the volume of falling water is relatively small, the great bed of shale below the Clinton ledges plays no part, and the rate at which the limestone breaks away is determined purely by the rate of erosion of the shale bed lying just beneath it.

The difference between the two processes is of great importance in the present connection, because the two rates of erosion are very different.

I am fully aware that this sketch of the cataract's work is not a satisfactory explanation of the mode of recession, but it yet serves a present purpose, for it renders it possible to point out that the rate of recession is affected by certain factors which may have varied during the early history of the river. We see that the process of recession is concerned with a heavy bed of hard rock above, with beds of softer rock beneath, with the force of falling water, and possibly, also, with the solvent power of the water.

Concerning each of these factors a number of pertinent questions may be asked, questions that should certainly be considered, whether they are answered or not, before any solution of the time problem is regarded as satisfactory. To illustrate their pertinence, a few will be propounded.

Question 1. Does the limestone vary in constitution in different parts of the gorge? If its texture or its system of cracks and joints varies, the process of recession may vary in consequence.

Question 2. How does the limestone bed vary in thickness in different parts of the gorge? This question is easily answered, for at all points it is well exposed for measurement.

Question 3. How is the thickness of the limestone related to the rate of recession? This is more difficult. The débris from a very thick bed of limestone would oppose great resistance to the cataract and check its work. The débris from a very thin bed would afford small and inefficient pestles for pot-hole action, and might lead to a slow rate of

* I am indebted for this suggestion to Mr. W J McGee.

recession. If the thickness now seen at the cataract were slightly increased or slightly diminished, it is not at once apparent how the rate of recession would be affected, and yet there might be an important difference.

We have seen that the pre-glacial stream whose channel is betrayed at the Whirlpool removed the Niagara limestone through a portion of the gorge, and

Question 4 asks: Through what portion of the gorge was the Niagara limestone absent when the Niagara River began its work?

Question 5. Does the rock section beneath the limestone—the shale series with its imbedded harder layers—does this vary in different parts of the gorge?

Question 6. Through what distance were the several members of the underlying rock series removed by the action of the pre-glacial stream?

Coming now to consider the force of the falling water, a little consideration serves to show that the force depends on at least three things: The height through which the water falls, the degree of concentration of the stream, and the volume of the river.

The height of the fall is the vertical distance from its crest to the surface of the pool below.

Question 7 asks: How has the height of the crest of the fall varied during the history of recession?

Question 8. How has the height of the base of the fall varied? And this involves a subsidiary question—to what extent has the excavated gorge, as left by the retreating cataract, been re-filled, either by the falling in of fragments from the cliffs or by contributions of débris brought by the current?

Question 9. What has been the form of the channel at the crest of the fall from point to point during the recession? Wherever the chanhas been broad, and the water of uniform depth from side to side, the force of the falling water has been applied disadvantageously; wherever the channel has been narrow, or has been much deeper in some parts than in others, the force of the water has been applied advantageously.

There are many ways in which it is possible that the volume of the river was made to differ at early dates from its present volume. During the presence of the ice there was a different climate, and there were different drainage systems.

Question 10. During the early history of the river was the annual rainfall on which its water supply depended greater or less than now?

Question 11. Was the evaporation from the basin at that time greater or less than now? It is believed that at the present time the Niagara River receives less than half the water that falls upon its basin in rain and snow, the remainder being returned to the air by evaporation from the lakes, from the surface of the land, and from vegetation.

Question 12. Was the water supply increased by ablation? There

may have been times when the overlapping edge of the glacier discharged to the Laurentian Basin large bodies of water furnished by the melting of ice that had congealed from the clouds of regions far away.

Question 13. Was the drainage area of the river at any time increased through the agency of ice barriers? Just as the Winnipeg basin was made to send its water to the Mississippi, so we can imagine that regions north of the Great Lakes and now tributary to Hudson's Bay had their discharge temporarily turned to Lake Superior and Lake Huron.

On the other hand, we have seen that the discharge of the whole district of the upper lakes was for a time turned away from the Niagara River. Therefore we ask:

Question 14. To what extent and for what periods was the volume of the river diminished through the diversion of the discharge of the upper lakes?

Assuming all these questions to be answered one by one, and the variations of different sorts determined, it is still necessary to learn the relations of those variations to each other, and so we ask:

Question 15. How have the variations of rock section, the variations of cataract height, the variations of form of channel, and the variations of volume been related to one another in point of time? What have been their actual combinations?

Question 16. How have the various temporary combinations of factors affected the process of retreat and the rate of recession?

The tale of questions is not exhausted, but no more are needed if only it has been shown that the subject is not in reality simple, as many have assumed, but highly complex. Some of the questions are, indeed, easily answered. It may be possible to show that others are of small moment. It may even be that careful study of the local features will enable the investigator to infer the process of cataract work at each point from the existing condition of the gorge, and thus relieve him from the necessity of considering such remote questions as the nature of glacial climate and the history of glacial retreat. But after all paring and pruning, what remains of the problem will be no bagatelle. It is not to be solved by a few figures on a slate, nor yet by the writing of many essays. It is not to be solved by the cunning discussion of our scant, yet too puzzling, knowledge—smoothing away inconvenient doubts with convenient assumptions and cancelling out, as though compensatory, terms of unknown value that happen to stand on opposite sides of the equation. It is a problem of nature, and, like other natural problems, demands the patient gathering of many facts, of facts of many kinds, of categories of facts suggested by the tentative theories of to-day, and of new categories of facts to be suggested by new theories.

I have said our problem is but the stepping stone to another problem, the discovery of common units for earth history and human history. The Niagara bridges the chasm in another way, or, more strictly, in

another sense, for the term of its life belongs to both histories. The river sprang from a great geologic revolution, the banishment of the dynasty of cold, and so its lifetime is a geologic epoch; but from first to last man has been the witness of its toil, and so its history is interwoven with the history of man. The human comrade of the river's youth was not, alas, a reporter with a notebook, else our present labor would be light. He has even told us little of himself. We only know that on a gravelly beach of Lake Iroquois, now the Ridge road, he rudely gathered stones to make a hearth, and built a fire; and the next storm breakers, forcing back the beach, buried and thus preserved, to gratify yet whet our curiosity, hearth, ashes, and charred sticks.*

In these Darwinian days we can not deem primeval the man possessed of the Promethean art of fire, and so his presence on the scene adds zest to the pursuit of the Niagara problem. Whatever the antiquity of the great cataract may be found to be, the antiquity of man is greater.

* *American Anthropologist*, vol. II, pp. 173, 174.

THE MEDITERRANEAN, PHYSICAL AND HISTORICAL.*

By Sir R. LAMBERT PLAYFAIR.

When the unexpected honor was proposed to me of presiding over your deliberations, I felt some embarrassment as to the subject of my address. Geography as a science, and the necessity of encouraging a more systematic study of it, had been treated in an exhaustive manner during previous meetings. - - - In my perplexity I applied for the advice of one of the most experienced geographers of our Society, whose reply brought comfort to my mind. He reminded me that it was generally the custom for presidents of sections to select subjects with which they were best acquainted, and added: "What more instructive and captivating subject could be wished than the Mediterranean, physical and historical?"

For nearly a quarter of a century I have held an official position in Algeria, and it has been my constant delight to make myself acquainted with the islands and shores of the Mediterranean, in the hope of being able to facilitate the travels of my countrymen in that beautiful part of the world.

I can not pretend to throw much new light on the subject, and I have written so often about it already that what I have to say may strike you as a twice-told tale; nevertheless, if you will permit me to descend from the elevated platform occupied by more learned predecessors, I should like to speak to you in a familiar manner of this "great sea," as it is called in sacred Scripture, the *Mare internum* of the ancients, "our sea," *Mare nostrum* of Pomponius Mela.

Its shores include about 3,000,000 square miles of the richest country on the earth's surface, enjoying a climate where the extremes of temperature are unknown, and with every variety of scenery, but chiefly consisting of mountains and elevated plateaux. It is a well defined region of many parts, all intimately connected with each other by their geographical character, their geological formation, their flora, fauna, and the physiognomy of the people who inhabit them. To this general

* Vice-presidential address before the Geographical Section of the British Association Adv. Sci. meeting at Leeds, September, 1890. (From *Nature*, September 11, 1890, vol. XLII, pp. 480–485.)

statement there are two exceptions;—namely, Palestine, which belongs rather to the tropical countries lying to the east of it, and so may be dismissed from our subject; and the Sahara, which stretches to the south of the Atlantic region—or region of the Atlas—but approaches the sea at the Syrtis, and again to the eastward of the Cyrenaica, and in which Egypt is merely a long oasis on either side of the Nile.

The Mediterranean region is the emblem of fertility and the cradle of civilization, while the Sahara—Egypt, of course, excepted—is the traditional panther's skin of sand, dotted here and there with oases, but always representing sterility and barbarism. The sea is in no sense, save a political one, the limit between them; it is a mere gulf, which, now bridged by steam, rather unites than separates the two shores. Civilization never could have existed if this inland sea had not formed the junction between the three surrounding continents, rendering the coasts of each easily accessible, whilst modifying the climate of its shores.

The Atlas range is a mere continuation of the south of Europe. It is a long strip of mountain land, about 200 miles broad, covered with splendid forests, fertile valleys, and in some places arid steppes, stretching eastward from the ocean to which it has given its name. The highest point is Morocco, forming a pendant to the Sierra Nevada of Spain; thence it runs, gradually decreasing in height, through Algeria and Tunisia, it becomes interrupted in Tripoli, and it ends in the beautiful green hills of the Cyrenaica, which must not be confounded with the oases of the Sahara, but is an island detached from the eastern spurs of the Atlas, in the ocean of the desert.

In the eastern part the flora and fauna do not essentially differ from those of Italy; in the west they resemble those of Spain; one of the noblest of the Atlantic conifers, the *Abies pinsapo*, is found also in the Iberian peninsula and nowhere else in the world, and the valuable alfa grass or esparto (*Stipa tenacissima*), from which a great part of our paper is made, forms one of the principal articles of export from Spain, Portugal, Morocco, Algeria, Tunisia, and Tripoli. On both sides of the sea the former plant is found on the highest and most inaccessible mountains, amongst snows which last during the greater part of the year, and the latter from the sea level to an altitude of 5,000 feet, but in places where the heat and drought would kill any other plant, and in undulating land where water can not lodge.

Of the three thousand plants found in Algeria, by far the greater number are natives of southern Europe, and less than one hundred are peculiar to the Sahara. The *macchie* or maquis of Algeria in no way differs from that of Corsica, Sardinia, and other places; it consists of lentisk, arbutus, myrtle, cistus, tree-heath, and other Mediterranean shrubs. If we take the commonest plant found on the southern shores of the Mediterranean, the dwarf palm (*Chamærops humilis*), we see at once how intimately connected is the whole Mediterranean region, with

the exception of the localities I have before indicated This palm still grows spontaneously in the south of Spain, and in some parts of Provence, in Corsica, Sardinia, and the Tuscan Archipelago, in Calabria and the Ionian Islands, on the continent of Greece, and in several of the islands in the Levant, and it has only disappeared from other countries as the land has been brought under regular cultivation. On the other hand, it occurs neither in Palestine, Egypt, nor in the Sahara.

The presence of European birds may not prove much, but there are mammalia, reptiles, fish, and insects common to both sides of the Mediterranean. Some of the larger animals, such as the lion, panther, jackal, etc., have disappeared before the march of civilization in the one continent, but have lingered, owing to Mohammedan barbarism, in the other. There is abundant evidence of the former existence of these and of the other large mammals which now characterize tropical Africa in France, Germany, and Greece. It is probable that they only migrated to their present habitat after the upheaval of the great sea which, in Eocene times, stretched from the Atlantic to the Indian Ocean, making southern Africa an island continent like Australia. The original fauna of Africa, of which the lemur is the distinctive type, is still preserved in Madagascar, which then formed part of it.

The fish fauna is naturally the most conclusive evidence as to the true line of separation between Europe and Africa. We find the trout in the Atlantic region and in all the snow-fed rivers falling into the Mediterranean; in Spain, Italy, Dalmatia; it occurs in Mount Olympus, in rivers of Asia Minor, and even in the Lebanon, but nowhere in Palestine south of that range, in Egypt, or in the Sahara. This fresh-water salmonoid is not exactly the same in all these localities, but is subject to considerable variation, sometimes amounting to specific distinction. Nevertheless it is a European type found in the Atlas, and it is not till we advance into the Sahara, at Tuggurt, that we come to a purely African form in the Chromidæ, which have a wide geographical distribution, being found everywhere between that place, the Nile, and Mozambique.

The presence of newts, tailed batrachians, in every country around the Mediterranean, except again in Palestine, Egypt, and the Sahara, is another example of the continuity of the Mediterranean fauna, even though the species are not the same throughout.

The Sahara is an immense zone of desert which commences on the shores of the Atlantic Ocean, between the Canaries and Cape de Verde, and traverses the whole of north Africa, Arabia, and Persia, as far as Central Asia. The Mediterranean portion of it may be said roughly to extend between the fifteenth and thirtieth degrees of north latitude.

This was popularly supposed to have been a vast inland sea in very recent times, but the theory was supported by geological facts wrongly

interpreted. It has been abundantly proved by the researches of travellers and geologists that such a sea was neither the cause nor the origin of the Libyan Desert.

Rainless and sterile regions of this nature are not peculiar to north Africa, but occur in two belts which go round the world in either hemisphere at about similar distances north and south of the equator. These correspond in locality to the great inland drainage areas from which no water can be discharged into the ocean, and which occupy about one-fifth of the total land surface of the globe.

The African Sahara is by no means a uniform plain, but forms several distinct basins containing a considerable extent of what may almost be called mountain land. The Hoggar Mountains, in the center of the Sahara, are 7,000 feet high, and are covered during three months with snow. The general average may be taken at 1,500. The physical character of the region is very varied; in some places, such as at Tiout, Moghrar, Touat, and other oases in or bordering on Morocco, there are well watered valleys, with fine scenery and almost European vegetation, where the fruits of the north flourish side by side with the palm tree. In others there are rivers like the Oued Guir, an affluent of the Niger, which the French soldiers, who saw it in 1870, compare to the Loire. Again, as in the bed of the Oued Rir, there is a subterranean river, which gives a sufficient supply of water to make a chain of rich and well-peopled oases equal in fertility to some of the finest portions of Algeria. The greater part of the Sahara, however, is hard and undulating, cut up by dry water courses, such as the Igharghar, which descends to the Chott Melghigh, and almost entirely without animal or vegetable life.

About one-sixth of its extent consists of dunes of moving sand, a vast accumulation of detritus washed down from more northern and southern regions—perhaps during the glacial epoch—but with no indication of marine formation. These are difficult and even dangerous to traverse; but they are not entirely destitute of vegetation. Water is found at rare but well-known intervals, and there is an abundance of salsolaceous plants which serve as food for the camel. This sand is largely produced by wind action on the underlying rocks, and is not sterile in itself; it is only the want of water which makes it so. Wherever water does exist or artesian wells are sunk oases of great fertility never fail to follow.

Some parts of the Sahara are below the level of the sea, and here are formed what are called *chotts* or *sebkhas*, open depressions without outlets, inundated by torrents from the southern slopes of the Atlas in winter, and covered with a saline efflorescence in summer. This salt by no means proves the former existence of an inland sea; it is produced by the concentration of the natural salts, which exist in every variety of soil, washed down by winter rains, with which the unevaporated residue of water becomes saturated.

Sometimes the drainage, instead of flooding open spaces and forming chotts, finds its way through the permeable sand till it meets impermeable strata below it, thus forming vast subterranean reservoirs where the artesian sound daily works as great miracles as did Moses's rod of yore at Meribah. I have seen a column of water thrown up into the air equal to 1,300 cubic meters per diem, a quantity sufficient to redeem 1,800 acres of land from sterility and to irrigate 60,000 palm trees. This seems to be the true solution of the problem of an inland sea, a sea of verdure and fertility caused by the multiplication of artesian wells, which never fail to bring riches and prosperity in their train.

The climate of the Sahara is quite different from that of what I have called the Mediterranean region, where periodical rains divide the year into two seasons. Here, in many places, years elapse without a single shower; there is no refreshing dew at night, and the winds are robbed of their moisture by the immense continental extents over which they blow. There can be no doubt that it is to these meteorological and not to geological causes that the Sahara owes its existence. Reclus divides the Mediterranean into two basins, which, in memory of their history, he calls the Phœnician and the Carthaginian, or the Greek and Roman Seas, more generally known to us as the Eastern and Western Basins, separated by the island of Sicily.

If we examine the submarine map of the Mediterranean we see that it must at one time have consisted of two inclosed or inland basins, like the Dead Sea. The western one is separated from the Atlantic by the Straits of Gibraltar, a shallow ridge, the deepest part of which is at its eastern extremity, averaging about 300 fathoms, while on the west, bounded by a line from Cape Spartel to Trafalgar, it varies from 50 to 200 fathoms. Fifty miles to the west of the straits the bottom suddenly sinks down to the depths of the Atlantic, while to the east it descends to the general level of the Mediterranean, from 1,000 to 2,000 fathoms.

The Western is separated from the Eastern Basin by the isthmus which extends between Cape Bon, in Tunisia, and Sicily, known as the "Adventure Bank," on which there is not more than from 30 to 250 fathoms. The depth between Italy and Sicily is insignificant, and Malta is a continuation of the latter, being only separated from it by a shallow patch of from 50 to 100 fathoms, while to the east and west of this bank the depth of the sea is very great. These shallows cut off the two basins from all but superficial communication.

The configuration of the bottom shows that the whole of this strait was at one time continuous land, affording free communication for land animals between Africa and Europe. The palæontological evidence of this is quite conclusive. In the caves and fissures of Malta, amongst river detritus, are found three species of fossil elephants, a hippopotamus, a gigantic dormouse, and other animals which could never have lived in so small an island. In Sicily, remains of the existing elephant

have been found, as well as the *Elephas antiquus*, and two species of hippopotamus, while nearly all these and many other animals of African type have been found in the Pliocene deposits and caverns of the Atlantic region.

The rapidity with which such a transformation might have occurred can be judged by the well-known instance of Graham's Shoal, between Sicily and the island of Pantellaria; this, owing to volcanic agency, actually rose above the water in 1832, and for a few weeks had an area of 3,240 feet in circumference and a height of 107 feet.

The submersion of this isthmus no doubt occurred when the waters of the Atlantic were introduced through the Straits of Gibraltar. The rainfall over the entire area of the Mediterranean is certainly not more than 30 inches, while the evaporation is at least twice as great; therefore, were the straits to be once more closed and were there no other agency for making good this deficiency, the level of the Mediterranean would sink again till its basin became restricted to an area no larger than might be necessary to equalize the amount of evaporation and precipitation. Thus not only would the strait between Sicily and Africa be again laid dry, but the Adriatic and Ægean Seas also, and a great part of the Eastern Basin.

The entire area of the Mediterranean and Black Seas has been estimated at upwards of a million square miles, and the volume of the rivers which are discharged into them at 226 cubic miles. All this and much more is evaporated annually. There are two constant currents passing through the Straits of Gibraltar, super-imposed on each other; the upper and most copious one flows in from the Atlantic at a rate of nearly 3 miles an hour, or 140,000 cubic metres per second, and supplies the difference between the rainfall and evaporation, while the under current of warmer water, which has undergone concentration by evaporation, is continually flowing out at about half the above rate of movement, getting rid of the excess of salinity; even thus, however, leaving the Mediterranean salter than any other part of the ocean except the Red Sea.

A similar phenomenon occurs at the eastern end, where the fresher water of the Black Sea flows as a surface current through the Dardanelles, and the salter water of the Mediterranean pours in below it.

The general temperature of the Mediterranean from a depth of 50 fathoms down to the bottom is almost constantly 56° F., whatever may be its surface rise of temperature. This is a great contrast to that of the Atlantic, which at a similar depth is at least 3° colder, and which at 1,000 fathoms sinks to 40° F.

This fact was of the greatest utility to Dr. Carpenter in connection with his investigations regarding currents through the straits, enabling him to distinguish with precision between Atlantic and Mediterranean water.

For all practical purposes the Mediterranean may be accepted as being,

what it is popularly supposed to be, a tideless sea; but it is not so in reality. In many places there is a distinct rise and fall, though this is more frequently due to winds and currents than to lunar attraction. At Venice there is a rise of from 1 to 2 feet in spring tides, according to the prevalence of winds up or down the Adriatic; but in that sea itself the tides are so weak that they can hardly be recognized, except during the prevalence of the Bora, our old friend *Boreas*, which generally raises a surcharge along the coast of Italy. In many straits and narrow arms of the sea there is a periodical flux and reflux; but the only place where tidal influence, properly so called, is unmistakably observed is in the Lesser Syrtis, or Gulf of Gabes. There the tide runs at the rate of 2 or 3 knots an hour, and the rise and fall varies from 3 to 8 feet. It is most marked and regular at Djerba, the Homeric island of the Lotophagi. One must be careful in landing there in a boat, so as not to be left high and dry a mile or two from the shore. Perhaps the companions of Ulysses were caught by the receding tide, and it was not only a banquet of dates, the "honey-sweet fruit of the Lotus," or the potent wine which is made from it, which made them "forgetful of their homeward way."

The Gulf of Gabes naturally calls to mind the proposals which were made a few years ago for inundating the Sahara, and so restoring to the Atlantic region the insular condition which it is alleged to have had in pre-historic times. I will not allude to the English project for introducing the waters of the Atlantic from the west coast of Africa. That does not belong to my subject. The French scheme advocated by Commandant Roudaire, and supported by M. de Lesseps, was quite as visionary and impracticable.

To the south of Algeria and Tunis there exists a great depression, stretching westward from the Gulf of Gabes to a distance of about 235 miles, in which are several *chotts* or salt lakes, sometimes only marshes, and in many places covered with a saline crust strong enough to bear the passage of camels. Commandant Roudaire proposed to cut through the isthmuses which separated the various chotts, and so prepare their basins to receive the waters of the Mediterranean. This done, he intended to introduce the sea by a canal, which should have a depth of 1 metre below low-water level.

This scheme was based on the assumption that the basin of the chotts has been an inland sea within historic times; that, little by little, owing to the difference between the quantity of water which entered and the amount of evaporation and absorption, this interior sea had disappeared, leaving the chotts as an evidence of the former condition of things; that, in fact, this was none other than the celebrated Lake Triton, the position of which has always been a puzzle to geographers.

This theory however is untenable. The isthmus of Gabes is not a mere sand bank. There is a band of rock between the sea and the basin of the chotts, through which the former never could have penetrated in modern times. It is much more probable that Lake Triton was the

large bight between the island of Djerba and the mainland, on the shores of which are the ruins of the ancient city of Meninx, which, to judge by the abundance of Greek marble found there, must have carried on an important commerce with the Levant.

The scheme has now been entirely abandoned. Nothing but the mania for cutting through isthmuses all over the world which followed the brilliant success achieved at Suez can explain its having been started at all. Of course, no mere mechanical operation is impossible in these days; but the mind refuses to realize the possibility of vessels circulating in a region which produces nothing, or that so small a sheet of water in the immensity of the Sahara could have any appreciable effect in modifying the climate of its shores.

The eastern basin is much more indented and cut up into separate seas than the western one. It was therefore better adapted for the commencement of commerce and navigation. Its high mountains were landmarks for the unpracticed sailor, and its numerous islands and harbors afforded shelter for his frail bark, and so facilitated communication between one point and another.

The advance of civilization naturally took place along the axis of this sea, Phœnicia, Greece, and Italy being successively the great nurseries of human knowledge and progress. Phœnicia had the glory of opening out the path of ancient commerce, for its position in the Levant gave it a natural command of the Mediterranean, and its people sought the profits of trade from every nation which had a seaboard on the three continents washed by this sea. Phœnicia was already a nation before the Jews entered the Promised Land; and when they did so, they carried on inland traffic as middlemen to the Phœnicians. Many of the commercial centers on the shores of the Mediterranean were founded before Greece and Rome acquired importance in history. Homer refers to them as daring traders nearly a thousand years before the Christian era.

For many centuries the commerce of the world was limited to the Mediterranean, and when it extended in the direction of the East it was the merchants of the Adriatic, of Genoa, and of Pisa who brought the merchandise of India, at an enormous cost, to the Mediterranean by land, and who monopolized the carrying trade by sea. It was thus that the elephant trade of India, the caravan traffic through Babylon and Palmyra, as well as the Arab *kafilehs*, became united with the Occidental commerce of the Mediterranean.

As civilization and commerce extended westward, mariners began to overcome their dread of the vast solitudes of the ocean beyond the Pillars of Hercules, and the discovery of America by Columbus and the circum-navigation of Africa by the Portuguese changed entirely the current of trade as well as increased its magnitude, and so relegated the

Mediterranean, which had hitherto been the central sea of human intercourse, to a position of secondary importance.

Time will not permit me to enter into further details regarding the physical geography of this region, and its history is a subject so vast that a few episodes of it are all that I can possibly attempt. It is intimately connected with that of every other country in the world, and here were successively evolved all the great dramas of the past and some of the most important events of less distant date.

As I have already said, long before the rise of Greece and Rome its shores and islands were the seat of an advanced civilization. Phœnicia had sent out her pacific colonies to the remotest parts, and not insignificant vestiges of their handicraft still exist to excite our wonder and admiration. We have the megalithic temples of Malta, sacred to the worship of Baal, the generative god, and Ashtoreth, the conceptive goddess, of the universe. The three thousand *nurhagi* of Sardinia, round towers of admirable masonry, intended probably for defense in case of sudden attack, and the so-called giant graves, were as great a mystery to classical authors as they are to us at the present day. Minorca has its *talayots*, tumuli somewhat analogous to but of ruder construction than the *nurhagi*, more than 200 groups of which exist in various parts of the island. With these are associated subordinate constructions intended for worship, altars composed of two immense monoliths erected in the form of a T, sacred inclosures and megalithic habitations. One type of *talayot* is especially remarkable, of better masonry than the others, and exactly resembling inverted boats. One is tempted to believe that the Phœnicians had in view the grass habitations or *mapalia* of the Numidians described by Sallust, and had endeavored to reproduce them in stone: *Oblonga, incurvis lateribus tecta, quasi navium carinæ sunt.*

For a long time the Phœnicians had no rivals in navigation, but subsequently the Greeks—especially the Phocians—established colonies in the western Mediterranean, in Spain, Corsica, Sardinia, Malta, and the south of France, through the means of which they propagated not only their commerce but their arts, literature, and ideas. They introduced many valuable plants, such as the olive, thereby modifying profoundly the agriculture of the countries in which they settled. They have even left traces of their blood, and it is no doubt to this that the women of Provence owe the classical beauty of their features.

But they were eclipsed by their successors. The empire of Alexander opened out a road to India, in which, indeed, the Phœnicians had preceded him, and introduced the produce of the East into the Mediterranean; while the Tyrian colony of Carthage became the capital of another vast empire, which, from its situation midway between the Levant and the Atlantic Ocean, enabled it to command the Mediterranean traffic.

The Carthaginians at one time ruled over territory extending along

the coast from Cyrene to Numidia, besides having a considerable influence over the interior of the continent, so that the name of Africa, given to their own dominions, was gradually applied to a whole quarter of the globe. The ruling passion with the Carthaginians was love of gain, not patriotism, and their wars were largely fought with mercenaries. It was the excellence of her civil constitution which, according to Aristotle, kept in cohesion for centuries her straggling possessions. A country feebly patriotic, which intrusts her defense to foreigners, has the seeds of inevitable decay, which ripened in her struggle with Rome, despite the warlike genius of Hamilcar and the devotion of the magnanimous Hannibal. The gloomy and cruel religion of Carthage, with its human sacrifices to Moloch and its worship of Baal under the name of Melkarth, led to a criminal code of Draconic severity and alienated it from surrounding nations. When the struggle with Rome began, Carthage had no friends. The first Punic war was a contest for the possession of Sicily, whose prosperity is even now attested by the splendor of its Hellenic monuments. When Sicily was lost by the Carthaginians, so also was the dominion of the sea, which hitherto had been uncontested. The second Punic war resulted in the utter prostration of Carthage and the loss of all her possessions out of Africa, and in 201 B. C., when this war was ended, 552 years after the foundation of the city, Rome was mistress of the world.

The destruction of Carthage after the third Punic war was a heavy blow to Mediterranean commerce. It was easy for Cato to utter his stern *Delenda est Carthago.* Destruction is easy, but construction is vastly more difficult. Although Augustus in his might built a new Carthage near the site of the old city, he could never attract again the trade of the Mediterranean, which had been diverted into other channels. Roman supremacy was unfavorable to the growth of commerce, because, though she allowed unrestricted trade throughout her vast empire and greatly improved internal communications in the subjugated countries, Rome itself absorbed the greater part of the wealth and did not produce any commodities in return for its immense consumption, therefore Mediterranean commerce did not thrive under the Roman rule. The conquest of Carthage, Greece, Egypt, and the East poured in riches to Rome, and dispensed for a time with the needs of productive industry, but formed no enduring basis of prosperity.

It is only in relation to the Mediterranean that I can refer to Roman history; but I must allude to the interesting episode in the life of Diocletian, who, after an anxious reign of 21 years in the eastern division of the empire, abdicated at Nicomedia, and retired to his native province of Illyria. He spent the rest of his life in rural pleasures and horticulture at Salona, near which he built that splendid palace within the walls of which subsequently arose the modern city of Spalato. Nothing more interesting exists on the shores of the Mediterranean than this extraordinary edifice, perhaps the largest that

ever arose at the bidding of a single man; not only vast and beautiful, but marking one of the most important epochs in the history of architecture.

Though now obstructed with a mass of narrow, tortuous streets, its salient features are distinctly visible. The great temple, probably the mausoleum of the founder, has become the cathedral, and after the Pantheon at Rome there is no finer specimen of a heathen temple turned into a Christian church. Strange it is that the tomb of him whose reign was marked by such unrelenting persecution of the Christians should have been accepted as the model of those baptisteries so commonly constructed in the following centuries.

Of Diocletian's Salona, one of the chief cities of the Roman world, but little now remains save traces of the long, irregular walls. Recent excavations have brought to light much that is interesting, but all of the Christian epoch, such as a large basilica which had been used as a necropolis, and a baptistery, one of those copied from the temple of Spalato, on the mosaic pavement of which can still be read the text, *Sicut cervus desiderat fontem aquarum ita anima mea ad te Deus.*

The final partition of the Roman Empire took place in 365; 40 years later the barbarians of the North began to invade Italy and the south of Europe; and in 429, Genseric, at the head of his Vandal hordes, crossed over into Africa from Andalusia, a province which still bears their name, devastating the country as far as the Cyrenaica. He subsequently annexed the Balearic Islands, Corsica, and Sardinia; he ravaged the coasts of Italy and Sicily, and even of Greece and Illyria; but the most memorable of his exploits was the unresisted sack of Rome, whence he returned to Africa laden with treasure and bearing the Empress Eudoxia a captive in his train.

The degenerate emperors of the West were powerless to avenge this insult; but Byzantium, though at this time sinking to decay, did make a futile attempt to attack the Vandal monarch in his African stronghold. It was not, however, till 533, in the reign of Justinian, when the successors of Genseric had fallen into luxurious habits and had lost the rough valor of their ancestors, that Belisarius was able to break their power and take their last king a prisoner to Constantinople. The Vandal domination in Africa was destroyed, but that of the Byzantines was never thoroughly consolidated; it rested not on its own strength, but on the weakness of its enemies; and it was quite unable to cope with the next great wave of invasion which swept over the land, perhaps the most extraordinary event in the world's history, save only the introduction of Christianity.

In 647, 27 years after the Hedjira of Mohammed, Abdulla ibn Saad started from Egypt for the conquest of Africa with an army of 40,000 men.

The expedition had two determining causes—the hope of plunder and the desire to promulgate the religion of El Islam. The sands and

scorching heat of the desert, which had nearly proved fatal to the army of Cato, were no bar to the hardy Arabians and their enduring camels. The march to Tripoli was a fatiguing one, but it was successfully accomplished; the invaders did not exhaust their force in a vain effort to reduce its fortifications, but swept on over the Syrtic desert and north to the province of Africa, where, near the splendid city of Suffetula, a great battle was fought between them and the army of the Exarch Gregorius, in which the Christians were signally defeated, their leader killed, and his daughter allotted to Ibn-ez-Zobair, who had slain her father.

Not only did the victorious Moslems overrun north Africa, but soon they had powerful fleets at sea, which dominated the entire Mediterranean, and the emperors of the East had enough to do to protect their own capital.

Egypt, Syria, Spain, Provence, and the islands of the Mediterranean successively fell to their arms, and until they were checked at the Pyrenees by Charles Martel it seemed at one time as if the whole of southern Europe would have been compelled to submit to the disciples of the new religion. Violent, implacable, and irresistible at the moment of conquest, the Arabs were not unjust or hard masters in countries which submitted to their conditions. Every endeavor was, of course, made to proselytize, but Christians were allowed to preserve their religion on payment of a tax, and even Popes were in the habit of entering into friendly relations with the invaders. The Church of St. Cyprian and St. Augustine, with its 500 sees, was indeed expunged, but five centuries after the passage of the Mohammedan army from Egypt to the Atlantic a remnant of it still existed. It was not till the twelfth century that the religion and language of Rome became utterly extinguished.

The Arabs introduced a high state of civilization into the countries where they settled; their architecture is the wonder and admiration of the world at the present day; their irrigational works in Spain have never been improved upon; they fostered literature and the arts of peace, and introduced a system of agriculture far superior to what existed before their arrival.

Commerce, discouraged by the Romans, was highly honored by the Arabs, and during their rule the Mediterranean recovered the trade which it possessed in the time of the Phœnicians and Carthaginians; it penetrated into the Indian Archipelago and China; it travelled westward to the Niger, and to the east as far as Madagascar, and the great trade route of the Mediterranean was once more developed.

The power and prosperity of the Arabs culminated in the ninth century, when Sicily fell to their arms; it was not, however, very long before their empire began to be undermined by dissensions; the temporal and spiritual authority of the Ommiade Khalifs, which extended from Sind to Spain and from the Oxus to Yemen, was overthrown by the Abba-

sides in the year 132 of the Hedjira, A. D. 750. Seven years later Spain detached itself from the Abbaside empire; a new caliphate was established at Cordova, and hereditary monarchies began to spring up in other Mohammedan countries.

The Carlovingian empire gave an impulse to the maritime power of the south of Europe, and in the Adriatic the fleets of Venice and Ragusa monopolized the traffic of the Levant. The merchants of the latter noble little republic penetrated even to our own shores, and Shakespeare has made the Argosy or Ragusie a household word in our language.

During the eleventh century the Christian powers were no longer content to resist the Mohammedans; they began to turn their arms against them. If the latter ravaged some of the fairest parts of Europe, the Christians began to take brilliant revenge.

The Mohammedans were driven out of Corsica, Sardinia, Sicily, and the Balearic Islands, but it was not till 1492 that they had finally to abandon Europe, after the conquest of Granada by Ferdinand and Isabella.

About the middle of the eleventh century an event took place which profoundly modified the condition of the Mohammedan world. The Caliph Mostansir let loose a horde of nomad Arabs, who, starting from Egypt, spread over the whole of north Africa, carrying destruction and blood wherever they passed, thus laying the foundation for the subsequent state of anarchy which rendered possible the interference of the Turks.

English commercial intercourse with the Mediterranean was not unknown even from the time of the Crusades, but it does not appear to have been carried on by means of our own vessels till the beginning of the sixteenth century. In 1522 it was so great that Henry VIII appointed a Cretan merchant, Censio de Balthazari, to be "master, governor, protector, and consul of all and singlar the merchants and others, his lieges and subjects, within the port, island, and country of Crete or Candia." This is the very first English consul known to history, but the first of English birth was my own predecessor in office, Master John Tipton, who, after having acted at Algiers during several years in an unofficial character, probably elected by the merchants themselves to protect their interests, was duly appointed consul by Sir William Harebone, ambassador at Constantinople, in 1585, and received just such an exequatur from the Porte as has been issued to every consul since by the Government of the country in which he resides.

Piracy has always been the scourge of the Mediterranean, but we are too apt to associate its horrors entirely with the Moors and Turks. The evil had existed from the earliest ages; even before the Roman conquest of Dalmatia the Illyrians were the general enemies of the Adriatic. Africa, under the Vandal reign, was a nest of the fiercest pirates. The Venetian chronicles are full of complaints of the ravages

of the Corsairs of Ancona, and there is no other name but piracy for such acts of the Genoese as the unprovoked pillage of Tripoli by Andrea Doria in 1535. To form a just idea of the Corsairs of the past, it is well to remember that commerce and piracy were often synonymous terms, even among the English, up to the reign of Elizabeth. Listen to the description given by the pious Cavendish of his commercial circumnavigation of the globe: "It has pleased Almighty God to suffer me to circumpass the whole globe of the world. - - - I navigated along the coast of Chile, Peru, and New Spain, where I made great spoils. All the villages and towns that ever I landed at, I burned and spoiled, and had I not been discovered upon the coast, I had taken a great quantity of treasure," and so he concludes, "The Lord be praised for all his mercies!"

Sir William Monson, when called upon by James I to propose a scheme for an attack on Algiers, recommended that all the maritime powers of Europe should contribute towards the expense and participate in the gains by the sale of Moors and Turks as slaves.

After the discovery of America and the expulsion of the Moors from Spain, piracy developed to an extraordinary extent. The audacity of the Barbary Corsairs seems incredible at the present day; they landed on the shores and islands of the Mediterranean, and even extended their ravages to Great Britain, carrying off all the inhabitants whom they could seize into the most wretched slavery. The most formidable of these piratical states was Algiers, a military oligarchy, consisting of a body of janissaries, recruited by adventurers from the Levent, the outcasts of the Mohammedan world, criminals and renegades from every nation in Europe. They elected their own ruler or Dey, who exercised despotic sway, tempered by frequent assassination; they oppressed without mercy the natives of the country, accumulated vast riches, had immense numbers of Christian slaves, and kept all Europe in a state bordering on subjection by the terror which they inspired. Nothing is sadder or more inexplicable than the shameful manner in which this state of things was accepted by civilized nations. Many futile attempts were made during successive centuries to humble their arrogance, but it only increased by every manifestation of the powerlessness of Europe to restrain it. It was reserved for our own countryman, Lord Exmouth, by his brilliant victory in 1816, forever to put an end to piracy and Christian slavery in the Mediterranean. His work, however, was left incomplete, for though he destroyed the navy of the Algerines and so rendered them powerless for evil on the seas, they were far from being humbled; they continued to slight their treaties and to subject even the agents of powerful nations to contumely and injustice. The French took the only means possible to destroy this nest of ruffians by the almost unresisted occupation of Algiers and the deportation of its Turkish aristocracy.

They found the whole country in the possession of a hostile people,

some of whom had never been subdued since the fall of the Roman Empire, and the world owes France no small debt of gratitude for having transformed what was a savage and almost uncultivated country into one of the richest as well as the most beautiful in the basin of the Mediterranean.

What has been accomplished in Algeria is being effected in Tunisia. The treaty of the Kasr-es-Saeed, which established a French protectorate there and military occupation of the regency, were about as high-handed and unjustifiable acts as are recorded in history; but there can be no possible doubt regarding the important work of civilization and improvement that has resulted from them. European courts of justice have been established all over the country, the exports and imports have increased from twenty-three to fifty-one millions of francs, the revenue from six to nineteen millions, without the imposition of a single new tax, and nearly half a million per annum is being spent on education.

Sooner or later the same thing must happen in the rest of north Africa, though at present international jealousies retard this desirable consummation. It seems hard to condemn such fair countries to continued barbarism in the interest of tyrants who mis-govern and oppress their people. The day can not be far off when the whole southern shores of the Mediterranean will enjoy the same prosperity and civilization as the northern coast, and when the deserts which are the result of mis-government and neglect will assume the fertility arising from security and industry, and will again blossom as the rose.

It cannot be said that any part of the Mediterranean basin is still unknown, if we except the Empire of Morocco. But even that country has been traversed in almost every direction during the past 20 years, and its geography and natural history have been illustrated by men of the greatest eminence, such as Gerhard Rohlfs, Monsieur Tissot, Sir Joseph Hooker, the Vicomte de Foucauld, Joseph Thomson, and numerous other travellers. The least known portion, at least on the Mediterranean coast, is the Riff country, the inhospitality of whose inhabitants has given the word "ruffian" to the English language. Even that has been penetrated by De Foucauld disguised as a Jew, and the record of his exploration is one of the most brilliant contributions to the geography of the country which has hitherto been made.

Although, therefore, but little remains to be done in the way of actual exploration, there are many by-ways of travel comparatively little known to that class of the community with which I have so much sympathy,—the ordinary British tourist. These flock every year in hundreds to Algeria and Tunis, but few of them visit the splendid Roman remains in the interior of those countries. The Cyrenaica is not so easily accessible, and I doubt whether any Englishmen have travelled in it since the exploration of Smith and Porcher in 1861.

Cyrene almost rivalled Carthage in commercial importance. The Hellenic ruins still existing bear witness to the splendor of its five great cities. It was the birth-place of many distinguished people, and amongst its hills and fountains were located some of the most interesting scenes in mythology, such as the Gardens of the Hesperides, and the "silent, dull, forgetful waters of Lethe."

This peninsula is only separated by a narrow strait from Greece, whence it was originally colonized. There, and indeed all over the eastern basin of the Mediterranean, are many little-trodden routes, but the subject is too extensive; I am reluctantly compelled to restrict my remarks to the western half.

The south of Italy is more frequently traversed, and less travelled in, than any part of that country. Of the thousands who yearly embark or dis-embark at Brindisi few ever visit the land of Manfred. Otranto is only known to them from the fanciful descriptions in Horace Walpole's romance. The general public in this country is quite ignorant of what is going on at Taranto, and of the great arsenal and dockyard which Italy is constructing in the Mare Piccolo, an inland sea containing more than 1,000 acres of anchorage for the largest ironclads afloat, yet with an entrance so narrow that it is spanned by a revolving bridge. Even the Adriatic, though traversed daily by steamers of the Austrian Lloyd's Company, is not a highway of travel, yet where is it possible to find so many places of interest within the short space of a week's voyage, between Corfu and Trieste, as along the Dalmatian and Istrian shores, and among the islands that fringe the former where it is difficult to realize that one is at sea at all, and not on some great inland lake?

There is the Bocche di Cattaro, a vast rent made by the Adriatic among the mountains, where the sea flows round their spurs in a series of canals, bays, and lakes of surpassing beauty. The city of Cattaro itself, the gateway of Montenegro, with its picturesque Venetian fortress, nestling at the foot of the black mountain, Ragusa, the Roman successor of the Hellenic Epidaurus, queen of the southern Adriatic, battling with the waves on her rock-bound peninsula, the one spot in all that sea which never submitted either to Venice or the Turk, and for centuries resisting the barbarians on every side, absolutely unique as a mediæval fortified town, and worthy to have given her name to the argosies she sent forth; Spalato, the grandest of Roman monuments; Lissa, colonized by Dionysius of Syracuse, and memorable to us as having been a British naval station from 1812 to 1814, while the French held Dalmatia; Zara, the capital, famous for its siege by the Crusaders, interesting from an ecclesiological point of view, and venerated as the last resting place of St. Simeon, the prophet of the *Nunc dimittis*; Parenza, with its great basilica; Pola, with its noble harbor, whence Belisarius sailed forth, now the chief naval port of the Austrian Empire, with its Roman amphitheater and graceful triumphal arches, be-

sides many other places of almost equal interest. Still farther west are Corsica, Sardinia, and the Balearic Islands, all easily accessible from the coasts of France, Italy, and Spain. Their ports are constantly visited by mail steamers and private yachts, yet they are but little explored in the interior. - - -

I have endeavored to sketch, necessarily in a very imperfect manner, the physical character and history of the Mediterranean, to show how the commerce of the world originated in a small maritime state at its eastern extremity; how it gradually advanced westward till it burst through the Straits of Gibraltar and extended over seas and continents until then undreampt of, an event which deprived the Mediterranean of that commercial prosperity and greatness which for centuries had been limited to its narrow basin.

Once more this historic sea has become the highway of nations; the persistent energy and genius of two men have revolutionized navigation, opened out new and boundless fields for commerce, and it is hardly too much to say that if the Mediterranean is to be restored to its old position of importance, if the struggle for Africa is to result in its regeneration, as happened in the New World, if the dark places still remaining in the farther East are to be civilized, it will be in a great measure due to Waghorn and Ferdinand de Lesseps, who developed the overland route and created the Suez Canal.

But the Mediterranean can only hope to retain its regenerated position in time of peace. Nothing is more certainly shown by past history than that war and conquest have changed the route of commerce in spite of favored geographical positions. Babylon was conquered by Assyrians, Persians, Macedonians, and Romans, and though for a time her position on the Euphrates caused her to rise like a Phœnix from her ashes, successive conquests combined with the luxury and effeminacy of her rulers, caused her to perish. Tyre, conquered by Nebuchadnezzar and Alexander, fell as completely as Babylon had done, and her trade passed to Alexandria. Ruined sites of commercial cities rarely again become emporia of commerce; Alexandria is an exception dependent on very exceptional circumstances.

The old route to the East was principally used by sailing vessels, and was abandoned for the shorter and more economical one by the Suez Canal, which now enables a round voyage to be made in 60 days, which formerly required from 6 to 8 months. This, however, can only remain open in time of peace. It is quite possible that in the event of war the old route by the Cape may be again used to the detriment of traffic by the Mediterranean. Modern invention has greatly economized the use of coal, and steamers, by the use of duplex and triplex engines, can run with a comparatively small consumption of fuel, thus leaving a larger space for cargo. England, the great carrying power of the world, may find it more advantageous to trust to her own strength and the secur-

ity of the open seas than to run the gauntlet of the numerous strategical positions of the Mediterranean, such as Port Mahon, Bizerta, and Taranto, each of which is capable of affording impregnable shelter to a hostile fleet, and though the ultimate key to the Indian Ocean is in our own hands, our passage to it may be beset by a thousand dangers. There is no act of my career on which I look back with so much satisfaction as on the share I had in the occupation of Perim, one of the most important links in that chain of coaling stations which extends through the Mediterranean to the farther East, and which is so necessary for the maintenance of our naval supremacy. It is a mere islet, it is true, a barren rock, but one surrounding a noble harbor, and so eminently in its right place that we can not contemplate with equanimity the possibility of its being in any other hands than our own.

It is by no means certain whether exaggerated armaments are best suited for preserving peace or hastening a destructive war; the golden age of disarmament and international arbitration may not be near at hand, but it is even now talked of as a possibility.

Should the poet's prophecy or the patriot's dream be realized and a universal peace indeed bless the world, then this sea of so many victories may long remain the harvest field of a commerce nobler than conquest.

STANLEY AND THE MAP OF AFRICA.*

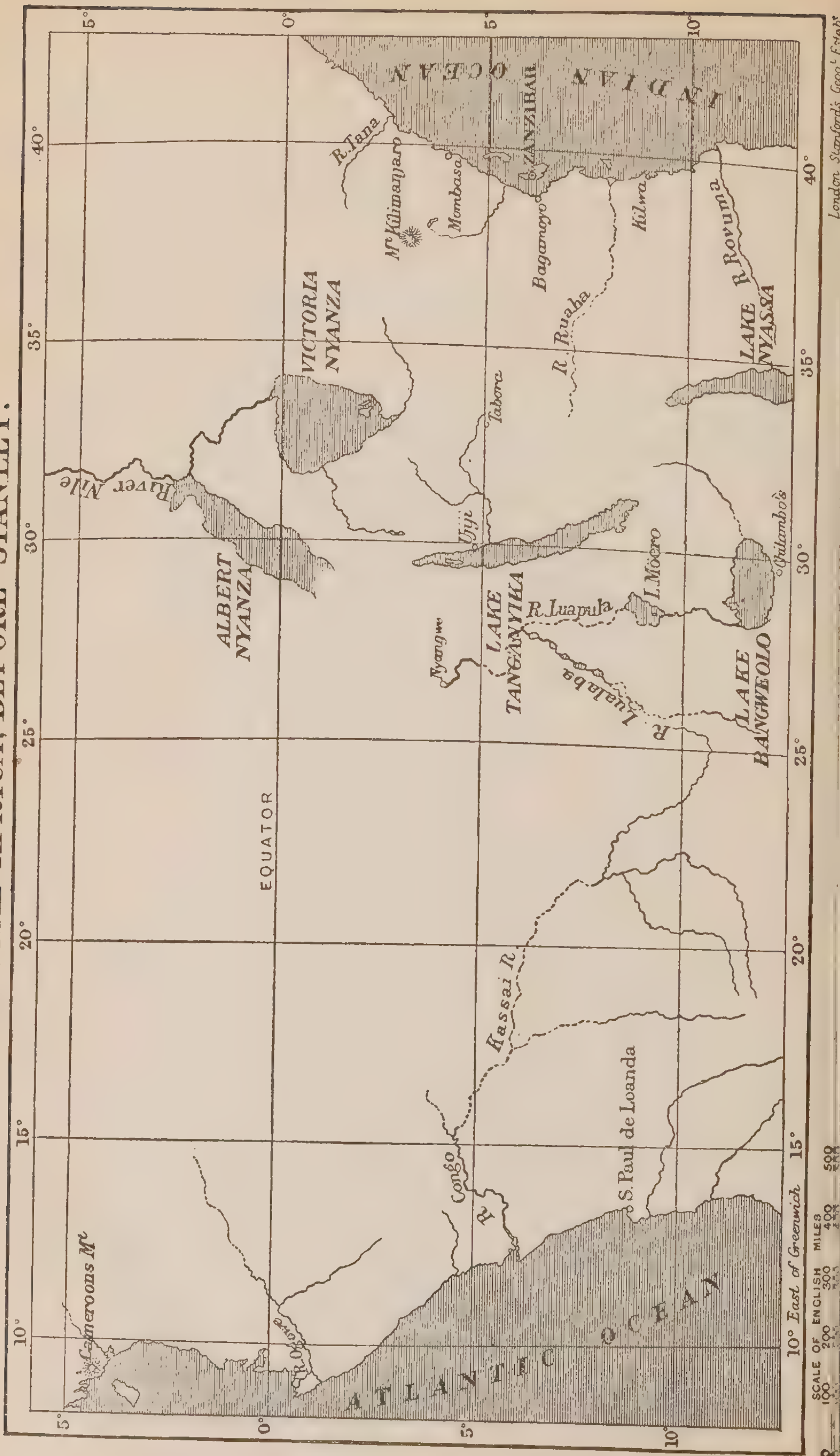
By J. Scott Keltie.

It is 19 years since Stanley first crossed the threshold of central Africa. He entered it as a newspaper correspondent to find and succor Livingstone, and came out burning with the fever of African exploration. While with Livingstone at Ujiji, he tried his 'prentice hand at a little exploring work, and between them they did something to settle the geography of the north end of Lake Tanganyika. Some three years and a half later he was once more on his way to Zanzibar, this time with the deliberate intention of doing something to fill up the great blank that still occupied the center of the continent. A glance at the first of the maps which accompany this paper will afford some idea of what Central Africa was like when Stanley entered it a second time. The ultimate sources of the Nile had yet to be settled. The contour and extent of Victoria Nyanza were of the most uncertain character. Indeed, so little was known of it beyond what Speke told us, that there was some danger of its being swept off the map altogether, not a few geographers believing it to be not one lake, but several. There was much to do in the region lying to the west of the lake, even though it had been traversed by Speke and Grant. Between a line drawn from the north end of Lake Tanganyika to some distance beyond the Albert Nyanza on one side, and the west coast region on the other, the map was almost white, with here and there the conjectural course of a river or two. Livingstone's latest work, it should be remembered, was then almost unknown, and Cameron had not yet returned. Beyond the Yellala Rapids there was no Congo, and Livingstone believed that the Lualaba swept northwards to the Nile. He had often gazed longingly at the broad river during his weary sojourn at Nyangwé, and yearned to follow it, but felt himself too old and exhausted for the task. Stanley was fired with the same ambition as his dead master, and was young and vigorous enough to indulge it.

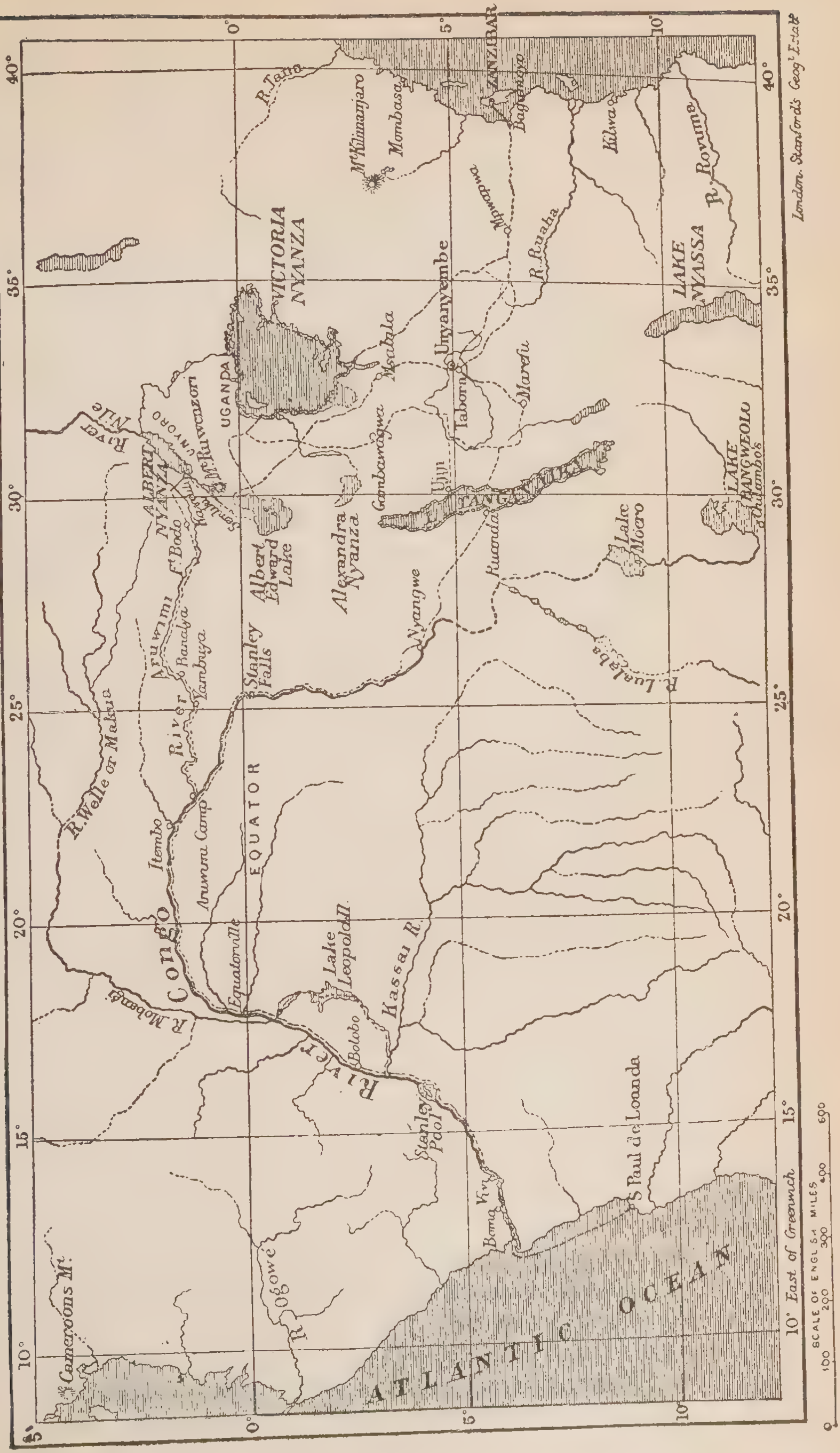
What, then, did Stanley do to map out the features of this great blank during the 2 years and 9 months which he spent in crossing from Bagamoyo to Boma, at the mouth of the Congo? He determined, with an accuracy which has since necessitated but slight modification,

* From *The Contemporary Review*, January, 1890, vol. LVII, pp. 126–140.

CENTRAL AFRICA, BEFORE STANLEY.



CENTRAL AFRICA, AFTER STANLEY.



the outline of the Victoria Nyanza; he found it to be one of the great lakes of the world, 21,500 square miles in extent, with an altitude of over 4,000 feet and border soundings of from 330 to 580 feet. Into the south shore of the lake a river flowed, which he traced for some 300 miles, and which he set down as the most southerly feeder of the Nile. With his stay at the court of the clever and cunning Mtesa of Uganda we need not concern ourselves; it has had momentous results. Westwards he came upon what he coneeived to be a part of the Albert Nyanza, which he named Beatrice Gulf, but of which more anon. Coming southwards to Ujiji, Stanley filled in many features in the region he traversed, and saw at a distance a great mountain, which he named Gordon Bennett, of which also more anon. A little lake to the south he named the Alexandra Nyanza; thence he conjectured issued the southwest source of the Nile, but on this point, within the last few months, he has seen cause to change his mind. Lake Tanganyika he circumnavigated, and gave greater accuracy to its outline; while through the Lukuga he found it sent its waters by the Lualaba to the Atlantic. Crossing to Nyangwé, where with longing eyes Livingstone beheld the mile-wide Lualaba flowing "north, north, north," Stanley saw his opportunity, and embraced it. Tippu Tip failed him then, as he did later; but the mystery of that great river he had made up his mind to solve, and solve it he did. The epic of that first recorded journey of a white man down this majestic river, which for ages had been sweeping its unknown way through the center of Africa, he and his dusky companions running the gauntlet through a thousand miles of hostile savages, is one of the most memorable things in the literature of travel. Leaving Nyangwé on November 5, 1876, in 9 months he traced the many-islanded Congo to the Atlantic, and placed on the map of Africa one of its most striking features; for the Congo ranks among the greatest rivers of the world. From the remote Chambeze, that enters Lake Bangweolo to the sea, it is 3,000 miles. It has many tributaries, themselves affording hundreds of miles of navigable drains, waters a basin of a million square miles, and pours into the Atlantic a volume estimated at 1,800,000 cubic feet per second. Thus, then, were the first broad lines drawn towards filling up the great blank. But, as we know, Stanley two years later was once more on his way to the Congo, and shortly after, within the compass of its great basin, he helped to found the Congo Free State. During the years he was officially connected with the river, either directly or through those who served under him, he went on filling up the blank by the exploration of other rivers, north and south, which poured their voluminous tribute into the main stream; and the impulse he gave has continued. The blank has become a network of dark lines, the interspaces covered with the names of tribes and rivers and lakes.

Such, then, briefly, is what Stanley did for the map of Africa during his great and ever-memorable journey across the continent. Once more

Mr. Stanley has crossed the continent, in the opposite direction, and taken just about the same time in which to do so. Discovery was not his main object this time, and therefore the results in this direction have not been so plentiful. Indeed, they could not be; he had left so comparatively little to be done. But the additions that he has made to our knowledge of the great blank are considerable, and of high importance in their bearing on the hydrography, the physical geography, the climate, and the people of central Africa.

Let us rapidly run over the incidents of this, in some respects, the most remarkable expedition that ever entered Africa. Its first purpose, as we know, was to relieve, and if necessary bring away, Emin Pasha, the governor of the abandoned equatorial province of the Egyptian Sudan, which spread on each side of the Bahr-el-Jebel, the branch of the Nile that issues from the Albert Nyanza. Here it was supposed that he and his Egyptian officers and troops, and their wives and children, were beleagured by the Madhist hordes, and that they were at the end of their supplies. Emin Pasha, who as Eduard Schnitzer was born in Prussian Silesia, and educated at Breslau and Berlin as a physician, spent 12 years (1864–1876) in the Turkish service, during which he traveled over much of the Asiatic dominions of Turkey, indulging his strong tastes for natural history. In 1876 he entered the service of Egypt, and was sent up to the Sudan as surgeon on the staff of Gordon Pasha, who at that time governed the equatorial province. In 1878, two years after Gordon had been appointed governor-general of the whole Sudan, Emin Effendi (he had Moslemized himself) was appointed governor of the equatorial province, which he found completely disorganized and demoralized, the happy hunting-ground of the slave-raider. Within a few months Emin had restored order, swept out the slavers, got rid of the Egyptian scum who pretended to be soldiers, improved the revenue, so that instead of a large deficit there was a considerable surplus, and established industry and legitimate trade. Meantime the Mahdi had appeared, and the movement of conquest was gathering strength. It was not, however, till 1884 that Emin began to fear danger. It was in January of that year that Gordon went out to hold Khartoum; just a year later both he and the city fell before the Madhist host. Emin withdrew with his officers and dependents, numbering about 1,500, to Wadelai, in the south of the province, within easy reach of Albert Nyanza.

Rumors of the events in the Sudan after the fall of Khartoum reached this country, but no one outside of scientific circles seemed to take much interest in Emin till 1886. Rapidly, however, Europe became aware what a noble stand this simple *savant*, who had been foisted into the position of governor of a half-savage province, was making against the forces of the Mahdi, and how he refused to desert his post and his people. Towards the autumn of 1886 public feeling on the subject rose to such a height that the British Government, which was held to blame

for the position in the Sudan, was compelled to take action. Our representative at Zanzibar, as early as August of that year, instituted inquiries as to the possibility of a relief expedition, but in the end, in dread of international complications, it was decided that a government expedition was impracticable. In this dilemma, Sir (then Mr.) William Mackinnon, chairman of the British India Steam Navigation Company, whose connection with east Africa is of old standing, came forward and offered to undertake the responsibility of getting up an expedition. The Emin Pasha relief committee was formed in December, 1886, and Government did all it could to aid, short of taking the actual responsibility. Mr. H. M. Stanley generously offered his services as leader, without fee or reward, giving up many lucrative engagements for the purpose. No time was lost. The sum of £20,000 had been subscribed, including £10,000 from the Egyptian Government. Mr. Stanley returned from America to England in the end of December; by the end of January he had made all his preparations, selecting 9 men as his staff, including 3 English officers and 2 surgeons, and was on his way to Zanzibar, which was reached on February 21. On the 25th the expedition was on board the *Madura*, bound for the mouth of the Congo, by way of the Cape; 9 European officers, 61 Sudanese, 13 Somalis, 3 interpreters, 620 Zanzibaris, the famous Arab slaver and merchant, Tippu Tip, and 407 of his people. The mouth of the Congo was reached on March 18; there the expedition was transshipped into small vessels and landed at Matadi, the limit of navigation on the lower river. From Matadi there was a march of 200 miles, past the cataracts, to Stanley Pool, where the navigation was resumed. The troubles of the expedition began on the Congo itself. The question of routes was much discussed at the time of organizing the expedition, the two that found most favor being that from the east coast through Masai land and round by the north of Uganda, and that by the Congo. Into the comparative merits of these two routes we shall not enter here. For reasons which were satisfactory to himself—and no one knows Africa better—Mr. Stanley selected the Congo route, though had he foreseen all that he and his men would have to undergo he might have hesitated. As it was, the expedition, which it was thought would be back in England by Christmas, 1887, only reached the coast in November, 1889. But the difficulties no one could have foreseen, the region traversed being completely unknown, and the obstacles encountered unprecedented even in Africa. Nor when the goal was reached was it expected that months would be wasted in persuading Emin and his people to quit their exile. Not the keenest-eyed of African explorers could have foreseen all this.

Want of sufficient boat accommodation and a scarcity of food almost amounting to famine hampered the expedition terribly on its way up the Congo. The mouth of the Aruwimi, the real starting point of the expedition, some 1,500 miles from the mouth of the Congo, was not

reached by Mr. Stanley and the first contingent, till the beginning of June, 1887. The distance from here in a straight line to the nearest point of the Albert Nyanza is about 450 miles; thence it was believed communication with Emin would be easy, for he had two steamers available. But it was possible that a detour would have to be made towards the north so as to reach Wadelia direct, for no one knew the conditions which prevailed in the country between the Aruwimi mouth and the Albert Nyanza. As it was Mr. Stanley took the course to the lake direct, but with many a circuit and many an obstruction and at a terrible sacrifice of life. An intrenched camp was established on a bluff at Yambuya, about 50 miles up the left bank of the Aruwimi. Major Barttelot was left in charge of this, and with him Dr. Bonny, Mr. Jameson, Mr. Rose Troup, Mr. Ward, and 257 men; the rear column was to follow as soon as Tippu Tip provided the contingent of 500 natives which he had solemnly promised. Although the whole of the men had not come up, yet everything seemed in satisfactory order; explicit instructions were issued to the officers of the rear column, and on June 28, 1887, Mr. Stanley, with a contingent consisting of 389 officers and men, set out to reach Emin Pasha. The officers with him were Captain Nelson, Lieutenant Stairs, Dr. Parke, and Mr. Jephson.

Five miles after leaving camp the difficulties began. The expedition was face to face with a dense forest of immense extent, choked with bushy undergrowth and obstructed by a network of creepers through which a way had often to be cleaved with the axes. Hostile natives harassed them day after day; the paths were studded with concealed spikes of wood; the arrows were poisoned; the natives burned their villages rather than have dealings with the intruders. Happily the river when it was again struck afforded relief, and the steel boat proved of service, though the weakened men found the portages past the cataracts a great trial. It was fondly hoped that here at least the Arab slaver had not penetrated; but on September 16, 200 miles from Yambuya, making 340 miles of actual travel, the slave camp of Ugarowwa was reached, and here the treatment was even worse than when fighting the savages of the forest. The brutalities practiced on Stanley's men cost many of them their lives. A month later the camp of another Arab slaver was reached, Kilinga Longa, and there the treatment was no better. These so-called Arabs, whose caravans consist mainly of the merciless Manyuema, from the country between Tanganyika and Nyangwé, had laid waste a great area of the region to be traversed by the expedition, so that between August 31 and November 12 every man was famished; and when at last the land of devastation was left behind, and the native village of Ibwiri entered, officers and men were reduced to skeletons. Out of the 389 who started only 174 entered Ibwiri, the rest dead, or missing, or left behind, unable to move, at Ugarowwa's. So weak was everybody that 70 tons of goods

and the boat had to be left at Kilinga Longa's with Captain Nelson and Surgeon Parke.

A halt of 13 days at Ibwiri, with its plenty of fowls, bananas, corn, yams, beans, restored everybody; and 173 sleek and robust men set out for the Albert Nyanza on November 24. A week later the gloomy and dreaded forest suddenly ended; the open country was reached; the light of day was unobstructed; it was an emergence from darkness to light. But the difficulties were not over; some little fighting with the natives on the populous plateau was necessary before the lake could be reached. On the 12th, the edge of the long slope from the Congo to Lake Albert was attained, and suddenly the eyes of all were gladdened by the sight of the lake lying some 3,000 feet almost sheer below. The expedition itself stood at an altitude of 5,200 feet above the sea. But the end was not yet. Down the expedition marched to the southwest corner of the lake, where the Kakongo natives were unfriendly. No Emin Pasha had been heard of; there was no sign even that he knew of Stanley's coming or that the messenger from Zanzibar had reached him. The only boat of the expedition was at Kilinga Longa's, 190 miles away. Of the men 94 were behind sick at Ugarowwa's and Kilinga Longa's; only 173 were with Stanley; 74 of the original 341 were dead or missing; and, moreover, there was anxiety about the rear column.

Stanley's resolution was soon taken. Moving to the village of Kavalli, some distance up the steep slope from the lake, the party began a night march on December 15, and by January 7, they were back at Ibwiri. Here Fort Bodo, famous in the records of the expedition, was built. The men were brought up from the rear, and on April 7, Stanley, with Jephson and Parke, once more led the expedition to Lake Albert, this time with the boat and fresh stores. Meantime Stanley himself was on the sick list for a month. This time all the natives along the route were friendly and even generous, and on April 22, the expedition reached the chief Kavalli, who delivered to Stanley a letter wrapped in American cloth. The note was from Emin and stated that he had heard rumors of Stanley's presence in the district; it begged Stanley to wait until Emin could communicate with him. The boat was launched and Jephson set off to find Emin. On the 29th, the *Khedive* steamer came down the lake with Emin, the Italian Casati, and Jephson on board. The great object of the expedition seemed at last to be all but fulfilled.

But the end was not yet. There was the party at Fort Bodo; there were the sick further back, with whom Lieutenant Stairs had not returned when Stanley left the fort; and, above all, there was the rear column left at Yambuya with Major Barttelot. It would take some time for Emin to bring down all his people from Wadelai and other stations. So after spending over 3 weeks with the vacillating Emin, Stanley, on May 25, was once more on the march back to Fort Bodo

to bring up all hands. He left Jephson, 3 Sudanese, and 2 Zanzibaris with Emin, who gave him 102 natives as porters, and 3 irregulars to accompany him back. Fort Bodo was reached on June 8, and was found in a flourishing state, surrounded by acres of cultivated fields. But of the 56 men left at Ugarowwa's only 16 were alive for Lieutenant Stairs to bring to Fort Bodo. As there was no sign of the rear column nor of the 20 messengers sent off in March with letters for Major Barttelot, Stanley felt bound to retrace his steps through the terrible forest. This time he was better provisioned, and his people (212) escaped the horrors of the wilderness.

Fort Bodo was left on June 16, Stanley letting all his white companions remain behind. Ugarowwa's camp was deserted, and he himself, with a flotilla of fifty-seven canoes, was overtaken far down the river on August 10, and with him, 17 of the carriers sent off to Major Barttelot in March; 3 of their number had been killed. On the 17th the rear column was met with at Bonalya, 80 miles above Yambuya, and then for the first time Stanley learned of the terrible disaster that had befallen it—Barttelot shot by the Manyuema; Jameson gone down the Congo (only to die); Ward away: and Troup invalided home. No one but Dr. Bonny; of the 257 men only 72 remaining, and of these only 52 fit for service. No wonder Mr. Stanley felt too sick to write the details; and until we have the whole of the evidence it would be unfair to pronounce judgment. One thing we may say: we know, from Mr. Werner's recently published "River Life on the Congo," that before Major Barttelot left Yambuya to follow Stanley it was known to Mr. Werner, to more than one Belgian officer, to several natives, and to the Manyuema people with Barttelot, that instructions had been given by Tippu Tip to these last to shoot Major Barttelot if he did not treat them well. Yet no one cared to warn the Major and he was allowed to depart to his almost certain fate. The thing is too sickening to dwell upon. It was at this stage that Stanley sent home his first letters, which reached England on April 1, 1889, 20 months after he started from the Aruwimi, and over 2 years after he left England. The relief was intense; all sorts of sinister rumors had been floated, and most people had given up the expedition for lost.

Once more back through the weary forest, with the expedition reorganized. A new route was taken to the north of the river through a region devasted by the Arab slavers; and here the expedition came near to starvation, but once more Fort Bodo was reached, on December 20. Here things were practically as Stanley had left them; there was no sign of Emin, though he had promised to come to the fort. The combined expedition marched onwards, and Mr. Stanley, pushing on with a contingent, reached the lake for the third time, on January 18, only to learn that Emin and Jephson had been made prisoners by Emin's own men; the Mahdists had attacked the station and created a panic, and all was disorganization and vacillation. At last, however,

the chief actors in this strange drama were together again; and Mr. Stanley's account of Emin's unstable purpose, the long arguments with the Pasha to persuade him to come to a decision; the ingratitude and treachery of the Egyptians, the gathering of the people and their burdensome goods and chattels preparatory to quitting the lake – these and many other details are fresh in our memories from Stanley's own letters. But the main purpose of the expedition was accomplished, at however terrible a cost, and however disappointing it was to find that after all Emin was reluctant to be "rescued." When the start was made from Kavalli's on April 10 last, 1,500 people in all were mustered. An almost mortal illness laid Stanley low for a month shortly after the start, and it was May 8 before the huge caravan was fairly under way. Some fighting had to be done with raiders from Unyoro, but on the whole the homeward march was comparatively free from trouble, and full of interest; and on December 6 Mr. Stanley once more entered Zanzibar, which he had left 2 years and 10 months before. Such briefly are some of the incidents of the rescue expedition; let us now as briefly sum up the geographical results.

When Stanley left for Africa, in January, 1887, there remained one of the great problems of African hydrography still unsolved—what is known as the problem of the Wellé. Schweinfurth and Junker had come upon a river at some points which seemed to rise in the neighborhood of the Albert Nyanza, and appeared to flow in a northwest direction. The favorite theory at the time was that the river Wellé was really the upper course of the Shari, which runs into Lake Chad far away to the northwest. But as the Congo and its great feeders on the north, and the lie of the land in that direction, became known, it began to be conjectured that after all the Wellé might send its waters to swell the mighty volume of the great river. Stanley, I know, hoped that, among other geographical work, he might be able to throw some light on the course of this puzzling river. But, as we see now, the cares and troubles that fell upon him prevented him going much out of the way to do geographical work. While, however, Stanley was cleaving his way through the tangled forest, Lieutenant Van Géle, one of the Free State officers, proved conclusively that the Wellé was really the upper course of the Mobangi, one of the largest northern tributaries of the Congo. But another kindred problem Stanley was able to solve. Before his journey the mouth of the river Aruwimi was known; the great naval battle which he fought there on his first descent of the river is one of the most striking of the many striking pictures in the narrative of that famous journey. But beyond Yambuya its course was a blank. The river, under various names, "Ituri" being the best known, led him almost to the brink of the Albert Nyanza. One of its upper contributories is only 10 minutes' walk from the brink of the escarpment that looks down upon the lake. With many rapids, it is for a great part of its course over 500 yards wide, with groups of islands here and there.

For a considerable stretch it is navigable, and its entire length, taking all its windings into account, from its source to the Congo, is 800 miles. One of its tributaries turns out to be another river which Junker met further north, and whose destination was a puzzle.—The Nepoko.

Thus this expedition has enabled us to form clearer notions of the hydrography of this remarkable region of rivers. We see that the sources of the Congo and the Nile lie almost within a few yards of each other. Indeed, so difficult is it to determine to which river the various waters in this region send their tribute that Mr. Stanley himself, in his first letter, was confident that the southern Lake Albert belonged to the Congo and not to the Nile system. It was only actual inspection that convinced him he was mistaken. How it is that the Ituri or the Aruwimi and other rivers in the same region are attracted to the Congo and not to the Nile is easily seen from Mr. Stanley's graphic description of the lay of the country between the Congo and the Albert Nyanza. It is, he says, like the glacis of a fort, some 350 miles long, sloping gradually up from the margin of the Congo (itself at the Aruwimi mouth 1,400 feet above the sea), until ten miles beyond one of the Ituri feeders it reaches a height of 5,200 feet to descend almost perpendicularly 2,900 feet to the surface of the lake, which forms the great western reservoir of the Nile.

But when the term "glacis" is used, it must not be inferred that the ascent from the Congo to Lake Albert is smooth and unobstructed. The fact is that Mr. Stanley found himself involved in the northern section of what is probably the most extensive and densest forest region in Africa. Livingstone spent many a weary day trudging its gloomy recesses away south at Nyangwé on the Lualaba. It stretches for many miles north to the Monbuttu country. Stanley entered it at Yambuya, and tunnelled his way through it to within 50 miles of the Albert Nyanza, when it all of a sudden ceased and gave way to grassy plains and the unobstructed light of day. How far west it may extend beyond the Aruwimi he can not say; but it was probably another section of this same forest region that Mr. Paul du Chaillu struck some 30 years ago, when gorilla hunting in the Gaboon. Mr. Stanley estimates the area of this great forest region at about 300,000 square miles, which is more likely to be under than over the mark. The typical African forest, as Mr. Drummond shows in his charming book on "Tropical Africa," is not of the kind found on the Aruwimi, which is much more South American than African. Not even in the "great sponge," from which the Zambesi and the Congo draw their remote supplies, do we meet with such impenetrable density. Trees scattered about as in an English park in small open clumps form, as a rule, the type of "forest" common in Africa. The physical causes which led to the dense packing of trees over the immense area between the Congo and the Nile Lakes will form an interesting investigation. Mr. Stanley's description of the great forest region, in his letter to Mr. Bruce, is well worth quoting:

"Take a thick Scottish copse, dripping with rain. Imagine this copse to be a mere undergrowth, nourished under the impenetrable shade of ancient trees, ranging from 100 to 180 feet high; briars and thorns abundant; lazy creeks, meandering through the depths of the jungle, and sometimes a deep affluent of a great river. Imagine this forest and jungle in all stages of decay and growth—old trees falling, leaning perilously over, fallen prostrate; ants and insects of all kinds, sizes, and colors murmuring around; monkeys and chimpanzees above, queer noises of birds and animals, crashes in the jungle as troops of elephants rush away; dwarfs with poisoned arrows securely hidden behind some buttress or in some dark recess; strong brown-bodied aborigines with terribly sharp spears standing poised, still as dead stumps; rain pattering down on you every other day in the year; an impure atmosphere with its dread consequences, fever and dysentery; gloom throughout the day, and darkness almost palpable throughout the night, and then if you will imagine such a forest extending the entire distance from Plymouth to Peterhead, you will have a fair idea of some of the inconvenience endured by us from June 28 to December 5, 1887, and from June 1, 1888, to the present date, to continue again from the present date till about December 10, 1888, when I hope to say a last farewell to the Congo forest."

Mr. Stanley tries to account for this great forest region by the abundance of moisture carried over the continent from the wide Atlantic by the winds which blow landward through a great part of the year; but it is to be feared the remarkable phenomenon is not to be accounted for in so easy a way. Investigation may prove that the rain of the rainiest region in Africa comes not from the Atlantic, but the Indian Ocean, with its moisture-laden monsoons; and so we should have here a case analogous to that which occurs in South America, the forests of which resemble in many features those of the region through which Mr. Stanley has passed.

But the forest itself is not more interesting than its human denizens. The banks of the river in many places are studded with large villages, some, at least, of the native tribes being cannibals. We are here on the northern border of the true negro peoples, so that when the subject is investigated the Aruwimi savages may be found to be much mixed. But unless Europe promptly intervenes, there will shortly be few people left in these forests to investigate. Mr. Stanley came upon two slave-hunting parties, both of them manned by the merciless people of Manyuema. Already great tracts have been turned into a wilderness, and thousands of the natives driven from their homes. From the ethnologist's point of view the most interesting inhabitants of the Aruwimi forests are the hostile and cunning dwarfs, or rather pigmies, who caused the expidition so much trouble. No doubt they are the same as the Monbuttu pigmies found farther north, and essentialy similar to the pigmy population found scattered all over Africa, from the Zambesi to

the Nile, and from the Gaboon to the east coast. Mr. Du Chaillu found them in the forests of the west 30 years ago, and away south on the great Sankuru tributary of the Congo Major Wissman and his fellow explorers met them within the past few years. They seem to be the remnants of a primitive population rather than the stunted examples of the normal negro. Around the villages in the forest wherever clearings had been made the ground was of the richest character, growing crops of all kinds. Mr. Stanley has always maintained that in the high lands around the great lakes will be found the most favorable region for European enterprise; and if in time much of the forest is cleared away, the country between the Congo and Lake Albert might become the granary of Africa.

To the geographer, however, the second half of the expedition's work is fuller of interest than the first. Some curious problems had to be solved in the lake region, problems that had given rise to much discussion. When in 1864 Sir Samuel Baker stood on the lofty escarpment that looks down on the east shore of the Albert Nyanza, at Vacovia, the lake seemed to him to stretch illimitably to the south, so that for long it appeared on our maps as extending beyond 1 degree south latitude. When Stanley, many years later, on his first great expedition, after crossing from Uganda, came upon a great bay of water, he was naturally inclined to think that it was a part of Baker's lake, and called it Beatrice Gulf. But Gessi and Mason, members of Gordon Pasha's staff, circumnavigated the lake later on and found that it ended more than a degree north of the equator. So when Stanley published his narrative he made his "Beatrice Gulf" a separate lake lying to the south of the Albert Nyanza. Mr. Stanley saw only a small portion of the southern lake, Muta Nzigé, but in time it expanded and expanded on our maps until there seemed some danger of its being joined on to Lake Tanganyika. Emin himself, during his 12 years' stay in the Sudan, did something towards exploring the Albert Nyanza, and found that its southern shore was fast advancing northward, partly owing to sediment brought down by a river, and partly due to the wearing away of the rocky bed of the Upper Nile, by which much water escaped and the level of the lake subsided. Thus, when Baker stood on the shore of the lake in 1864, it may well have extended many miles farther south than it does now. But where did the river come from that Mason and Emin saw running into the lake from the south? As was pointed out above, Stanley at first thought it could not come from his own lake to the south, which he believed must send its waters to the Congo. But all controversy has now been ended. During the famous exodus of the 1,500 from Kavilli to the coast, the intensely interesting country lying between the northern lake, Albert, and the southern lake, now named Albert Edward, was traversed. Great white, grassy plains stretch away south from the shores of Lake Albert, which under the glitter of a tropical sun might well be mistaken for water; evidently they had been

under water at quite a recent period. But soon the country begins to rise, and round the base of a great mountain boss the river Semliki winds its way through its valley, receiving through the picturesque glens many streams of water from the snows that clothe the mountain tops. Here we have a splendid country, unfortunately harassed by the raids of the Wanyoro, in dread of whom the simple natives of the mountain side often creep up to near the limit of snow. Up the mountain, which Lieutenant Stairs ascended for over 10,000 feet, blackberries, bilberries, violets, heaths, lichens, and trees that might have reminded him of England flourish abundantly. Here evidently we have a region that might well harbor a European population. The mountain itself, Ruwenzori, a great boss with numerous spurs, is quite evidently an extinct volcano, rising to something like 19,000 feet, and reminding one of Kilimanjaro, farther to the east. It is not yet clear whether it is the same mountain as the Gordon Bennett seen by Stanley in his former expedition, though the probability is that, if distinct, they belong to the same group or mass. Apart from the mountain the country gradually ascends as the Semliki is traced up to its origin in Lake Albert Edward. Mr. Stanley found that, after all, the southern Nyanza belongs to the great Nile system, giving origin to the farthest southwest source of Egypt's wonderful river, which we know receives a tribute from the snows of the equator.

The southern lake itself is of comparatively small dimensions, probably not more than 45 miles long, and is 900 feet above the northern Lake Albert. Mr. Stanley only skirted its west, north, and east shores, so that probably he has not been able to obtain complete data as to size and shape. But he has solved one of the few remaining great problems in African geography. The two lakes lie in a trough, the sides of which rise steeply in places 3,000 feet, to the great plateaus that extend away east and west. This trough, from the north end of Lake Albert to the south end of Lake Albert Edward, is some 260 statute miles in length. About 100 miles of this is occupied by the former lake, 45 by the latter, and the rest by the country between, where the trough, if we may indulge in an Irishism, becomes partly a plain, and partly a great mountain mass. But this trough, or fissure, a glance at a good map will show, is continued more or less south and southeast in Lakes Tanganyika and Nayassa, which are essentially of the same character as Lakes Albert and Albert Edward, and totally different from such lakes as Victoria Nyanza and Bangweolo. Here we have a feature of the greatest geographical interest, which still has to be worked out as to its origin.

There is little more to say as to the geographical results of the Emin Pasha relief expedition. There are many minute details of great interest, which the reader may see for himself in Mr. Stanley's letters, or in his forthcoming detailed narrative. In his own characteristic way he tells of the tribes and peoples around the lakes, and between the

lakes and the coast; and it was left for him on his way home to discover a great southwest extension of Victoria Nyanza, which brings that lake within 150 miles of Lake Tanganyika. The results which have been achieved have been achieved at a great sacrifice of life and of suffering to all concerned; but no one, I am sure, will wish that the work had been left undone. The few great geographical problems in Africa that Livingstone had to leave untouched, Stanley has solved. Little remains for himself and others in the future beyond the filling in of details; but these are all-important, and will keep the great army of explorers busy for many years, if not for generations.

ANTARCTIC EXPLORATION.*

By G. S. Griffiths.

My experience during the four years which have elapsed since this project was first mooted in Melbourne is that any reference to the subject is sure to be met with the query *Cui bono?* What good can it do? What benefit can come from it? What is the object to be served by such an expedition?

In setting myself to the task of answering these questions let me observe that it would indeed be strange if an unexplored region 8,000,000 square miles in area—twice the size of Europe—and grouped around the axis of rotation and the magnetic pole could fail to yield to investigators some novel and valuable information. But when we notice that the circle is engirdled without by peculiar physical conditions which must be correlated to special physical conditions within, speculation is exchanged for a confident belief that an adequate reward must await the skilled explorer. The expected additions to the geography of the region are, of all the knowledge that is to be sought for there, the least valuable. Where so many of the physical features of the country—the hills, the valleys, and the drainage lines—have been buried beneath the snow of ages, a naked outline, a bare skeleton of a map, is the utmost that can be delineated. Still, even such knowledge as this has a distinct value, and as it can be acquired by the explorers as they proceed about their more important researches, its relatively small value ought not to be admitted as a complete objection to any enterprise which has other objects of importance. Our present acquaintance with the geography of the region is excessively limited. Ross just viewed the coasts of Victoria Land between 163° E. and 160° W. longitude; he trod its barren strand twice, but on each occasion for a few minutes only. From the adjacent gulf he measured the heights of its volcanoes, and from its offing he sketched the walls of its icy barrier. Wilkes traced on our map a shore line from 97° E. to 167° E. longitude, and he backed it up with a range of mountains, but he landed nowhere. Subsequently Ross sailed over the site assigned to part of this land,

* An address on "The Objects of Antarctic Exploration," delivered at the annual meeting of the Bankers' Institute of Australia, at Melbourne, on Wednesday, August 27. (From *Nature*, October 16, 1890, vol. XLII, pp. 601–604.)

and hove his lead 600 fathoms deep where Wilkes had drawn a mountain. He tells us that the weather was so very clear that had high land been within 70 miles of that position he must have seen it ("Ross's Voyage," 1278). More recently Nares, in the *Challenger*, tested another part of Wilkes's coast line, and with a like result; and these circumstances throw doubts upon the value of his reported discoveries. D'Urville subsequently followed a bold shore for a distance of about 300 miles from 136° E. to 142° E. longitude; whilst in 67° S. latitude, and between 45° E. and 60° E. longitude, are Enderby's and Kemp's lands. Again, there is land to the south of the Horn which trends from 45° to 75° S. latitude. These few discontinuous coast lines comprise all our scanty knowledge of the Antarctic land. It will be seen from these facts that the principal geographical problem awaiting solution in these regions is the interconnection of these scattered shores. The question is, do they constitute parts of a continent, or are they, like the coasts of Greenland, portions of an archipelago, smothered under an overload of frozen snow, which conceals their insularity? Ross inclined to the latter view, and he believed that a wide channel leading towards the Pole existed between North Cape and the Balleny Islands ("Ross's Voyage," 1221). This view was also held by the late Sir Wyville Thomson. A series of careful observations upon the local currents might throw some light upon these questions. Ross notes several such in his log. Off Possession Island a current, running southward, took the ships to windward (*ibid.*, 1195). Off Coulman Island another drifted them in the same direction at the rate of 18 miles a day (*ibid.*, 1204). A three-quarter knot northerly current was felt off the barrier, and may have issued from beneath some part of it. Such isolated observations are of little value, but they were multiplied, and were the currents correlated with the winds experienced the information thus obtained might enable us to detect the existence of straits, even where the channels themselves are masked by ice barriers.

Finally, it is calculated that the center of the polar ice-cap must be 3 miles, and may be 12 miles deep, and that the material of this ice mountain being viscous, its base must spread out under the crushing pressure of the weight of its center. The extrusive movement thus set up is supposed to thrust the ice cliffs off the land at the rate of a quarter of a mile per annum. These are some of the geographical questions which await settlement.

In the geology of this region we have another subject replete with interest. The lofty volcanoes of Victoria Land must present peculiar features. Nowhere else do fire and frost divide the sway so completely. Ross saw Erebus belching out lava and ashes over the snow and ice which coated its flanks. This circumstance leads us to speculate on the strata that would result from the alternate fall of snow and ashes during long periods and under a low temperature. Volcanoes are built up, as contra-distinguished from other mountains, which result

from elevation or erosion. They consist of débris piled round a vent. Lava and ashes surround the crater in alternate layers. But in this polar region the snow-fall must be taken into account as well as the ash deposit and the lava flow. It may be thought that any volcanic ejecta would speedily melt the snow upon which they fell, but this does not by any means necessarily follow. Volcanic ash, the most wide-spread and most abundant material ejected, falls comparatively cold, cakes, and then forms one of the most effective nonconductors known. When such a layer a few inches thick is spread over snow even molten lava may flow over it without melting the snow beneath. This may seem to be incredible, but it has been observed to occur. In 1828 Lyell saw on the flanks of Etna a glacier sealed up under a crust of lava. Now, the Antarctic is the region of thick-ribbed ice. All exposed surfaces are quickly covered with snow. Snow-falls, fish-falls, and lava-flows must have been heaping themselves up around the craters during unknown ages. What has been the result? Has the viscosity of the ice been modified by the intercalation of beds of rigid lava and of hard-set ash? Does the growing mass tend to pile up or to settle down and spread out? Is the ice wasted by evaporation, or does the ash layer preserve it against this mode of dissipation? These interesting questions can be studied round the South Pole, and perhaps nowhere else so well.

Another question of interest, as bearing upon the location of the great Antarctic continent, which it is now certain existed in the Secondary period of geologists, is the nature of the rocks upon which the lowest of these lava beds rest. If they can be discovered, and if they then be found to be sedimentary rocks—such as slates and sandstones, or plutonic rocks—such as granite, they will at once afford us some data to go upon, for the surface exposure of granite signifies that the locality has been part of a continental land sufficiently long for the weathering and removal of the many thousands of feet of sedimentary rocks which of necessity overlie crystalline rocks during their genesis; whilst the presence of sedimentary rocks implies the sometime proximity of a continent from the surfaces of which alone these sediments, as rain-wash, could have been derived.

As ancient slate rocks have already been discovered in the ice-clad South Georgias, and as the drag-nets of the *Erebus* and the *Challenger* have brought up from the beds of these icy seas fragments of sandstones, slates, and granite, as well as the typical blue mud which invariably fringes continental land, there is every reason to expect that such strata will be found.

Wherever the state of the snow will permit, the polar mountains should be searched for basaltic dikes, in the hope that masses of specular iron and nickel might be found, similar to those discovered by Nordenskiöld, at Ovifak, in north Greenland. The interest taken in these metallic masses arises from the fact that they alone, of all the

rocks of the earth, resemble those masses of extra-terrestial origin which we know as meteorites. Such bodies of unoxidized metal are unknown elsewhere in the mass, and why they are peculiar to the Arctic it is hard to say. Should similar masses be found within the Antarctic, a fresh stimulus would be given to speculation. Geologists would have to consider whether the oxidized strata of the earth's crust thin out at the poles; whether in such a case the thinning is due to severe local erosion, or to the protection against oxygen afforded to the surface of the polar regions by their ice caps, or to what other cause. Such discoveries would add something to our knowledge of the materials of the interior of our globe and their relation to those of meteorites.

Still looking for fresh knowledge in the same direction, a series of pendulum observations should be taken at points as near as possible to the pole. Within the Arctic circle the pendulum makes about 240 more vibrations per day than it does at the equator. The vibrations increase in number there because the force of gravity at the earth's surface is more intense in that area, and this again is believed to be due to the oblateness of that part of the earth's figure, but it might be caused by the bodily approach to the surface at the poles of the masses of dense ultra-basic rocks just referred to. Thus, pendulum experiments may reveal to us the earth's figure, and a series of such observations, recorded from such a vast and untried area, must yield important data for the physicist to work up. We should probably learn from such investigations whether the earth's figure is as much flattened at the Antarctic as it is known to be at the Arctic.

We now know that in the past the North Polar regions have enjoyed a temperate climate more than once. Abundant seams of Paleozoic coal, large deposits of fossiliferous Jurassic rocks, and extensive Eocene beds, containing the remains of evergreen and deciduous trees and flowering plants, occur far within the Arctic circle. This circumstance leads us to wonder whether the corresponding southern latitudes have ever experienced similar climatic vicissitudes. Conclusive evidence on this point it is difficult to get, but competent biologists who have examined the floras and faunas of South Africa and Australia, of New Zealand, South America, and the isolated islets of the Southern Ocean, find features which absolutely involve the existence of an extensive Antarctic land—a land which must have been clothed with a varied vegetation, and have been alive with beasts, birds, and insects. As it also had had its fresh-water fishes, it must have had its rivers flowing and not frost-bound, and in those circumstances we again see indications of a modified Antarctic climate. Let us briefly consider some of the evidence for the existence of this continent. We are told by Professor Hutton, of Christchurch, that 44 per cent. of the New Zealand flora is of Antarctic origin. The Auckland, Campbell, and Macquarie Islands all support Antarctic plants, some of which appear never to

have reached New Zealand. New Zealand and South America have three flowering plants in common, also two fresh-water fishes, five seaweeds, three marine crustaceans, one marine mollusk, and one marine fish. Similarly New Zealand and Africa have certain common forms, and the floras and faunas of the Kerguelen, the Crozets, and the Marion Islands are almost identical, although in each case the islands are very small, and very isolated from each other and from the rest of the world. Tristan d'Acunha has fifty-eight species of marine Mollusca, of which number thirteen are also found in South America, six or seven in New Zealand, and four in South Africa (Hutton's *Origin of New Zealand Flora and Fauna*). Temperate South America has seventy-four genera of plants in common with New Zealand, and eleven of its species are identical (Wallace's *Island Life*). Penguins of the genus Eudyptes are common to South America and Australia (Wallace, *Dist. of Animals*, 1399). Three groups of fresh-water fishes are entirely confined to these two regions. Aphritis, a fresh-water genus, has one species in Tasmania and two in Patagonia. Another small group of fishes known as the Haplochitonidæ inhabit Tierra del Fuego, the Falklands, and South Australia, and are not found elsewhere, while the genus Galaxias is confined to South Temperate America, New Zealand, and Australia. Yet the lands which have these plants and animals in common are so widely separated from each other that they could not now possibly interchange their inhabitants. Certainly towards the equator they approach each other rather more, but even this fact fails to account for the present distribution, for, as Wallace has pointed out, "the heat-loving Reptilia afford hardly any indications of close affinity between the two regions" of South America and Australia, "whilst the cold-enduring Amphibia and fresh-water fishes offer them in abundance" (Wallace, *Dist. of Animals*, 1400). Thus we see that to the north interchange is prohibited by tropical heat, while it is barred to the south by a nearly shoreless circumpolar sea. Yet there must have been some means of intercommunication in the past, and it appears certain that it took the shape of a common fatherland for the various common forms from which they spread to the northern hemisphere. As this father-land must have been accessible from all these scattered southern lands, its size and its disposition must have been such as would serve the emigrants either as a bridge or as a series of stepping-stones. It must have been either a continent or an archipelago.

But a further and a peculiar interest attaches to this lost continent. Those who have any acquaintance with geology know that the placental Mammalia—that is, animals which are classed with such higher forms of life as apes, cats, dogs, bears, horses, and oxen—appear very abruptly with the incoming of the Tertiary period. Now, judging by analogy, it is not likely that these creatures can have been developed out of Mesozoic forms with anything like the suddenness of their apparent

entrance upon the scene. For such changes they must have required a long time, and an extensive region of the earth, and it is probable that each of them had a lengthy series of progenitors, which ultimately linked it back to lower forms.

Why, then, it is constantly asked, if this was the sequence of creation, do these missing links never turn up? In reply to this query, it was suggested by Huxley that they may have been developed in some lost continent, the boundaries of which were gradually shifted by the slow elevation of the sea margin on one side and its simultaneous slow depression upon the other, so that there has always been in existence a large dry area with its live stock. This dry spot, with its fauna and flora, like a great raft or Noah's Ark, moved with great slowness in whatever direction the great earth-undulation travelled. But to-day this area, with its fossil evidences, is a sea-bottom; and Huxley supposes that the continent, which once occupied a part of the Pacific Ocean, is now represented by Asia.

This movement of land-surface translation eastwards eventually created a connection between this land and Africa and Europe, and if when this happened the mammalia spread rapidly over these countries, this circumstance would account for the abruptness of their appearance there.

Now, Mr. Blanford, the president of the Geological Society of London, in his annual address, recently delivered, advances matters a stage further, for he tells us that a growing acquaintance with the biology of the world leads naturalists to a belief that the placental mammalia and other of the higher forms of terrestrial life originated during the Mesozoic period still further to the southwards—that is to say, in the lost Antarctic continent, for the traces of which we desire to seek.

But it almost necessarily follows that wherever the mammalia were developed there also man had his birth-place, and if these speculations should prove to have been well founded we may have to shift the location of the Garden of Eden from the northern to the southern hemisphere.

I need hardly suggest to you that possibilities such as these must add greatly to our interest in the recovery of any traces of this mysterious region. This land appears to have sunk beneath the seas after the close of the Mesozoic. Now, the submergence of any mass of land will disturb the climatic equilibrium of that region, and the disappearance of an Antarctic continent would prove extremely potent in varying the climate of this hemisphere. For to-day the sun's rays fall on the South Polar regions to small purpose. The unstable sea absorbs the heat, and in wide and comparatively warm streams it carries off the caloric to the northern hemisphere to raise its temperature at the expense of ours. But when extensive land received those same heat rays, its rigid surfaces, so to speak, tethered their caloric in this hemi-

sphere, and thus when there was no mobile current to steal northwards with it, warmth could accumulate and modify the climate.

Under the influences of such changes the icy mantle would be slowly rolled back towards the South Pole, and thus many plants and animals were able to live and multiply in latitudes that to day are barren. What has undoubtedly occurred in the extreme north is equally possible in the extreme south. But if it did occur—if South Polar lands, now ice bound, were then as prolific of life as Disco and Spitzbergen once were—then, like Spitzbergen and Disco, the unsubmerged remnants of this continent may still retain organic evidences of the fact in the shape of fossil-bearing beds, and the discovery of such deposits would confirm or confute such speculations as these. The key to the geological problem lies within the Antarctic Circle, and to find it would be to recover some of the past history of the southern hemisphere. There is no reason to despair of discovering such evidence, as Dr. McCormack, in his account of Ross's voyage, records that portions of Victoria Land were free from snow, and therefore available for investigation; besides which their surface may still support some living forms, for they can not be colder or bleaker than the peaks which rise out of the continental ice of North Greenland, and these, long held to be sterile, have recently disclosed the existence upon them of a rich though humble flora.

We have now to consider some important meteorological questions. If we look at the distribution of the atmosphere around the globe we shall see that it is spread unequally. It forms a stratum which is deeper within the tropics than about the poles and over the northern than over the southern hemisphere, so that the barometer normals fall more as we approach the Antarctic than they do when we near the Arctic. Maury, taking the known isobars as his guide, has calculated that the mean pressure at the North Pole is 29.1, but that it is only 28 at the South (Maury's *Meteorology*, 259). In other words, the Antarctic Circle is permanently much barer of atmosphere than any other part of the globe. Again, if we consult a wind chart we shall see that both poles are marked as calm areas. Each is the dead center of a perpetual wind vortex, but the South Polar indraught is the stronger. Polarward winds blow across the forty-fifth degree of north latitude for 189 days in the year, but across the forty-fifth degree of south latitude for 209 days. And while they are drawn in to the North Pole from over a disk-shaped area 5,500 miles in diameter, the South Polar indraught is felt throughout an area of 7,000 miles across. Lastly, the winds which circulate about the South Pole are more heavily charged with moisture than are the winds of corresponding parts of the other hemisphere. Now, the extreme degree in which these three conditions, of a perpetual grand cyclone, a moist atmosphere, and a low barometer, co-operate without the Antarctic ought to produce within it an exceptional meteorological state, and the point to be determined is

what that condition may be. Maury maintained that the conjunction will make the climate of the South Polar area milder than that of the north. His theory is that the saturated winds being drawn up to great heights within the Antarctic must then be eased of their moisture, and that simultaneously they must disengage vast quantities of latent heat; and it is because more heat must be liberated in this manner in the South Polar regions than in the north that he infers a less severe climate for the Antarctic. He estimates that the resultant relative differences between the two polar climates will be greater than that between a Canadian and an English winter (Maury's *Meteorology*, p. 466). Ross reports that the South Polar summer is rather colder than that of the north, but still the southern winter may be less extreme, and so the mean temperature may be higher. If we examine the weather reports logged by Antarctic voyagers, instead of the temperature merely, the advantage still seems to rest with the south. In the first place, when the voyager enters the Antarctic he sails out of a tempestuous zone into one of calms. To demonstrate the truth of this statement I have made an abstract of Ross's log for the two months of January and February, 1841, which he spent within the Antarctic Circle. To enable everyone to understand it, it may be well to explain that the wind force is registered in figures from 0, which stands for a dead calm, up to 12, which represents a hurricane. I find that during these 60 days it never once blew with the force 8—that is, a fresh gale; only twice did it blow force 7, and then only for half a day each time. Force 5 to 6—fresh to strong breezes—is logged on 21 days. Force 1 to 3—that is, gentle breezes—prevailed on 34 days. The mean wind force registered under the entire 60 days was 3.43—that is, only a 4 to 5 knot breeze. On 38 days blue sky was logged. They never had a single fog, and on 11 days only was it even misty. On the other hand, snow fell almost every second day. We find such entries as these: "Beautifully clear weather," and "Atmosphere so extraordinarily clear that Mount Herschel, distant 90 miles, looked only 30 miles distant." And again, "Land seen 120 miles distant; sky beautifully clear." Nor was this season exceptional, so far as we can tell, for Dr. McCormack, of the *Erebus*, in the third year of the voyage, and after they had left the Antarctic for the third and last time, enters in his diary the following remark. He says: "It is a curious thing that we have always met with the finest weather within the Antarctic circle; clear, cloudless sky, bright sun, light wind, and a long swell" (McCormack's *Antarctic Voyage*, vol. I, p. 345). It would seem as if the stormy westerlies, so familiar to all Australian visitors, had given to the whole southern hemisphere a name for bad weather, which, as yet at least, has not been earned by the South Polar regions. It is probable, too, that the almost continuous gloom and fog of the Arctic (Scoresby's *Arctic Regions*, pp. 97 and 137) July and August have prejudiced seamen against the Antarctic summer. The true character of the climate of this region is

one of the problems awaiting solution. Whatever its nature may be, the area is so large and so near to us that its meteorology must have a dominant influence on the climate of Australia, and on this fact the value of a knowledge of the weather of these parts must rest.

To turn to another branch of science, there are several questions relating to the earth's magnetism which require for their solution long-maintained and continuous observations within the Antarctic circle. The mean or permanent distribution of the world's magnetism is believed to depend upon causes acting in the interior of the earth, while the periodic variations of the needle probably arise from the superficial and subordinate currents produced by the daily and yearly variations in the temperature of the earth's surface. Other variations occur at irregular intervals, and these are supposed to be due to atmospheric electricity. All these different currents are excessively frequent and powerful about the poles, and a sufficient series of observations might enable physicists to differentiate the various kinds of currents, and to trace them to their several sources, whether internal, superficial, or meteoric. To do this properly at least one land observatory should be established for a period. In it, the variation, dip, and intensity of the magnetic currents, as well as the momentary fluctuations, of these elements would all be recorded. Fixed term days would be agreed on with the observatories of Australia, of the Cape, America, and Europe, and during these terms a concerted continuous watch would be kept up all round the globe to determine which vibrations were local and which general.

The present exact position of the principal south magnetic pole has also to be fixed, and data to be obtained from which to calculate the rate of changes in the future, and the same may be said of the foci of magnetic intensity and their movements. In relation to this part of the subject, Captain Creak recently reported to the British Association his conclusions in the following terms. He says: "Great advantage to the science of terrestrial magnetism would be derived from a new magnetic survey of the southern hemisphere extending from the parallel of 40° south, as far towards the geographical pole as possible."

Intimately connected with terrestrial magnetism are the phenomena of auroras. Their nature is very obscure, but quite recently a distinct advance has been made towards discovering some of the laws which regulate them. Thanks to the labors of Dr. Sophus Tromholt, who has spent a year within the Arctic circle studying them, we now know that their movements are not as eccentric as they have hitherto appeared to be. He tells us that the Aurora Borealis, with its crown of many lights, encircles the pole obliquely, and that it has its lower edge suspended above the earth at a height of from 50 to 100 miles, the mean of 18 trigonometrical measurements, taken with a base line of 50 miles, being 75 miles. The aurora forms a ring round the pole which changes its latitude four times a year. At the equinoxes it attains its greatest distance from the pole, and at midsummer and midwinter it approaches

it most closely, and it has a zone of maximum intensity which is placed obliquely between the parallels of 60° and 70° N. The length of its meridional excursion varies from year to year, decreasing and increasing through tolerably regular periods, and reaching a maximum about every 11 years, when, also, its appearance simultaneously attains to its greatest brilliancy. Again, it has its regular yearly and daily movements or periods. At the winter solstice it reaches its maximum annual intensity, and it has its daily maximum at from 8 P. M. and 2 A. M., according to the latitude. Thus at Prague, in latitude 50° N., the lights appear at about 8.45 P. M.; at Upsala, latitude 60° N., at 9.30 P. M.; at Bossekop, 70° N., at 1.30 A. M. Now, while these data may be true for the northern hemisphere, it remains to be proved how far they apply to the southern. Indeed, seeing that the atmosphere of the latter region is moister and shallower than that of the former, it is probable that the phenomena would be modified. A systematic observation of the Aurora Australis at a number of stations in high latitudes is therefore desirable.

Whether or not there is any connection between auroral exhibitions and the weather is a disputed point. Tromholt believes that such a relationship is probable (*Under the Rays*, 1283). He says that, "however clear the sky, it always became overcast immediately after a vivid exhibition, and it generally cleared again as quickly" (*Under the Rays*, 1235). Payer declares that brilliant auroras were generally succeeded by bad weather (*Voyage of Tegethoff*, 1324), but that those which had a low altitude and little mobility appeared to precede calms. Ross remarks of a particular display "that it was followed by a fall of snow, as usual" (*Ross's Voyage*, 1312). Scoresby appears to have formed the opinion that there is a relationship indicated by his experience. It is, therefore, allowable to regard the ultimate establishment of some connection between these two phenomena as a possible contingency. If, then, we look at the eleven-year cycle of auroral intensity from the meteorological point of view, it assumes a new interest, for these periods may coincide with the cycles of wet and dry seasons which some meteorologists have deduced from the records of our Australian climate, and the culmination of the one might be related to some equivalent change in the other. For if a solitary auroral display be followed by a lowered sky, surely a period of continuous auroras might give rise to a period of continuous cloudy weather, with rain and snow. Fritz considers that he has established this eleven-year cycle upon the strength of auroral records extending from 1583 to 1874, and his deductions have been verified by others.

In January, 1886, we had a wide-spread and heavy rain-fall, and also an auroral display seen only at Hobart, but which was sufficiently powerful to totally suspend communication over all the telegraph lines situated between Tasmania and the China coast. This sensitiveness upon the part of the electric currents to auroral excitation is not novel, for long experience on the telegraph wires of Scandinavia has shown that

there is such a delicate sympathy between them that the electric wires there manifest the same daily and yearly periods of activity as those that mark the auroras. The current that reveals itself in fire in the higher regions of the atmosphere is precisely the same current that plagues the operator in his office. Therefore, in the records of these troublesome earth currents, now being accumulated at the observatory by Mr. Ellery, we are collecting valuable data, which may possibly enable the physicist to count the unseen auroras of the Antarctic, to calculate their periods of activity and lethargy, and, again, to check these with our seasons. But it need hardly be said that the observations which may be made in the higher latitudes and directly under the rays of the Aurora Australis will have the greater value, because it is only near the zone of maximum auroral intensity that the phenomena are manifested in all their aspects. In this periodicity of the southern aurora I have named the last scientific problem to which I had to direct your attention, and I would point out that if its determination should give to us any clew to the changes in the Australian seasons which would enable us to forecast their mutations in any degree, it would give to us, in conducting those great interests of the country which depend for their success upon the annual rain-fall, an advantage which would be worth many times over all the cost of the expeditions necessary to establish it.

Finally, there is a commercial object to be served by Antarctic exploration, and it is to be found in the establishment of a whaling trade between this region and Australia. The price of whalebone has now risen to the large sum of £2,000 a ton, which adds greatly to the possibilities of securing to the whalers a profitable return. Sir James Ross and his officers have left it on record that the whale of commerce was seen by them in these seas, beyond the possibility of a mistake. They have stated that the animals were large, and very tame, and that they could have been caught in large numbers. Within the last few years whales have been getting very scarce in the Arctic, and in consequence of this two of the most successful of the whaling masters of the present day, Capts. David and John Gray, of Peterhead, Scotland, have devoted some labor to collecting all the data relating to this question, and they have consulted such survivors of Ross's expedition as are still available. They have published the results of their investigations in a pamphlet, in which they urge the establishment of the fishery strongly, and they state their conclusions in the following words. They say: "We think it is established beyond doubt that whales of a species similar to the right or Greenland whale, found in high northern latitudes, exist in great numbers in the Antarctic seas, and that the establishment of a whale fishery within that area would be attended with successful and profitable results." It is not necessary for me to add anything to the opinion of such experts in the business. All I desire to say is that if such a fishery were created, with its headquarters in Melbourne, it

would probably be a material addition to our prosperity, and it would soon increase our population by causing the families of the hardy seamen who would man the fleet to remove from their homes in Shetland and Orkney and the Scotch coasts and settle here.

In conclusion, I venture to submit that I have been able to point to good and substantial objects, both scientific and commercial, to justify a renewal of Antarctic research, and I feel assured that nothing could bring to us greater distinction in the eyes of the whole civilized world than such an expedition, judiciously planned and skillfully carried out.

HISTORY OF GEODETIC OPERATIONS IN RUSSIA.

By Col. B. WITSKOWSKI, of the General Staff,
and Prof. J. HOWARD GORE, B. S., Ph. D.

From the time of the unification of the several Moscovite states there has been felt the need of descriptions of the separate parts. But it was not until the middle of the sixteenth century that the inexact and unsatisfactory "Great Plan" made any attempt towards filling this need. Systematic geodetic operations, however, did not receive any attention until the time of Peter the Great, who sent out foreigners, especially invited for this purpose, together with such Russians as had been under their instruction, to make surveys of different parts of the empire. These disconnected surveys were made without any definite correlation of the separate parts, and in a very crude manner—using cords for measuring lines, astrolabes for angle determinations, and large quadrants for latitude observations.

When Delisle arrived in St. Petersburg in 1726, in response to an invitation from the emperor, an impetus was given to the exact sciences. In connection with the Academy of Sciences, founded likewise in 1726, he organized special astronomic expeditions for determining, in addition to other work, the geographical position of points to check the geography of the Great Plan, and to make such revisions as might be necessary. In these operations longitudes were determined by the eclipse of Jupiter's satellites. The result of these expeditions was the Russian Atlas, edited by the Academy in 1745, consisting of one general and nineteen special maps, constructed on a scale of 34 versts* to the inch. Notwithstanding its imperfections, this atlas was far superior to any of its period, and ante-dated all general maps except those of France and Italy.

Delisle awakened great interest in astronomy at Russia's capital, and secured the necessary permission and aid to observe every important astronomic event that was visible from any part of her domain. This created a need for assistants, and called forth a number of astronomers whose names are known to us, as Krassilnikow, a member of Bering's expedition; Krashennikow, who made the first description of Kam-

* 1 verst equals 3,500 English feet.

chatka, and Roumovsky, explorer of northern European Russia. In 1789 the last named published a table of the geographical positions of sixty-two stations in Russia. It may be remarked, in this connection, that at that time no country of western Europe possessed such a number of well-determined places.

In the eighteenth century the measurement of an arc of the meridian was even planned, but why it was not carried out is not known at the present time. Delisle thought it possible to measure in the meridian of St. Petersburg an arc of 22° or 23°, and in the year 1737 a base line was measured on the ice between St. Petersburg and Cronstadt and several stations were selected.

In 1796, by order of the Emperor Paul, the Depot of Maps was instituted, which laid a solid foundation for a separate department specially occupied with all the geodetic and cartographic work in the state. Soon after Schubert gave special instruction in astronomy and geodesy, looking to the better qualification of men for this work. But owing to the troublesome times at the beginning of the present century, a stop was put to the progress of all geodetic operations. However, cartographic work was making rapid progress, not only in the interior of the state, but in such neighboring states as the fortunes of war introduced Russian troops, as for instance in 1816–'18, while the army was in France, more than 10,000 square versts were mapped. In this survey mountains were for the first time drawn by cross hatchings, according to Lehman's system.

After the close of the war with Napoleon geodetic operations in Russia began to develop very rapidly, and lying at the foundation of accurate maps, the practical value was so apparent that no obstacle to their progress was encountered. The great extent of the country precluded the plan which naturally suggested itself of covering the entire state with a network of triangulation before beginning the mapping. Consequently independent nets were started which later could be united and brought into a harmonious whole. Vilna was the first province which was covered by a triangulation. It was prosecuted in 1816–1821, under the direction of General Tenner, and is of interest to us because its principal triangle entered into the great meridional arc.

This work rested on three bases measured with an apparatus constructed on the Borda principle under the supervision of Professor Reisig. Tenner discovered that the behavior of the metal components under varying temperatures was wholly unreliable and at once proposed an apparatus consisting of only one metal, in the shape of a bar of iron 14 feet long, with a slide projecting beyond the end of one of the bars to measure the interval between two bars when they are brought into approximate contact. This device has been employed in a variety of forms and is now known as the contact-slide. The temperature of the bars during the measuring was ascertained from two thermometers on each bar, the bulbs of which were inserted into the body of the bar.

The angles in this net were measured with repeating circles, employing for each angle from twenty to fifty repetitions. For the probable error of angle determinations $0''.62$ was found to be an average. Astronomic observations were made at only one point with the longitudes referred to the observatory of Vilna.

Almost simultaneously with the above-named operations in Vilna, a young enthusiastic astronomer of the Dorpat University, W. Struve, acting in response to a request from the Livonian Economical Society, covered Livonia with a trigonometrical net. In this work the angles were measured with a sextant and the bases with wooden rods, so that but little confidence can be placed in the results, still it was while engaged upon this work that Struve formed a liking for geodesy and conceived the plan of making a great arc measurement for the purpose of determining the lengths of degrees in different latitudes.

His great interest in the work attracted the attention of the university authorities, and in answer to his request they furnished him with the necessary means and instruments. The base apparatus was of his own invention, and still bears his name. The salient feature introduced in its construction was the contact lever, which indicated on a graduated arc over which one end of the lever swept the exact measuring length each time the bar was put in place. Inclination was determined by means of a special level.

A large theodolite, provided with four verniers, served as the angle-reading instrument. In this work Struve was the first to abandon the seductive, unreliable method of repetition, using in its place the method of directions. It was so apparent to many that an angle measured say twenty times, with only one reading of the circle, would be affected by an error only one-twentieth as large as if the single reading corresponded to only one pointing. Struve clearly saw that this method introduced other errors more pernicious than those of reading, but so firmly was the Borda repeating circle fixed in the confidence of its users that had not Gauss embraced the new plan in his monumental work it is likely that the method of repetition would have continued to impair geodetic determinations. However much we are indebted to Gauss for assisting in the change, we owe the inception to Struve.

The results of this first degree measurement, which extended from the isle of Hohland, in the Finnish Gulf, to the town Jacobstadt, on the river Dvina, are given in Struve's *Breitengradmessung in den Ostseeprovinzen Russlands*, Dorpat, 1831.

On finishing this work, Struve, seeing no natural obstacles in the way, hoped to extend an arc along the meridian of Dorpat. He was soon in a position to take up this undertaking, since as director of the observatory at Pulkova he was virtually at the head of all astronomic and geodetic operations in Russia. Fortunately he received the approbation of Emperor Nicholas, and under his patronage this branch of scientific work prospered. The great arc, which received well-nigh uninterrupted attention for more than 40 years, had as its central fea-

ture the Baltic arc; to this was successively joined Fenner's meridional chains in the provinces of Vilna, Lithonia, Volynia, and Podolsky. In the years 1830–1844, triangles were added until the chain reached from the isle of Hohland to Tornea, in the north, and in the following years Tenner carried the southern end through Bessarabia, terminating at Staraja-Nekrasovka, at the mouth of the Danube. For the continuation of the arc northward from Tornea the co-operation of the Swedish Government was necessary, as the best disposition of the triangles threw the stations alternately in Russia and in Sweden, finally crossing the north of Norway. Struve went to Stockholm to lay the matter before King Oscar, who at once entered into the spirit of the undertaking, and not only gave his consent but contributed aid in carrying it on. In 1845, this part of the work was begun, and with the assistance of Selander, on the part of Sweden, and Hansteen, for Norway, the field work was completed in 1852.

This entire arc comprises 25° 20′, in which there are 258 principal triangles resting on ten base lines, and fixed in position on the earth's surface by astronomic observations at thirteen stations. As a supplement to this work may be mentioned the chronometrical expedition between Pulkova and Dorpat, made in 1854. In this operation thirty-one chronometers were transported ten times. The details of this arc measurement are given quite fully in "Arc du Méridien," which was published in French and Russian in 1860. This arc has entered into all of the more recent determinations of the figure of the earth, and in the computations of General Bonsdorff it alone gives for the ellipticity $\frac{1}{298.59}$, which agrees quite well with the best values.

Arcs of parallel have also received some attention. In 1826, the French Government announced that there was already in existence an arc of parallel approximately in latitude 47° N. from Brest, on the west, to Tchernowizt, on the east, and that if the Russians would continue this arc eastward valuable geodetic data would result. The plan was received with favor, but different obstacles intervened, so that it was not until 1848 that it could be carried out. By this time the triangulation had reached the so-called New Russia, and in the general purpose to cover this entire section with a network of triangles General Wrochenko, the chief, received instructions to so perform his work that amongst his triangles there should be an uninterrupted chain along this parallel of such strength and accuracy that they could form an integral part of this arc.

The field operations continued without serious interruption up to their completion in 1856, extending over an arc of about 20° amplitude from Bologan to Astrakhan, at the mouth of the Volga. For this work three bases have been measured in addition to the checks which came down from the northern work. As the determination of the amplitude depends upon differences of longitudes this part of the work was delayed awaiting the construction of telegraph lines. At the

present time longitudes of five stations are known, and the final results will soon be published.

In 1860, it was decided to carry an arc along the fifty-second parallel, which, when completed, would have, between Haversfordwest, in England, and Orsk, on the river Ural, an amplitude of 63° 31′. To Russia's share fell 29° 24′, while the other countries had their work finished. In addition to this, Russia at this time had only a few triangles suitably situated that were sufficiently accurate to form a part of this arc; therefore it was necessary to revise some of the former work and to add to it much that was wholly new. In the prosecution of this work many obstacles were met with, especially while traversing the marshes of Minsk, where, on account of the heavy timber and the flat character of the ground, it was necessary to build high signals, in some cases as much as 150 feet in height.

The field operations were completed in 1872. One can form an idea of the magnitude of this triangulation when it is said that in Russia there are 321 triangles, of which 199 are taken from Tenner's nets in Poland and along the Volga, while 122 were measured by General Zilinsky especially for this arc. They rest on seven base lines, two in Tenner's chain and five in the eastern part. Fifteen astronomical stations have been occupied for longitude determinations, chiefly by Russian officers, although six points were in other countries; these were: Breslau, Leipzig, Bonn, Newport, Greenwich, and Haversfordwest. Time observations were made with portable transit instruments, and latitudes were ascertained from observations made with the vertical circles of Repsold. For the transmission of time, telegraphic signals consisting of the turning aside of the needle of a galvanoscope were employed. Between two complete determinations of time four groups of twelve signals each were sent at irregular intervals of time, varying from 13 to 17 seconds. Six repetitions of such a set constituted a longitude determination.

At the present time the computations are in press, forming parts of volumes 46 and 47 of the Memoirs of the Topographic Section of the General Staff. We are fortunately able to give the final results, as follows:

Stations.	Geodetic diff. of longitude.			Astronomic diff. of longitude.			Diff.	Arc of fifty-second parallel in metres.
	°	′	″	°	′	″	″	
Chenstohow—Warsaw	1	53	57.77	1	54	8.85	+11.08	131,854.1
Warsaw—Grodno	2	48	10.12	2	48	3.45	— 6.67	192,501.4
Grodno—Bobruisk	5	23	38.38	5	23	46.50	+ 8.12	370,468.1
Bobruisk—Orel	6	50	14.77	6	50	23.70	+ 8.93	469,605.9
Orel—Lipetzk	3	32	24.02	3	32	18.15	— 5.87	243,027.2
Lipetzk—Saratov	6	26	12.99	6	26	25.35	+12.36	441,906.5
Saratov—Samara	4	2	34.94	4	2	21.60	—13.34	277,561.2
Samara—Orenburg	5	1	27.02	5	1	35.85	+ 8.83	344,917.6
Orenburg—Orsk	3	27	23.22	3	26	47.70	—35.52	237,290.8
Chenstohow—Orsk	39	26	3.23	39	25	51.15	—12.08	2,709,132.8

In 1816, was begun the general triangulation of Russia which was to serve as the basis of accurate maps. At first the operations in different sections were isolated, and when connections were made discrepancies were discovered. This suggested to General Schubert, at the time chief of triangulation in the province of St. Peterburgh, that a central department having charge of all astronomic, geodetic, topographic, and cartographic work should be established. His proposal was favorably received by the authorities, and in 1822, the Military Topographic Corps was founded with Schubert at its head. At the same time was organized the Topographic School, where young men could prepare themselves for service in the corps. That the founder showed great wisdom in forming his plan of organization is apparent from the fact that but few changes have taken place up to the present time.

This institution is charged with all operations looking towards the complete mapping of all Russian possessions. These in a great part lie in inhospitable climes, and many are the abode of deadly fevers or savage hordes, so that the work is of surpassing difficulty. All this, however, has delayed but not deterred the determined observers, so that at the present time nearly all Russia is provided with a secondary triangulation suitable for cartographic operations. In this work the only important feature introduced was in the measurement of base lines by means of wires. This method, known in Europe as the Jäderin apparatus, consists of a pair of tapes of different metals, usually one brass and one steel, each 25 metres long. In measuring both are used side by side and are stretched under the action of a constant tension. Two sliding scales attached to the top of a tripod are adjusted so that the zero mark on one coincides with the end of the brass wire and the zero of the other coincides with the end of the steel wire. Then the wires are carried forward and the rear end of the brass wire is brought into coincidence with the zero of the scale which had been adjusted to its front end, and the same adjustment is made for the steel wire. If the two wires should remain equal in length there would be no disagreement in the zero marks, but as the rates of expansion of these two metals are widely different the distance between the zeros at the first laying of the wires is due to their unequal expansion, and each time the wires are put in place this distance is augmented or diminished according as the temperature is continually increasing or decreasing. From this it can be seen that the entire base line can be regarded as measured by a single length of an apparatus constructed on the Borda principle and at a temperature equal to the mean temperature experienced in measuring. With these wires great speed can be attained, reaching as much as 8 kilometres a day, and judging from the Moloskowizy base, where the discrepancy between two measures was only 1 centimeter in a base 9,822 metres, sufficient accuracy is readily secured.

Not only for the purpose of determining the amplitude of arcs of par-

allel, but also for locating or correcting the location of points distant from fixed observatories, was it early necessary to ascertain differences of longitude. The first step in this direction was made in 1833, when fifty-six chronometers were transported in the steamer *Hercules* to points along the shores of the Baltic Sea. This was followed by several large or primary expeditions, fixing points from which smaller or secondary expeditions radiated as from centers. The most important of these is the well-known expedition carried on under the direction of Struve, for determining the difference of longitude between Greenwich and Pulkova. The next was between Pulkova and Moscow, with forty chronometers. During these exchanges a great number of box chronometers were transported in carriages, and it was found that in a good spring vehicle, even over bad roads, the rate of the chronometers were as constant as when they were carried by water. In the frequent expeditions following these, when no less than eighty chronometers were employed, observations and comparisons were made not only at the terminal points, but also at several intermediate stations. The great number of chronometers in use made it necessary to find some means of lessening the time necessary for their comparison. When, as was at first the case, siderial and mean-time chronometers were compared, 4 minutes were lost while waiting for a coincidence. As the outcome of this necessity Struve invented the thirteen striker, that is, a chronometer making thirteen beats or strokes in 6 seconds. This gives, whether comparing with a star or mean chronometer, a coincidence every 6 seconds within a range of $0''.02$, which is sufficiently accurate. An uncompensated chronometer always formed a part of the equipment, serving as a means for finding the temperature coefficients of the compensated chronometers more satisfactorily than if temperatures were taken from accompanying thermometers. As one would expect, the Russians have made very elaborate investigations regarding the rates of chronometers and their disturbing causes.

As soon as Russia was covered with a telegraphic net the new method of determining difference of longitudes was tried and at once adopted The first application of this scheme was in Finland, between the stations Cronstadt and Uleåborg. This was in 1860, and since that time each year has witnessed at least one new determination. In 1868 observations were made for finding the longitudes of Wiborg, Lovisa, Helsingfors, and Ålbo with reference to Pulkova. In these operations there was used for the first time the method of finding time by a transit instrument set in the vertical of Polaris. This method had been known for a long time, but had not been used because of the complicated computations involved. But W. Dällen, of Pulkova, gave formulæ and tables which made it possible to compute the correction of the clock almost as quickly as if the observations were made in the meridian.

The greatest undertaking in the way of telegraphic longitudes are the labors of Shamgorst and Kulberg, who, in 1873–'76, gave a series of

points from Moscow to Vladivostak, covering Siberia and embracing arcs having a total amplitude of more than 100°. This huge undertaking had two objects in view: to give the exact position of a number of stations which were to serve as the bases of numerous smaller operations, especially chronometric expeditions, and to determine in the most accurate maner the longitude of stations where observations of the transit of Venus were to be made in 1874. The observations were made with portable transit instruments specially adapted for quick and convenient shifting in azimuth, making it possible to readily place the instrument in the vertical of Polaris. For latitudes these same instruments were used, being placed in the prime vertical. The account of this expedition takes up nearly the whole of the thirty-eighth volume of the Memoirs of the Topographical Section of the General Staff. Upon examination it is found that the latitudes were affected with a probable error of $0''.1$, while the probable error of a longitude determination is $0''.043$. From the successive transmission of time backwards and forwards the velocity of the galvanic current was found to be 93,548 kilometres per second.

While the triangulation was in progress, zenith-distances were observed from which the heights of stations were completed, but these operations have not been consistently followed out, so that there are in many parts of Russia a lack of well-determined altitudes. General Tenner gave due attention to this special werk, and in his chain he united the Baltic and the Black Seas. His results showed that the former is 0.53 fathom higher, but as the probable error is 1.5 fathoms but little confidence was placed in the theory that there was any difference in the level of these two seas. But with the Caspian Sea a different state of affairs was supposed to exist. It had been suspected that this sea was lower than either of the two just named, so in 1836–'37, a large expedition was organized, in which Fuss, Sawitch, and Sabler were participants. They began at Kagalnik near the Asov Sea, crossed the northern portion of the Caucasian deserts to the Tschornoi Rynok on the Caspian. For greater accuracy the zenith distances were measured at very short distances, approximately 3.5 versts. These distances were ascertained by computation from short lines measured by placing bars end to end on a rope stretched tight. The results, published in 1849, showed that the Caspian Sea is 85.45 feet lower than the Black Sea. Subsequently almost the same value was obtained, but still later a value 4 feet greater was found, suggesting that the level of the Caspian is decreasing. This fact has had further demonstration. The academician Lenz made a mark on a rock near the town of Baku exactly on a level with the sea; this mark in 1861 was 3.93 feet higher than the water, and more recent comparisons show that the difference is increasing.

The other Russian interior sea, the Aral, is, on the contrary, higher than the level of the ocean. The special levelling party sent out for

this purpose in 1874, came to the conclusion that the Aral Sea is higher than the Caspian by 243 feet.

In 1871, systematic spirit leveling was begun, and in its prosecution many interesting facts have been brought to light. One of these is the different levels of the water in the Baltic Sea. Taking 0 for the level of water at Cronstadt, the height of the sea level proves to be:

	Metres.
At Revol	— 0.57
At Dinaminde	— 0.88
At Libau	— 1.24

Another is the discrepancy between spirit levelling and geodetic levelling in obtaining the elevation of the threshold of the Dorpat Observatory. This amounts to nearly 4 metres, and is suggestive of a considerable local disturbance.

The first local attraction observed in Russia was in the neighborhood of Moscow, where, owing to the absence of hills, one might least expect a discrepancy between geodetic and astronomic results. Soon after the completion of the triangulation in the province of Moscow this deflection attracted public attention, and the astronomer Schweizer undertook a special investigation. The result showed that in this province, almost in the direction from east to west, there is a strip along whose northern boundary there is a considerable (8″) northern deflection, and on the southern border a southerly deflection of 10″. It is supposed that along this belt there must be a vast extent of matter of comparatively small density, or underlying it great cavities.

The most elaborate investigation of local deflection of the plumb-line was made in the Caucasus by General Stebnizki and published in the Memoirs of the St. Petersburg Academy of Sciences for 1870. From the analysis of the astronomic and trigonometric operations executed on both sides of the principal Caucasian ridge it became evident that, in general, to the north of the mountains there exists a deflection to the south and on the south an opposite deflection. The greatest discrepancies in the astronomic and geodetic latitudes proved to be in Vladikaukasus, −35″.76; in Alexandrovskaja, −18″.14; in Petrovk, −18″.56; and in Dushet, +18″.29. Availing himself of the surveys already executed furnishing a great number of very accurately determined points, General Stebnizki computed the effect which the attraction of the exterior mountainous mass would have upon the astronomic latitude of the different stations. In these computations no attracting mass was considered which was distant more than 240 versts, while the chief disturbing causes were frequently found to lie within a circle with a radius of 80 versts, the station occupying the central point. It was found that the greater part of the noted discrepancies were sufficiently accounted for by the law of attraction having regard to the exterior mass alone. In the cases just cited the computed differences reduced the station errors to 3″, −1′.31, +2″.15 and −0″.86. But there are other

stations where the computed attraction is either insufficient for the explanation of the observed discrepancy or even contradicts it. Among such stations the following are remarkable:

	Tiflis.	Elisabetpol.	Shemaha.
	″	″	″
The observed deflection	−7.56	−32.75	−23.21
The computed deflection	+2.41	−20.50	+16.43
Difference	−9.97	−12.25	−39.64

As all of these stations lie approximately on the same parallel, and each showed a strong deflection to the south, there must lie to the south under the surface of the earth an extent of matter of great density, or to the north under the Caucasian ridge a mass of less density. The latter hypothesis has found a parallel in the deflections observed near the foot of the Himalaya Mountains. Besides the latitude deflections, General Stebnizki calculated the deflections of the vertical at longitude stations, but their number so far is insufficient to serve as a basis for generalization.

For more than a century, the pendulum has been regarded in Russia as a geodetic instrument of great value, but no very accurate observations were made prior to 1826–'29, when Captain Lutke made a cruise around the world on the man-of-war *Seniavin*. He swung a Kater pendulum at ten stations. The results, published in 1833, gave for the ellipticity 1 : 267.8, or 1 : 269 if two somewhat doubtful stations are disregarded. Besides the desultory observations of Professor Parrot of Dorpat in 1829, nothing of consequence was attempted until 1865–'68, when the Academy of Sciences of St. Petersburg sent out an expedition in charge of Sawitch, Smyslow, and Lenz. They selected twelve stations along the great Russian meridional arc (Tornea, Nicolaistad, St. Petersburg, Reval, Dorpat, Jakobstadt, Vilna, Belin, Kremenetz, Kishener, Kamenetz and Ismail), and employed a reversible Repsold pendulum. The results 1 : 309 for the ellipticity of the earth.

Since this time, many observations have been made in various portions of the Russian domain, and with the pendulum work, as with all other branches of geodetic operations, the best methods soon find a place, and results are obtained that are comparable with those of any country.

QUARTZ FIBERS*

By C. V. Boys, F. R. S.

I.

In almost all investigations which the physicist carries out in the laboratory, he has to deal with and to measure with accuracy those subtle and to our senses inappreciable forces to which the so-called laws of nature give rise. Whether he is observing by an electrometer the behavior of electricity at rest, or by a galvanometer the action of electricity in motion; whether in the tube of Crookes he is investigating the power of radiant matter, or with the famous experiment of Cavendish he is finding the mass of the earth—in these and in a host of other cases he is bound to measure with certainty and accuracy forces so small that in no ordinary way could their existence be detected; while disturbing causes which might seem to be of no particular consequence must be eliminated if his experiments are to have any value. It is not too much to say that the very existence of the physicist depends upon the power which he possesses of producing at will and by artificial means forces against which he balances those that he wishes to measure.

I had better perhaps at once indicate in a general way the magnitude of the forces with which we have to deal.

The weight of a single grain is not to our senses appreciable, while the weight of a ton is sufficient to crush the life out of anyone in a moment. A ton is about 15,000,000 grains. It is quite possible to measure with unfailing accuracy forces which bear the same relation to the weight of a grain that a grain bears to a ton.

To show how the torsion of wires or threads is made use of in measuring forces, I have arranged what I can hardly dignify by the name of an experiment. It is simply a straw hung horizontally by a piece of wire. Resting on the straw is a fragment of sheet-iron weighing 10 grains. A magnet so weak that it can not lift the iron yet is able to pull the straw round through an angle so great that the existence of the feeble attraction is evident to everyone in the room.

Now it is clear that if, instead of a straw moving over the table simply,

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*Lecture delivered at the Royal Institution, on Friday, June 14, 1889. (From *Nature*, July 11, 1889, and October 16, 1890, vols. XL., pp. 247–251, and XLII, pp. 604–608.)

we had here an arm in a glass case and a mirror to read the motion of the arm, it would be easy to observe a movement a hundred or a thousand times less than that just produced, and therefore to measure a force a hundred or a thousand times less than that exerted by this feeble magnet.

Again, if instead of wire as thick as an ordinary pin I had used the finest wire that can be obtained, it would have opposed the movement of the straw with a far less force. It is possible to obtain wire ten times finer than this stubborn material, but wire ten times finer is much more than ten times more easily twisted. It is ten thousand times more easily twisted. This is because the torsion varies as the fourth power of the diameter, so we say $10 \times 10 = 100$; $100 \times 100 = 10{,}000$. Therefore with the finest wire, forces 10,000 times feebler still could be observed.

It is therefore evident how great is the advantage of reducing the size of a torsion wire. Even if it is only halved the torsion is reduced sixteen-fold. To give a better idea of the actual sizes of such wires and fibers as are in use I shall show upon the screen a series of photographs taken by Mr. Chapman, on each of which a scale of thousandths of an inch has been printed.

Scale of 1000ths of an inch for Figs. 1 to 7. The scale of Figs. 8 and 9 is much finer.

Fig. 1.

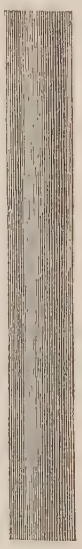

Fig. 2.

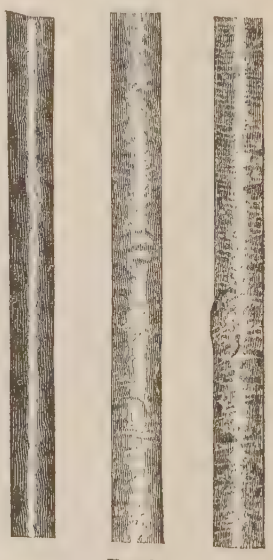

Fig. 3.

The first photograph (Fig. 1) is an ordinary hair—a sufficiently familiar object, and one that is generally spoken of as if it were rather fine. Much finer than this is the specimen of copper wire now on the screen

(Fig. 2), which I recently obtained from Messrs. Nalder Brothers. It is only a little over one-thousandth of an inch in diameter. Ordinary spun glass, a most beautiful material, is about one-thousandth of an inch in diameter, and this would appear to be an ideal torsion thread (Fig. 3). Owing to its fineness its torsion would be extremely small, and the more so because glass is more easily deformed than metals. Owing to its very great strength, it can carry heavier loads than would be expected of it. I imagine many physicists must have turned to this material in their endeavor to find a really delicate torsion thread. I have so turned only to be disappointed. It has every good quality but one, and that is its imperfect elasticity. For instance, a mirror hung by a piece of spun glass is casting an image of a spot of light on the scale. If I turn the mirror, by means of a fork, twice to the right, and then turn it back again, the light does not come back to its old point of rest, but oscillates about a point on one side, which however is slowly changing, so that it is impossible to say what the point of rest really is. Further, if the glass is twisted one way first, and then the other way, the point of rest moves in a manner which shows that it is not influenced by the last deflection alone; the glass remembers what was done to it previously. For this reason spun glass is quite unsuitable as a torsion thread; it is impossible to say what the twist is at any time, and therefore what is the force developed.

So great has the difficulty been in finding a fine torsion thread that the attempt has been given up, and in all the most exact instruments silk has been used. The natural cocoon fibers, as shown on the screen (Fig. 4), consist of two irregular lines gummed together, each about one two-thousandth of an inch in diameter. These fibers must be separated from one another and washed. Then each component will, according to the experiment of Gray, carry nearly 60 grains before breaking, and can be safely loaded with 15 grains. Silk is therefore very strong, carrying at the rate of from 10 to 20 tons to the square inch. It is further valuable in that its torsion is far less than that of a fiber of the same size of metal or even of glass, if such could be produced. The torsion of silk, though exceedingly small, is quite sufficient to upset the working of any delicate instrument, because it is never constant. At one time the fiber twists one way, and another time in another, and the evil effect can only be mitigated by using large apparatus in which strong forces are developed. Any attempt that may be made to increase the delicacy of apparatus by reducing their dimensions is at once prevented by the relatively great importance of the vagaries of the silk suspension.

Fig. 4.

The result then is this. The smallness, the length of period, and therefore delicacy, of the instruments at the

physicist's disposal have until lately been simply limited by the behavior of silk. A more perfect suspension means still more perfect instruments, and therefore advance in knowledge.

It was in this way that some improvements that I was making in an instrument for measuring radiant heat came to a dead-lock about 2 years ago. I would not use silk, and I could not find anything else that would do. Spun glass even, was far too coarse for my purpose; it was a thousand times too stiff.

There is a material invented by Wollaston long ago, which however I did not try because it is so easily broken. It is platinum wire which has been drawn in silver, and finally separated by the action of nitric acid. A specimen about the size of a single line of silk is now on the screen, showing the silver coating at one end (Fig. 5).

As nothing that I knew of could be obtained that would be of use to me, I was driven to the necessity of trying by experiment to find some new material. The result of these experiments was the development of a process of almost ridiculous simplicity which it may be of interest for me to show.

FIG. 5.

The apparatus consists of a small cross-bow, and an arrow made of straw with a needle point. To the tail of the arrow is attached a fine rod of quartz which has been melted and drawn out in the oxy-hydrogen jet. I have a piece of the same material in my hand, and now after melting their ends and joining them together, an operation which produces a beautiful and dazzling light, all I have to do is to liberate the string of the bow by pulling the trigger with one foot, and then if all is well a fiber will have been drawn by the arrow, the existence of which can be made evident by fastening to it a piece of stamp paper.

In this way threads can be produced of great length, of almost any degree of fineness, of extraordinary uniformity, and of enormous strength. I do not believe, if any experimentalist had been promised by a good fairy that he might have anything he desired, that he would have ventured to ask for any one thing with so many valuable properties as these fibers possess. I hope in the course of this evening to show that I am not exaggerating their merits.

In the first place, let me say something about the degree of fineness to which they can be drawn. There is now projected upon the screen a quartz fiber one five-thousandth of an inch in diameter (Fig. 6). This is one which I had in constant use in an instrument loaded with about 30 grains. It has a section only one-sixth of that of a single

line of silk, and it is just as strong. Not being organic, it is in no way affected by changes of moisture and temperature, and so it is free from the vagaries of silk which give so much trouble. The piece used in the instrument was about 16 inches long. Had it been necessary to employ spun glass, which hitherto was the finest torsion material, then, instead of 16 inches, I should have required a piece 1,000 feet long, and an instrument as high as the Eiffel tower to put it in.

FIG. 6. FIG. 7.

There is no difficulty in obtaining pieces as fine as this, yards long if required, nor in spinning it very much finer. There is upon the screen a single line made by the small garden spider, and the size of this is perfectly evident (Fig. 7). You now see a quartz fiber far finer than this, or, rather, you see a diffraction phenomenon, for no true image is formed at all; but even this is a conspicuous object in comparison with the tapering ends, which it is absolutely impossible to trace in a microscope. The next two photographs, taken by Mr. Nelson, whose skill and resources are so famous, represent the extreme end of a tail of quartz, and though the scale is a great deal larger than that used in the other photographs, the end will be visible only to a few. Mr. Nelson has photographed here what it is absolutely impossible to see. What the size of these ends may be I have no means of telling. Dr. Royston Piggott has estimated some of them at less than one-millionth of an inch, but whatever they are they supply for the first time objects of extreme smallness the form of which is certainly known, and therefore I can not help looking upon them as more satisfactory tests for the microscope than diatoms and other things of the real shape of which we know nothing whatever.

Since figures as large as a million can not be realized properly, it may be worth while to give an illustration of what is meant by a fiber one-millionth of an inch in diameter.

A piece of quartz an inch long and an inch in diameter would, if drawn out to this degree of fineness, be sufficient to go all the way round the world 658 times; or a grain of sand just visible—that is, one-hundredth of an inch long and one-hundredth of an inch in diameter—would make 1,000 miles of such thread. Further, the pressure inside such a thread due to a surface tension equal to that of water would be 60 atmospheres.

Going back to such threads as can be used in instruments, I have made use of fibers one ten-thousanth of an inch in diameter, and in these the torsion is 10,000 times less than that of spun glass.

As these fibers are made finer their strength increases in proportion

to their size, and surpasses that of ordinary bar steel, reaching, to use the language of engineers, as high a figure as 80 tons to the inch. Fibers of ordinary size have a strength of 50 tons to the inch.

While it is evident that these fibers give us the means of producing an exceedingly small torsion, and one that is not affected by weather, it is not yet evident that they may not show the same fatigue that makes spun glass useless. I have therefore a duplicate apparatus with a quartz fiber, and you will see that the spot of light comes back to its true place on the screen after the mirror has been twisted round twice.

I shall now for a moment draw your attention to that peculiar property of melted quartz that makes threads such as I have been describing a possibility. A liquid cylinder, as Plateau has so beautifully shown, is an unstable form. It can no more exist than can a pencil stand on its point. It immediately breaks up into a series of spheres. This is well illustrated in that very ancient experiment of shooting threads of resin electrically. When the resin is hot, the liquid cylinders which are projected in all directions break up into spheres, as you see now upon the screen. As the resin cools they begin to develop tails; and when it is cool enough, *i. e.*, sufficiently viscous, the tails thicken, and the beads become less, and at last uniform threads are the result. The series of photographs show this well.


Fig. 8.

There is a far more perfect illustration which we have only to go into the garden to find. There we may see in abundance what is now upon the screen—the webs of those beautiful geometrical spiders. The radial threads are smooth, like the one you saw a few minutes ago, but the threads that go round and round are beaded. The spider draws these webs slowly, and at the same time pours upon them a liquid, and still further to obtain the effect of launching a liquid cylinder in space he, or rather she, pulls it out like the string of a bow, and lets it go with a jerk. The liquid cylinder can not exist, and the result is what you now see upon the screen (Fig. 8). A more perfect illustration of the regular breaking up of a liquid cylinder it would be impossible to find. The beads are, as Plateau showed they ought to be, alternately large and small, and their regularity is marvellous. Sometimes two still smaller beads are developed, as may be seen in the second photograph, thus completely agreeing with the results of Plateau's investigations.

I have heard it maintained that the spider goes round her web and places these beads there afterwards. But since a web with about 360,000 beads is completed in an hour—that is, at the rate of about 100 a second—this does not seem likely. That what I have said is true, is made more probable by the photograph of

a beaded web that I have made myself by simply stroking a quartz fiber with a straw wetted with castor oil (Fig. 9). It is rather larger than a spider line; but I have made beaded threads, using a fine fiber, quite indistinguishable from a real spider web, and they have the further similarity that they are just as good for catching flies.

FIG. 9.

Now, going back to the melted quartz, it is evident that if it ever became perfectly liquid it could not exist as a fiber for an instant. It is the extreme viscosity of quartz, at the heat even of an electric arc, that makes these fibers possible. The only difference between quartz in the oxy-hydrogen jet, and quartz in the arc, is that in the first you make threads and in the second are blown bubbles. I have in my hand some microscopic bubbles of quartz showing all the perfection of form and color that we are familiar with in the soap bubble.

An invaluable property of quartz is its power of insulating perfectly, even in an atmosphere saturated with water. The gold leaves now diverging were charged some time before the lecture, and hardly show any change, yet the insulator is a rod of quartz only three-quarters of an inch long, and the air is kept moist by a dish of water. The quartz may even be dipped in the water and replaced with the water upon it without any difference in the insulation being observed.

Not only can fibers be made of extreme fineness, but they are wonderfully uniform in diameter. So uniform are they that they perfectly stand an optical test so severe that irregularities invisible in any microscope would immediately be made apparent. Everyone must have noticed when the sun is shining upon a border of flowers and shrubs how the lines which spiders use as railways to travel from place to place glisten with brilliant colors. These colors are only produced when the fibers are sufficiently fine. If you take one of these webs and examine it in the sunlight, you will find that the colors are variegated, and the effect consequently is one of great beauty.

A quartz fiber of about the same size shows colors in the same way, but the tint is perfectly uniform on the fiber. If the color of the fiber is examined with a prism, the spectrum is found to consist of alternate bright and dark bands. Upon the screen are photographs taken by Mr. Briscoe, a student in the laboratory of South Kensington, of the spectra of some of these fibers at different angles of incidence. It will be seen that coarse fibers have more bands than fine, and that the number increases with the angles of incidence of the light. There are peculiarities in the march of the bands as the angle increases which I can not describe now. I may only say that they appear to move not uniformly but in waves, presenting very much the appearance of a caterpillar walking.

So uniform are the quartz fibers that the spectrum from end to end consists of parallel bands. Occasionally a fiber is found which presents a slight irregularity here and there. A spider line is so irregular that these bands are hardly observable; but as the photograph on the screen shows, it is possible to trace them running up and down the spectrum when you know what to look for.

To show that these longitudinal bands are due to the irregularities, I have drawn a taper piece of quartz by hand, in which the two edges make with one another an almost imperceptible angle, and the spectrum of this shows the gradual change of diameter by the very steep angle at which the bands run up the spectrum.

Into the theory of the development of these bands I am unable to enter; that is a subject upon which your professor of natural philosophy is best able to speak. Perhaps I may venture to express the hope, as the experimental investigation of this subject is now rendered possible, that he may be induced to carry out a research for which he is so eminently fitted.

Though this is a subject which is altogether beyond me, I have been able to use the results in a practical way. When it is required to place into an instrument a fiber of any particular size, all that has to be done is to hold the frame of fibers toward a bright and distant light, and look at them through a low-angled prism. The banded spectra are then visible, and it is the work of a moment to pick out one with the number of bands that has been found to be given by a fiber of the desired size. A coarse fiber may have a dozen or more, while such fibers as I find most useful have only two dark bands. Much finer ones exist, showing the colors of the first order with one dark band; and fibers so fine as to correspond to the white or even the gray of Newton's scale are easily produced.

Passing now from the most scientific test of the uniformity of these fibers, I shall next refer to one more homely. It is simply this: the common garden spider, except when very young, can not climb up one of the same size as the web on which she displays such activity. She is perfectly helpless, and slips down with a run. After vainly trying to make any headway, she finally puts her hands (or feet) into her mouth, and then tries again, with no better success. I may mention that a male of the same species is able to run up one of these with the greatest ease, a feat which may perhaps save the lives of a few of these unprotected creatures when quartz fibers are more common.

It is possible to make any quantity of very fine quartz fiber without a bow and arrow at all, by simply drawing out a rod of quartz over and over again in a strong oxyhydrogen jet. Then, if a stand of any sort has been placed a few feet in front of the jet, it will be found covered with a maze of thread, of which the photograph on the screen represents a sample. This is hardly distinguishable from the web spun

by this magnificent spider in corners of greenhouses and such places. By regulating the jet and the manipulation, anything from one of these stranded cables to a single ultra-microscope line may be developed.

And now that I have explained that these fibers have such valuable properties, it will no doubt be expected that I should perform some feat with their aid which, up to the present time, has been considered impossible, and this I intend to do.

Of all experiments the one which has most excited my admiration is the famous experiment of Cavendish, of which I have a full-size model before you. The object of this experiment is to weigh the earth by comparing directly the force with which it attracts things with that due to large masses of lead. As is shown by the model, any attraction which these large balls exert on the small ones will tend to deflect this 6-foot beam in one direction, and then if the balls are reversed in position the deflection will be in the other direction. Now, when it is considered how enormously greater the earth is than these balls, it will be evident that the attraction due to them must be in comparison excessively small. To make this evident the enormous apparatus you see had to be constructed, and then, using a fine torsion wire, a perfectly certain but small effect was produced. The experiment however could only be successfully carried out in cellars and underground places, because changes of temperature produced effects greater than those due to gravity.*

Now I have—in a hole in the wall—an instrument no bigger than a galvanometer, of which a model is on the table. The balls of the Cavendish apparatus, weighing several hundredweight each, are replaced by balls weighing 1¾ pounds only. The smaller balls of 1¾ pounds are replaced by little weights of 15 grains each. The 6-foot beam is replaced by one that will swing round freely in a tube three-quarters of an inch in diameter. The beam is, of course, suspended by a quartz fibre. With this microscopic apparatus, not only is the very feeble attraction observable, but I can actually obtain an effect eighteen times as great as that given by the apparatus of Cavendish, and, what is more important, the accuracy of observation is enormously increased.

The light from a lamp passes through a telescope lens and falls on the mirror of the instrument. It is reflected back to the table, and thence by a fixed mirror to the scale on the wall, where it comes to a focus. If the mirror on the table were plane, the whole movement of the light would be only about 8 inches, but the mirror is convex, and this magnifies the motion nearly eight times. At the present moment the attracting weights are in one extreme position, and the line of light is quiet. I will now move them to the other position, and you will see the result—the light slowly begins to move, and slowly increases in

* Dr. Lodge has been able, by an elaborate arrangement of screens, to make this attraction just evident to an audience.—C. V. B.

movement. In 40 seconds it will have acquired its highest velocity, and in 40 more it will have stopped at 5 feet 8½ inches from the starting point, after which it will slowly move back again, oscillating about its new position of rest.

It is not possible at this hour to enter into any calculations; I will only say that the motion you have seen is the effect of a force of less than one ten-millionth of the weight of a grain, and that with this apparatus I can detect a force two thousand times smaller still. There would be no difficulty even in showing the attraction between two No. 5 shot.

And now in conclusion, I would only say that if there is anything that is good in the experiments to which I have this evening directed your attention, experiments conducted largely with sticks and string and straw and sealing-wax, I may perhaps be pardoned if I express my conviction that in these days we are too apt to depart from the simple ways of our fathers, and instead of following them, to fall down and worship the brazen image which the instrument-maker hath set up.

II.*

Before I enter upon the subject upon which I have to address you, I wish to point out that, quite apart from any deficiency on my part which will be only too apparent in the course of the evening, it is my intention to commit two faults which may well be considered unpardonable. In the first place, I shall speak entirely about my own experiments, even though I know that the iteration of the first personal pronoun for the space of one hour is apt to be as monotonous to an audience as it is wanting in taste on the part of a lecturer. In the second place, I am going almost to depend upon the motions of a spot of light to illustrate the actions which I shall have to describe, in spite of the fact that it is impossible for an audience to get up any enthusiasm when watching the wandering motion of a spot of light the result of the manipulation of a mystery box, of which it is impossible to see the inside. These however are faults which are the immediate consequence of the nature of my subject.

Physicists deal very largely with the measurement of extremely minute forces, which it is of the utmost importance that they should be able to measure accurately. Now, forces may be considered under two aspects. It may be that the force which is developed and which has to be measured is a twist, in which case the twisting force may be applied to the end of a wire directly, when the amount through which that wire is twisted is a measure of the twisting force. Or the force may be a direct pull or a push, which may also be measured by the twist of a wire if it is applied to the end of a lever or arm carried by the wire.

* Lecture delivered on September 8, 1890, at the Leeds meeting of the British Association.

Now supposing that the force—whether of the nature of a twist or of a pull (it does not matter which)—is too small to produce an appreciable twist in the wire, it is obvious that a finer wire must be employed, but it is not obvious how much more easily a fine wire is twisted than a coarse one. If the fine wire is one-tenth of the diameter of the coarse one, we must multiply ten by itself four times over in order to find how much more easily twisted it is, and thus obtain the enormous number 10,000; it is 10,000 times more easily twisted than the coarse one. Thus there is an enormous advantage in increasing the minuteness of the wire by means of which feeble twisting or pulling forces are measured. But if the delicacy of the research is such that even the finest wire which can be made is still too stiff, then, even though with such wire, which is somewhere about the thousandth of an inch in diameter, forces as small as the millionth part of the weight of a single grain can be detected with certainty, the wire is of no use; and as wire can not be made finer, some other material must be used. Spun glass is fine and strong, and is still more easily twisted than the finest wire, but it possesses a property somewhat analogous to putty. When it has been twisted and then let go, it does not come back to its old place, so that though it is much more largely twisted than wire by the application of a force, it is not possible with accuracy to measure that force. There is, or rather I should say there was, no material that could be used as a torsion thread finer than spun glass; and therefore physicists use instead a fiber almost free from torsion. A single thread of silk as spun by the silkworm is taken and split down the middle, for it is really double, and one-half only is used. This is far finer than spun glass, and being softer in texture, it is much more easily twisted. Silk is ten thousand times more easily twisted than spun glass. So easily twisted is silk that in the majority of instruments the stiffness of the silk is either of no consequence at all, or at any rate it only produces but the slightest disturbing effect. Now, if it is necessary to push the investigation further still by the continued increase in the delicacy of the apparatus, silk itself begins to prevent any progress. Silk has a certain stiffness, but if that were always the same it would not matter; but then it possesses that putty-like character of spun glass, but in a far higher degree; it is affected by every variation of temperature and moisture, and any really delicate measures are out of the question when silk is used as the suspending fiber.

This, I believe, is a fairly accurate account of the state of the case, three years ago. At that time I was improving, or attempting to improve, a certain class of apparatus of which I shall have more to say presently, and I was met by the difficulty that a greater degree of delicacy was required than was possible with existing torsion threads. Silk would have entirely prevented me from reaching the degree of delicacy and certainly in this instrument that I hope to show this evening that I have attained.

Being then in this difficulty I was by good fortune and necessity led to devise a process which I propose at once to show you. I shall not describe the preliminary experiments, but simply describe the process as it stands. There is a small cross-bow held in a vice, and a little arrow made of straw with a needle point, and I have here a fragment of rock crystal which has been melted and drawn into a rod. It requires a temperature greater than that developed in any furnace to melt this material so that it may be drawn out. If the arrow, which also carries a piece of the quartz rod, is placed in the bow, and if both pieces are heated up to the melting point and joined together, and then the arrow is shot, a fiber of quartz is drawn,—that is to say, it is drawn if there is not an accident.

The arrow has flown, and there is now a fiber not very fine this time, which I shall hand to our president. At the same time I can pass him a piece of much finer fiber, made this afternoon, which shows (and this is a proof of its fineness) all the brilliant colors of the spider line when the sun shines upon it, but with a degree of magnificence and splendor which has never been seen on any natural object.

The main features of these fibers are these. You can make them as fine as you please; you can make them of very considerable length; you can make pieces 40 or 50 feet long, without the slightest trouble, at almost every shot. Even though of that great length, they are very uniform in diameter from end to end, or at any rate the variation is small and perfectly regular. The strength of the fiber is, I think I may safely say, something astonishing. Fibers such as I have in use at the present time in an instrument behind me are stronger than ordinary bar steel; they carry from 60 to 80 tons to the square inch. That is one of their most important features, for this reason,—that on account of their enormous strength you can make use of very much finer fibers than would be possible if they were not so strong; and I have already explained the importance of the fineness of the fiber when delicacy is of the first importance.

As to the diameter of these fibers, I have said they can be made as fine as you please. I shall not trouble you with a large number of figures, but one or two may probably be interesting to those who are in the habit of using philosophical apparatus. In the first place, a fiber a great deal finer that a single fiber of silk (that is, one five-thousandth of an inch in diameter), will carry an apparatus more than 30 grains in weight. I have in one of the pieces of apparatus which I shall use presently, a fiber the fifteen-thousandth of an inch in diameter. That is, so fine that if you were to take a hundred of them and twist them into a bundle you would produce a compound cable of the thickness of a single silkworm's thread. I do not mean the silk used for sewing that is wound on a reel, because that is composed of an enormous number of silk threads; but a single silkworm's thread as it is wound from the

cocoon, and that fiber is at the present time carrying a mirror the movements of which will presently be visible in all parts of this large room.

But that is by no means the limit of the degree of fineness which can be reached. A fiber the fifteen-thousandth of an inch in thickness is quite a strong and conspicuous object. You may go on making them until you can not see them with the naked eye. You may go on following them with the microscope until you can not see them with the microscope—that is to say, you can not find their end,—they gradually *go out.* The ends are so fine that it is impossible ever to see them in any microscope that can be constructed, not because the microscopes are bad, but because of the nature of light. But that is a point upon which I shall not say more this evening. It has been estimated that probably the ends of some of these are as fine as the millionth part of an inch—I do not care whether they are or whether they are not, because they can never be seen and never be used—but certainly the hundred-thousandth of an inch is by no means beyond the limit which can be obtained. As these large numbers of hundreds of thousands and millions are figures which it is impossible for anybody thoroughly to realize, I may for the purpose of illustration say that if we were to take a piece of quartz about as big as a walnut, and if we could draw the whole of that into a thread one hundred-thousandth of an inch in diameter—threads which can certainly be produced—there would be enough to go round the world about six or seven times.

These quartz fibers, on account of their fineness, are eminently capable of measuring minute forces—that is to say, they would be capable if they were free from that putty-like quality which I have described as making spun glass useless. Now, experiments made both in this country and in Australia show that to a most extraordinary degree they are perfectly free from that one fault of spun glass.

The number of useful properties of quartz that has been melted is so great that I can merely take, in a more or less disjointed way, one or two; and I propose, in the first place, to say something which I think may be especially interesting to chemists and perhaps to our president. I should like to ask experimental chemists what they would think of a material which could be drawn into tubes, blown into bulbs, joined together in the same way that glass is joined, drawn out, attached to a Sprengel pump, sealed off with a Sprengel vacuum which would be transparent, which would be less acted upon than glass by corrosive chemicals, and which finally at the point at which platinum is as fluid as water would still retain its form. Here is such a tube with a bulb blown at the end. I have found that it is possible to make tubes (though it can not be done in the ordinary way as with glass) and to blow bulbs with quartz, and that they have this advantage which glass does not possess, namely, that it is almost impossible to crack them by the sudden application of heat.

Then there is another property which quartz fibers and rods possess

which I shall be able to show only imperfectly, namely, the power of insulating anything charged with electricity under conditions under which in general insulation is impossible. You now see upon the screen an electroscope, the leaves of which were charged at noon, and they are still divergent, but not to a very great extent, because they have suffered from unavoidable shaking during the day. The point to which I especially wish to refer is this. In electroscopes and all electrostatic apparatus one puts in a dish of sulphuric acid, (which is an abomination,) in order to keep the atmosphere dry. I have in this electroscope such a dish, but it is filled with water in order to keep the atmosphere moist. Experiments carefully made, using the same box—everything the same, except that in one case the insulating stem was made of quartz and in the second case it was made of the best flint glass well washed, of the same shape and size—show that if the atmosphere is perfectly dry the electricity escapes from both at the same rate; but that if the atmosphere is perfectly moist the electricity escapes from the leaves insulated by the clean washed flint glass only too quickly; whereas, from the leaves insulated by the quartz the rate is identically the same as it was in either case when the atmosphere was perfectly dry.

I have said that these fibers are uniform in diameter, and fine and smooth and strong, and that they glisten with all the colors of the spider web, but that they are far more brilliant. It was naturally rather a curious point to note what a spider would do if by any chance she should find herself on such a web; and now that I am dealing with live and wild animals which can not possibly be trained, the conditions are such as to render the success of an experiment entirely a matter of chance. However, I propose to make use of the spider as a test of the very great smoothness and slipperiness of one of these fibers. There are here three little spiders which have been good enough, since they came to Leeds, to spin upon these little wooden frames their perfect and beautiful geometrical webs. I have succeeded in placing one of these frames in the lantern without disturbing the spider, which you can now see waiting upon her web. I must now, without disturbing the peace of mind of the spider, carry her to a web of quartz; and therefore it is necessary that the spider should be fortunate enough to catch a fly. Now, instead of bringing a fly I will make an ordinary tuning-fork buzz against the web. She immediately pounces upon the imaginary fly, and thus I can without frightening her place her upon the quartz fiber. Unfortunately this spider has slipped and has got away, but with another I am more successful. I intended to show that the small and common garden spider could not climb the quartz fiber, but for some reason this spider is able to get up with difficulty. However I shall not spend any more time upon this experiment.

I shall now at once speak about the instrument which actually led me to the invention of the process for making quartz fibers. This,

which I have called a radio-micrometer, is an instrument of very great delicacy for measuring radiant heat from such a thing as a candle, a fire, the sun, or anything else which radiates heat through space.

The radio-micrometer which I wish to show this evening is resting upon a solid and steady beam, and as usual its index is a spot of light upon the scale. You see that that spot of light is almost perfectly steady. Now the heat that I propose to measure, or rather the influence of which I intend to show you, is the heat which is being radiated from a candle fixed in the front of the upper gallery some 70 or 80 feet from the instrument; and in order that you may be sure that the indication of the instrument is due to the heat from the candle, and not to any manipulation of the apparatus on the beam, I shall perform the experiment as follows. None of the apparatus at this end of the room will be touched or moved in any way; but by a string I shall simply pull the candle along a slide up to a stop, at which position it will shine upon the sensitive part of the radio-micrometer. Instantly the spot of light darts along the scale for a distance of ten feet, and then after leaving the scale it comes to rest upon the face of the balcony five or six seconds after it began to move. Now if the candle is allowed to move back through about a foot, you will see that the instrument will cool down at once—it is at present suffering from the heat which falls upon it from the distant candle; but it will cool down at once, and the index will go back to its old place. It is very nearly at its old place now. I will now let the candle shine upon it again. The index at once goes on to the balcony as before, and now that the candle is moved away again, the index has assumed its old place upon the scale.

That really shows that we have here the means of measuring heat with a degree of delicacy, and also with a degree of certainty, ease, and quickness, which has never yet been equalled. It is probable that the measure which I have given of the degree of delicacy that I have reached in my astronomical apparatus—namely, that the heat of a candle more than two miles away can certainly be felt—will not seem so absurd now that you have seen this less perfect apparatus at work, as it does to people whose experience is limited by the thermopile or their senses.

You can now see the spot of light; it is perfectly quiet in its old place. I wish to show you that this instrument is unlike those which are ordinarily used for this purpose. All the heat, the very considerable heat, due to this electric arc lamp, is actually falling on the instrument, but not upon its sensitive surface, and there is no indication. There are a large number of people in the room—it does not feel the heat from them. Stray heat which it is not meant to feel—which is not in the line along which it can see, or feel—has no influence upon it. When the candle was moved to the place to which it was looking, it felt the heat, and you saw the movement of the index. What is perhaps more important than all is that it is an instrument which does

not even feel the influence of a magnet. I have here a magnet, and on waving the magnet about near the instrument there is no movement of the index at all; it does not dance up and down the scale, as it certainly would do in the case of a galvanometer, because this magnet would affect a galvanometer at the other end of the room. We have then a degree of sensibility which is certainly not easily developed in any other way. I must except however the instrument which Professor Langley of America has recently brought to a great state of perfection. I am unable to state, from want of information, whether his instrument is as sensitive as the one I have just shown, but whether it is or is not as sensitive, it certainly can not compare with this in its freedom from the disturbing effects of stray heat falling upon it, or of the magnetic or thermo-electric disturbances which give so much trouble where the galvanometer is employed.

Now this apparatus I was recently using in some astronomical experiments on the heat of the moon and the stars. As these experiments could only be made with an instrument such as this, possessing extreme sensibility and freedom from extraneous disturbances, and as this instrument is both the cause of the discovery and the first result of the application of the quartz fibers, I have thought it well to repeat a typical experiment upon the moon's heat, but, like Peter Quince, I am in this difficulty. As he said, "There is two hard things, that is to bring the moonlight into a chamber." In fact, at the present time the moon has not risen, and if it had we should not be much better off. Peter Quince proposed that they should in case of moonlight failing have a "lanthorn" and a bunch of thorns. That no doubt was sufficient for the conversation of Pyramus and Thisbe, but that would not do for the purpose of showing the variation of radiation from point to point upon the moon's surface, and as that is the experiment which I now wish to show—an experiment which this instrument enables one to make with the greatest ease and certainty—it is necessary to have something better than a "lanthorn" and a bunch of thorns. Therefore I have been obliged, as the moon is not available, to bring a moon. Now this moon is a real moon; it is not a representation; it is not a slide; it is a real moon, and it is made by taking an egg-shell and painting it white. That egg-shell is now placed upon a stand, and is illuminated by the sun—that is, an electric light; and in order that the moon may be visible, the room must be darkened. The moon is now shining in the sky. An image of the moon is cast by means of a concave mirror upon a translucent screen. There is in addition another mirror which throws a small image of the same moon upon the radio-micrometer There is one more thing to explain. There is upon the screen a black spot which represents the sensitive surface of the radio-micrometer. That bears the same proportion to the moon which you see on the screen as the sensitive surface of the radio-micrometer bears to the image of the moon that is cast upon it. Now the two mir-

rors are arranged to move by clock-work, so as to make the two images travel at proportional rates. The moon is travelling with the dark edge foremost, and now that the terminator of the moon has come upon the sensitive surface, the heat is felt and the deflection of the instrument is the result. Now, as the moon is gradually travelling through the sky, the radiation is slowly and steadily increasing, because the radiation from the moon gets greater and greater, as the point at which the sun is shining vertically—that is, a point at right angles with the terminator—is approached; it is here a maximum, and then it falls back, and as soon as the moon has gone off the instrument, you will see the index fall back almost suddenly. But there is something more. This moon in one respect is better than the other moon. At the present time it represents the moon nineteen days old, a moon, that is to say, which is waning, and which goes through the sky with its dark edge foremost. The clock-work will now bring the moon back again, and convert the nineteen-day moon into a nine-day moon, one in which the bright edge goes forward. What I want you to notice (and it will be perfectly evident) is this, that the spot of light will now go up the scale suddenly, will then rise to a maximum position, and will then fall slowly until the terminator is reached, which proves that in the former case the slow rise and sudden fall, or the present sudden rise and slow fall was not a peculiarity of the instrument, but was due to the fact that the different points of the moon radiated in the manner which I have stated. There is one point which, as the moon has now left the instrument, I should like to show; that is, that it is a real moon and not a mere slide. That is shown by gradually moving the sun round. Now it is at right angles to the line of view, and we have got the half-moon. As it goes round, the moon continues waning, appearing more like a new moon, and at last we have an eclipse of the sun, which may be annular if the proportions of the apparatus are properly arranged.

I wish now to make a few statements as to the delicacy of apparatus that can be made with the help of quartz fibers. I would wish you most distinctly to understand that it is not sufficient to go into a shop and buy apparatus as it is now made, replace the silk by quartz, and to suppose you can get a degree of delicacy such as I have shown you. That is not sufficient. If you take out the silk and put in a quartz fiber the apparatus will be much improved, and you can then increase its delicacy. You will then escape the troubles due to silk; but one after the other a new series of disturbances will appear, and anything like ultimate, extreme, and minute accuracy will still seem out of the question. Now, it has been my business to eliminate one by one these disturbing influences. I will not weary you with a description of them all, and the methods by which they may be certainly provided against. These disturbing causes, which at the present time with instruments carrying a silk fiber are not even known to exist, or if known to exist, are practically of no consequence whatever, come one by one into prom-

inence, when you attempt to push the delicacy of your apparatus to the extent that I have reached in the home-made apparatus which I have here this evening. I do not propose to give more than one illustration, and as this is one which I found out by accident, and which at the time very much annoyed me, I imagine that it may be of interest to explain the circumstances under which this was observed.

In the experiments I made on the heat of the moon and the stars it was necessary to determine to what degree of delicacy the apparatus could be brought,—that is to say, to determine what deflection would be produced by a known and familiar source of radiation. For this purpose the source of heat that I used was a common candle, placed sufficiently far off to produce a convenient deflection. I began by placing the candle about 100 yards away, but I was obliged to place the candle at a distance of 250 yards. At that distance I could not conveniently at night turn the shutter on and off with a string. Therefore I adopted the more simple and practical plan of asking my niece to stand at the top of the hill and to pull the string when I gave the signal. The signal was nothing more nor less than my saying the word "on" or "off," so that without moving I could observe the deflection due to the heat of the candle at that distance. Those were the circumstances, but when I shouted "on," before the sound could have reached my niece at the top of the hill, the spot of light had been driven violently off the scale. This seemed as if, as I suspected at the time, one of my little eight-legged friends had got inside the apparatus, and feeling the trembling due to the sound, struck forward, as the diadema spider is known to do, and tried to catch the thing that was flying by. But further experiments showed that this was not the case. It happened that the sound of my voice was just that to which the telescope tube would respond. It echoed to that note, the instrument felt the vibration of the air, and that was the result.

In order to show that an instrument will feel the motion in the air under the influence of sound, I have arranged an experiment of the simplest possible character. I should say that the first instrument of this kind was made many years ago by Lord Rayleigh; but I feel sure that even he would not be prepared for the delicacy to which apparatus on this principle can be brought. It simply depends upon this familiar and well-known fact. A card or a leaf allowed to drop through the air does not fall the way of the least resistance—that is, edgeways—but it turns into the position of greatest resistance, and falls broadside on, or it overshoots the mark, and so gets up a spin.

Supposing you take a little mirror suspended at an angle of 45 degrees to the direction of the waves of sound, the instant sound-waves proceed to travel, that mirror turns so as to get into such a position as to obstruct them. The mirror that I have for this purpose weighs about the twentieth part of a grain, and the fiber on which it is suspended is about the fifteen-thousandth part of an inch in diameter.

The mirror is so small and light that the moment of inertia is a two-hundredth part of that which people ordinarily call the minute and delicate needle of the Thomson mirror-galvanometer. With a fiber only a few inches long, there is no difficulty in getting a period of oscillation of 10 or 11 seconds. When the light from the lamp is reflected and falls upon the scale, as it will be in a minute, then a movement of the light from one of those great divisions to the next—that is, a movement of 3 inches—will correspond to a twisting force such as would be produced by pulling the end of a lever an inch long with a force of a thousand-millionth part of the weight of a grain. It would be easy to observe a movement ten or a hundred times less. My difficulty now is that it is impossible to speak and at the same time to keep that spot at rest, because the instrument is arranged to respond to a certain note. This is not the predominating note of my voice, but since the voice, like all other noises as distinguished from pure musical sounds, consists of a great number of notes, every now and then the note to which the instrument is tuned is sure to be sounded, and then it will respond. Therefore, while I am speaking it is impossible to keep the spot of light at rest. However, in order to show that the instrument does respond to certain notes, even if feeble, with a degree of energy and suddenness which I believe would never be expected, I shall with these small organ pipes sound three notes. But I must explain beforehand what I am going to do, as the sound of my voice will spoil the experiment. I shall, standing as far away as I can get from the instrument, first sound a note that is too high; I shall then sound a note that is too low; and then I shall sound the note to which the instrument is tuned. I must ask everyone during this experiment to be as quiet as possible, as the faintest sound of the right sort will interfere with the success of the experiment. [The first two notes sounded loudly produced no result, while the moment the right note was heard the light went violently off the scale and travelled round the room.] When this little organ pipe was blown at the farthest end of the room this afternoon, it drove the light off the scale almost as violently as it did just now.

[The Cavendish experiment of observing the attraction due to gravitation between masses of lead was then explained, and the actual experiment, performed with apparatus no larger than a galvanometer, in which the attracting masses were two pounds and fifteen grains, respectively, in which the beam was only about five-eighths of an inch long, and in which the total force was less than one ten-millionth of the weight of a grain, was then shown. The actual deflection on the scale was rather more than ten feet, and eighty seconds were required for the single oscillation. With this apparatus forces two thousand times as small could be observed, though the fiber is, in comparison with others that were made use of, exceedingly coarse. Forces equivalent to one

million-millionth of the weight of a grain were stated to be within the reach of a manageable quartz fiber.]

Now that I have shown all that my limited time has permitted me, I wish finally to answer a question which is frequently put to me, and which possibly some in the room may have asked theselves. The question may be put broadly in this form: "These fibers no doubt are very fine and very wonderful, but are they of any practical use?" This is a question which I find it difficult to answer, because I do not clearly know what is meant by "practical use." If by "a thing of practical use" you mean something which is good to eat or to drink or if you mean something which we may employ to protect ourselves from the extremes of heat or cold or moisture, or if you mean—and this is a point which those who have studied biology will perhaps appreciate more than others—something which may be made use of for the purpose of personal adornment, if that is what you mean by "practical use," then, with the exception of the possibility of being able to weave garments of an extraordinary degree of fineness, softness, and transparency, quartz fibers are of no "practical use." But if you mean something which will enable a large and distinguished body of men to do that which is most important to them more perfectly than has been possible hitherto—I allude of course to the experimental philosopher and his experimental work, which after all has laid the foundations upon which so much that is called practical actually is built—if this is what you mean, then I hope that the few experiments which I have been able to show this evening are sufficient to prove that quartz fibers are of some practical use; and they have served this additional purpose, with what success I am unable to say: they have provided a subject for an evening lecture of the British Association.

THE RESEARCHES OF DR. R. KŒNIG

ON THE PHYSICAL BASIS OF MUSICAL HARMONY, AND TIMBRE.*

By Prof. SYLVANUS P. THOMPSON.

I.

Not often does it fall to the lot of a scientific man to become the mouthpiece of another whose researches have lasted over a quarter of a century; yet this is the enviable position in which I find myself on this occasion as the spokesman of Dr. Rudolph Kœnig, who is known not only as the constructor of the finest acoustical instruments in the world, but as an investigator of great originality and distinction, and author of numerous memoirs on acoustics. Dr. Kœnig, who has of late made very important contributions to our knowledge of the physical basis of music, using apparatus immeasurably superior to any hitherto employed in experimental investigations of this subject, has on various occasions, when I have visited him in Paris, shown me these instruments, and repeated to me the results of his researches. Important as these are, they are all too little known in this country, even by the professors of physics. It was, therefore, with no little satisfaction that the Council of the Physical Society learned that Dr. Kœnig was willing to send over to London for exhibition on this occasion the instruments and apparatus used in these researches. And their satisfaction to-day is heightened by the fact that Dr. Kœnig has himself very kindly come over to demonstrate his own researches, and has given us the opportunity to welcome him personally amongst us.

The splendid apparatus around me belongs to Dr. Kœnig and forms but a very small part of the collection which adorns his *atelier* on the Quai d'Anjou. He lives and works in seclusion, surrounded by his instruments, even as our own Faraday lived and worked amongst his electric and magnetic apparatus. His great tonometer, now nearly completed, comprises a set of standard tuning forks, adjusted each one by his own hands, ranging from 20 vibrations per second up to nearly 40,000, with perfect continuity, many of the forks being furnished with sliding adjustments, so as to give by actual marks upon them any de-

* Read to the Physical Society of London, May 16, 1890. (From *Nature*, January 1, 8, and 15, 1891, vol. XLIII, pp. 199–203, 224–227, and 249–253.)

sired number of vibrations within their own limits. Beside this colossal master-piece, Dr. Kœnig's collection includes several large wave-sirens and innumerable pieces of apparatus in which his ingenious manometric flames are adapted to acoustical investigation. There also stands his tonometric clock, a timepiece governed, not by a pendulum, but by a standard tuning-fork, the rate of vibration of which it accurately records.

It is not surprising that one who lives amongst the instruments of his own creation and who is familiar with their every detail should discover amongst their properties things which others whose acquaintance with them is less intimate have either overlooked or only imperfectly discerned. If he has in his researches advanced propositions which contradict or seem to contradict the accepted doctrines of the professors of natural philosophy, it is not that he deems himself one whit more able than they to offer mathematical or philosophical explanations of them; it is because, with his unique opportunities of ascertaining the facts by daily observation and usage, he is impelled to state what those facts are and to propound generalized statements of them, even though those facts and generalized statements differ from those at present commonly received and supposed to be true.

At the very foundations of the physical theory of music stand three questions of vital importance:

(1) Why is it that the ear is pleased by a succession of sounds belonging to a certain particular set called a scale?

(2) Why is it that, when two (or more) musical sounds are simultaneously sounded, the ear finds some combinations agreeable and others disagreeable?

(3) Why is it that a note sounded on a musical instrument of one sort is different from and is distinguishable from the same note sounded with equal loudness upon an instrument of another sort?

These three queries involve the origin of *melody*, the cause of *harmony*, and the reason of *timbre*.

The theories which have been framed to account for each of these three features of music are based on a double foundation, partly physical, partly physiological. With the physiological aspect of this foundation we have to-night nothing to do, being concerned only with the physical aspect. What, then, are the physical foundations of melody, of harmony, and of timbre? Demonstrable by experiment they must be, in common with all other physical facts; otherwise they can not be accepted as proven. What are the facts and how can they be demonstrated?

We are not here, however, to fight over again the battle of the temperaments, nor do I purpose to enter upon a discussion of the origin of melody, which, indeed, I believe to be associative rather than physical. I shall confine myself to two matters only, with which the recent researches of Dr. Kœnig are concerned:—the *cause of harmony*, and the

nature of timbres. Returning, then, to the ratios of the vibration numbers of the major scale, we may note that two of these, namely, the ratios 9:8 and 15:8, which correspond to the intervals called the major whole tone and the seventh, are dissonant—or, at least, are usually so regarded. It will also be noticed that these particular fractions are more complex than those that represent the consonant intervals. This naturally raises the question: *Why is it that the consonant intervals should be represented by ratios made up of the numbers* 1 *to* 6 *and by no others?*

To this problem the only answer for long was the entirely evasive and metaphysical one that the mind instinctively delights in order and number. The true answer or rather the first approximation to a true answer was only given about 40 years ago, when von Helmholtz, as the result of his ever-memorable researches on the sensations of tone, returned the reply: *Because only by fulfilling numerical relations which are at once exact and simple can the "beats" be avoided which are the cause of dissonance.* The phenomenon of beats is so well known that I may assume the term to be familiar. An excellent mode of making beats audible to a large audience is to place upon a wind-chest two organ-pipes tuned to $ut_2=128$, and then flatten one of them slightly by holding a finger in front of its mouth. Von Helmholtz's theory of dissonance may be briefly summarized by saying that any two notes are discordant if their vibration numbers are such that they produce beats;—maximum discordance occurring when the beats occur at about 33 per second,—beats if either fewer than these or more numerous being less disagreeable than beats at this frequency. It is an immediate consequence that the degree of dissonance of any given interval will depend on its position on the scale. For example, the interval of the major whole tone, represented by the ratio 9:8, produces four beats per second at the bottom of the pianoforte keyboard, 32 beats per second at the middle of the keyboard, and 256 beats per second at the top. Such an interval ought to be discordant therefore in the middle octaves of the scale only.

To this view of von Helmholtz it was at first objected that, if that were all, all intervals should be equally harmonious provided one got far enough away from being in a bad unison; fifths, augmented fifths, and sixths, minor and major, ought to be equally harmonious. This no musician will allow. To account for this von Helmholtz makes the further supposition that the beats occur, not simply between the fundamental or prime tones, but also between the upper partials which usually accompany prime tones. This leads me to say a word about *upper partial tones* and *harmonics.* I believe many musicians use these two terms as synonymous, but they ought to be carefully distinguished. The term harmonics ought to be rigidly reserved to denote higher tones which stand in definite harmonic relations to the fundamental tone. The great mathematican Fourier first showed that any truly

periodic function, however complex, could be analyzed out and expressed as the sum of a certain series of periodic functions having frequencies related to that of the fundamental or first number of the series, as the simple numbers 2, 3, 4, 5, etc. Thirty years later, G. S. Ohm suggested that the human ear actually performs such an analysis, by virture of its mechanical structures, upon every complex sound of a periodic character, resolving it into a fundamental tone, the octave of that tone, the twelfth, the double octave, etc. Von Helmholtz, arming himself with a series of tuned resonators, sought to pick up and recognize as members of a Fourier series the higher harmonics of the tones of various instruments. In his researches he goes over the ground previously traversed by Rameau, Smith, and Young, who had all observed the co-existence, in the tones of musical instruments, of higher partial tones. These higher tones correspond to higher modes of vibration in which the vibratile organ—string, reel, or air column—subdivides into two, three, four, or more parts. Such parts naturally possess greater frequency of vibration, and their higher tones, when they co-exist along with the lower or fundamental tone, are denominated *upper partial tones*, thereby signifying that they are higher in the scale and that they correspond to vibrations *in parts*. It is to be regretted that Professor Tyndall, in his lectures on sound, rendered von Helmholtz's *Oberpartialtöne* by the term *overtones*, omitting the most significant half of the word. To avoid all confusion in the use of such a term I shall rather follow Dr. Kœnig in speaking of these as *sounds of subdivision*. And I must protest emphatically against calling these sounds harmonics, for the simple reason that in many cases they are very inharmonious. It is a matter to which I shall recur presently.

Returning to the subject of beats, the question arises, What becomes of the beats when they occur so rapidly that they cease to produce a discontinuous sensation upon the ear? The view which I have to put before you in the name of Dr. Kœnig is that they blend to make a tone of their own. Earlier acousticians have propounded, in accordance with this view, that the *grave harmonic* of Tartini (a sound which corresponds to a frequency of vibration that is the difference between those of the two tones producing it) is due to this cause. Von Helmholtz has taken a different view, denying that the beats can blend to form a sound, giving reasons presently to be examined. Von Helmholtz considered that he had discovered a new species of combinational tone, namely, one corresponding in frequency to the *sum* of the frequencies of the two tones, whereas that discovered by Tartini (and before him by Sorge) corresponded to their *difference*. Accordingly, he includes under the term of combinational tones the differential tone of Tartini and the summational tone which he considered himself to have discovered. To the existence of such combinational tones he ascribed a very important part in determining the character, harmonious or otherwise, of cords; and to them also he attributes the ability of the

ear to discriminate between the degrees of harmoniousness possessed by such intervals (fifths, sixths, etc.) as consist of two tones too widely apart on the scale to give beats of a discontinuous character. He also considers that such combinational tones are chiefly effective in producing beats, the summational tones of the primaries beating with their upper partial tones; and that this is the way in which they make an interval more or less harmonious.

The whole fabric of the theory of harmony as laid down by von Helmholtz is thus seen to repose upon the presence or absence of beats; and the beats themselves are in turn made to depend, not upon the mere interval between two notes, but upon the timbres also of those notes, as to what upper partials they contain, and whether those partials can beat with the summational tone of the primaries. It becomes, then, of the utmost importance to ascertain the precise facts about the beats and about the supposed combinational tones. What the numbers of beats are in any given case, whether they do or do not correspond to the alleged differential and summational tones, these are vital to the theory of harmony. Equally vital is it to know what the timbres of sounds are, and whether they can be accurately or adequately represented by the sum of a set of pure harmonics corresponding to the terms of a Fourier series.

In investigating beats and combinational tones, Dr. Kœnig deemed it of the highest importance to work with instruments producing the purest tones; not with harmonium reeds or with polyphonic sirens, the tones of which are avowedly complex in timbre, but with massive steel tuning forks, the pendular movements of which are of the simplest possible character. Massive tuning-forks properly excited by bowing with a violoncello bow, or, in the case of those of high pitch, by striking them with an ivory mallet, emit tones remarkably free from all sounds of subdivision, and of so truly pendular a character (unless over-excited) that none of the harmonics corresponding to the members of a Fourier series can be detected. No living soul has had a tithe of the experience of Dr. Kœnig in the handling of tuning forks. Tens of thousands of them have passed through his hands. He is accustomed to tune them himself, making use of the phenomenon of beats to test their accuracy. He has traced out the phenomenon of beats through every possible degree of pitch, even beyond the ordinary limits of audibility, with a thoroughness utterly impossible to surpass or to equal. Hence, when he states the results of his experience, it is idle to contest the facts gathered on such a unique basis. The results of Dr. Koenig's observations on beats are easily stated. He has observed primary beats, as well as beats of secondary and higher orders, from the interference of two simple tones simultaneously sounded.

When two simple tones interfere, the primary beats always belong to one or other of two sets, called an *inferior* and a *superior* set, corresponding respectively in number to the two remainders, positive and

negative, to be found by dividing the frequency of the higher tone by that of the lower.

This mode of stating the facts is a little strange to those trained in English modes of expressing arithmetical calculations, but an example or two will make it plain. Let there be as the two primary sounds two low tones having the respective frequencies of 40 vibrations and 74 vibrations. What are the two remainders, positive and negative, which result from dividing the higher number, 74, by the lower number 40? Our English way of stating it is to say that 40 goes into 74 once and leaves a (positive) remainder of 34 over. But it is equally correct to say that 40 goes into 74 twice all but 6, or that there is a negative remainder of 6. Well, Dr. Kœnig finds that, when these two tuning forks are tried, the ear can distinguish two sets of beats, one rapid, at 34 per second, and one slow, at 6 per second.

Again, if the forks chosen are of frequencies 100 and 512, we may calculate thus: 100 goes into 512 five times, plus 12; or 100 goes into 512 six times, minus 88. In this actual case the 12 beats belonging to the inferior set would be well heard; the 88 beats belonging to the superior set would probably be almost indistinguishable. As a rule, the inferior beat is heard best when its number is *less* than half the frequency of the lower primary, whilst, when its number is *greater*, the superior beat is then better heard. Dr. Kœnig has never been able to hear any primary beat which did not fall within this rule.

Dr. Kœnig will now illustrate to you the beats, inferior and superior, as produced by these two massive tuning-forks,* each weighing about 50 pounds and each provided with a large resonating cavity consisting of a metal cylinder about 4 feet long, fitted with an adjustable piston. One of them is tuned to the note $ut_1=64$. The other also sounds ut_1; but, by sliding down its prongs the adjustable weights of gun-metal and screwing in the piston of the resonator, its pitch can be raised a whole tone to $re_1=72$. Dr. Kœnig excites them with the 'cello bow, first separately, that you may hear their individual tones, then together. At once you hear an intolerable beating, the beats coming 8 per second. This is the inferior beat, corresponding to the positive remainder; the superior beat you cannot hear. Dr. Kœnig will raise the note of the second fork from re_1 to $mi_1=80$, and the beats quicken to 16 per second. Raising it to $fa_1=85\frac{1}{3}$, and then to $sol_1=96$, while the first fork is still kept at ut_1, the beats increase in rapidity, but are fainter in distinctness. If Dr. Kœnig now substitutes for the second fork one tuned to $la_1=106\frac{2}{3}$, you may be able to hear two beats, the inferior one rapid and faint at $42\frac{2}{3}$ per second, and the superior one slower, but also faint, at $21\frac{1}{3}$ per second. Still raising the pitch to the true seventh tone$=112$, the rapid inferior beat has died out, but now you hear the superior

* These splendid forks, with their resonators, along with other important pieces of Dr. Koenig's apparatus, have since been acquired by the Science and Art Department for the Science Collection at South Kensington.

strongly at 16 per second. If it is raised once more to $si_1=120$ (the seventh of the ordinary scale), the beats are still stronger and slower at 8 per second. Finally, when we bring the pitch up to the octave $ut_2=128$, we find that all beats have disappeared: there is a perfectly smooth consonance. The facts so observed are tabulated for you as follows:

TABLE I.—*Primary beats.*

Primary tones.		Ratio.	Inferior beats.	Superior beats.
ut_1 64	re_1 72	8 : 9	8	—
ut_1 64	mi_1 80	4 : 5	16	—
ut_1 64	fa_1 $85\frac{1}{3}$	3 : 4	$21\frac{1}{3}$	—
ut_1 64	sol_1 96	2 : 3	32	32
ut_1 64	la_1 $106\frac{2}{3}$	3 : 5	$42\frac{2}{3}$	$21\frac{1}{3}$
ut_1 64	(7) 112	4 : 7	—	16
ut_1 64	si_1 120	8 : 15	—	8
ut_1 64	ut_2 128	1 : 2	—	0

Suppose now, keeping the lower fork unaltered, we raise the pitch of the higher note (taking a new fork that starts at the octave) from ut_2 to sol_2 by gradual steps, we shall find that there begins a new set of primary beats, an inferior set, which are at first slow, then get more rapid and become undistinguishable, but succeeded by another rapid and indistinct, which grow stronger and slower, until as the pitch rises to sol_2, the frequency of which is exactly three times that of ut_1, all beats again vanish. This range between the octave and the twelfth tone may be called the second "period," to distinguish it from the period from unison to the first octave, which was our first period. Similarly, the range from the twelfth tone to the second octave is the third period, and from thence to the major third above is the fourth period, and so forth. In each period up to the sixth or seventh of such periods, a set of inferior and a set of superior beats may be observed, and in every case the frequency of the beats corresponds, as I have said, to one or other of the two remainders of the frequencies of the two tones. No beat has ever been observed corresponding to the sum of the frequencies, even when using the slowest forks. None has ever been observed corresponding to the difference of the frequencies, save in the first period, where of course the positive remainder is simply the difference of the two numbers.

That you may hear for yourselves the beats belonging to one of the higher periods, Dr. Kœnig will take a pair of forks which will give us some of the superior beats in the fourth period. One of the forks is the great $ut_1=64$, as previously used, the other is $mi_3=320$, their ratio being 1 : 5. Sounded together they give a pure consonance, but if the smaller one is loaded with small pellets of wax to lower its pitch

slightly, and then bow it, at once you hear beats. It was in studying the beats of these higher periods that Dr. Kœnig made the observation that, whereas the beats of an imperfect unison are heard as alternate silences and sounds, the beats of the (imperfect) higher periods—twelfth tone, double octave, etc.—consist mainly in variations in the loudness of the lower of the two primary tones, an observation which was independently made by Mr. Bosanquet, of Oxford.

Passing from the beats themselves, I approach the question, What becomes of the beats when they occur too rapidly to produce on the ear a discontinuous sensation? On this matter there have been several conflicting opinions, some holding, with Lagrange and Young, that they blend into a separate tone; others, with von Helmholtz, maintaining that the combinational tones can not be so explained and arise from a different cause. Let it be observed that, even if beat-tones exist, it is quite possible for beats and beat-tones to be simultaneously heard. A similar co-existence of a continuous and a discontinuous sensation is afforded by the familiar experiment of producing a tone by pressing a card against the periphery of a rapidly rotating toothed wheel. There is a certain speed at which the individual impulses begin to blend into a continuous low tone, while yet there are distinguishable the discontinuous impulses, the degree of distinctness of the two co existing sounds being dependent on the manner in which the card is pressed against the wheel, that is to say, on the nature of the individual impulses themselves. The opponents of the view that beats blend into a tone state plainly enough that, in their opinion, a mere succession of alternate sounds and silences cannot blend into a tone different from that of the beating tone. Having said that the beats can not blend, they then add that they do not blend; for, say they, the combinational tones are a purely subjective phenomenon. Lastly, they say that even if the beats blend they will not so explain the existence of combinational tones, because the combinational tones have frequencies which do not correspond to the number of the beats.

In the teeth of all these views and opinions, Dr. Kœnig—without dogmatizing as to how or why it is—emphatically affirms that beats do produce *beat tones;* and he has pursued the matter down to a point that leaves no room for doubting the general truth of the fact. The alleged discrepancy between the frequency of the observed combinational tones and that of the beats disappears when closely scrutinized. Those who count the beats by merely taking the difference between the frequencies of the two primary tones, instead of calculating the two remainders, will assuredly find that their numbers do not agree in pitch with the actual sounds heard. But that is the fault of their miscalculation. Those who use harmonium reeds or polyphonic sirens instead of tuning forks to produce their primary tones must not expect from such impure sources to re-produce the effects to be obtained from pure tones. And those who say that the beats calculated truly from the two remainders

will not account for the summational tones have unfortunately something to unlearn—namely, that, when pure tones are used, under no circumstances is a tone ever heard the frequency of which is the sum of the frequencies of the two primary tones.

The apparatus which Dr. Kœnig has brought over enables him to demonstrate in a manner audible, I trust, to the whole assembly in this theatre the existence of the beat tones. His first illustrations relate to tones of primary beats, some belonging to the inferior, others to the superior set, in the first period.

He takes here the fork $ut_6 = 2048$, five octaves higher than the great ut_1. To excite it he may either bow it or strike it with an ivory mallet. With it he will take the fork one note higher, $re_6 = 2304$. When he took the same interval with ut_1 and re_1, the number of beats was 8. The *ut* and *re* of the next octave higher would have given us 16 beats, that of the next 32, that of the next 64, of the fourth octave 128, and that of the fifth 256. But 256 per second is a rapidity far too great for the ear to hear as separate sounds. If there were 256 separate impulses, they would blend to give us the note $ut_3 = 256$. They are not *impulses*, but *beats;* nevertheless, they blend. Dr. Kœnig strikes the ut_6, then the re_6, both shrill sounds when you hear them separately; but when he strikes them in quick succession one after the other, at the moment when the mallet strikes the second fork you hear this clear ut_3 sounding out. I am not going to waste your time in a disputation as to whether the sound you hear is objective or subjective. It is enough that you hear it, pure and unmistakable in pitch. It is the grave harmonic; and the number 256, which represents its frequency, corresponds to the positive remainder when you divide 2304 by 2048.

Now let me give you a beat tone belonging to the superior set; it also will be a grave harmonic, if you so please to call it; but its frequency will correspond neither to the difference nor to the sum of the frequencies of the two primary tones. Dr. Kœnig takes $ut_6 = 2048$ as previously, and with it $si_6 = 3840$. Let us calculate what the superior beats ought to be: 2048 goes into 3840 twice, less 256. Then, 256 being the negative remainder, we ought to hear from these two forks the beat tone of 256 vibrations, which is ut_3, the same note as in our last experiment. He strikes the forks, and you hear the result. The beat tone, which is neither a differential tone nor a summational tone, corresponds to the calculated number of beats.

If I take $ut_6 = 2048$ and $sol_6 = 3072$, the two remainders both come out at 1024, which is ut_5. Dr. Kœnig will first sound ut_5 itself, separately, on an ut_5 fork, that you may know what sound to listen for. Its sound has died away; and now he strikes ut_6 and sol_6, when at once you hear ut_5 ringing out. That sound which you all heard corresponds to the calculated number of beats. That is enough for my present purpose.

The next illustration is a little more complex. I select a case in which the beat tones corresponding to the inferior and the superior

beats will both be present. We shall have four tones altogether—two primary tones and two beat tones. The forks I select are $ut_6 = 2048$, as before, and a fork which is tuned to vibrate exactly 11 times as rapidly at ut_3—it is the eleventh harmonic of that note, but does not correspond precisely to any note of the diatonic scale. It has 2816 vibrations, and is related to ut_6 as 11 : 8. The two remainders, will now be 768 and 1280, which are the respective frequencies of sol_4 and mi_5. Dr. Kœnig will first sound those notes on two other forks, that you may know beforehand what to listen for. Now, on striking the two shrill forks in rapid succession, the *two* beat tones are heard.

If I select, instead of the eleventh harmonic, the thirteenth harmonic of ut_3, vibrating 3328 times in the second, to be sounded along with ut_6, the same two beat tones will be produced as in the preceding case; but $mi_5 = 1280$ is now the inferior one, corresponding to the positive remainder, whilst $sol_4 = 768$ is the superior tone, corresponding to the negative remainder. It is certainly a striking corroboration of Dr. Kœnig's view that the beat tones actually heard in these last two experiments should come out precisely alike, though on the old view, that the combinational tones were simply the summational and differential tones, one would have been led to expect the sounds in the two experiments to be quite different.

One other example I will give you of a beat tone belonging to the second period. The two primary notes are given by the forks $ut_5 = 1024$ and $re_6 = 2304$. The beat tone which you hear is $ut_3 = 256$, which corresponds to the positive remainder.

It will be convenient to draw up in tabular form the results just obtained. These may be considered as abbreviations of the much more extended tables drawn up by Dr. Kœnig, which hang upon the walls, and which are to be found in his book, "Quelques Expériences d'Acoustique."

TABLE II.—*Sounds of primary beats.*

Primary tones.		Ratio.	Inferior beat tone.	Superior beat tone.
ut_6 2048	re_6 2304	8 : 9	1 { ut_3 256	—
ut_6 2048	si_6 3840	8 : 15	—	1 { ut_3 256
ut_6 2048	sol_6 3072	8 : 12	4 { ut_5 1024	4 { ut_5 1024
ut_6 2048	(11th) 2816	8 : 11	3 { sol_4 768	5 { mi_5 1280
ut_6 2048	(13th) 3328	8 : 13	5 { mi_5 1280	3 { sol_4 768
ut_5 1024	re_6 2304	4 : 9	1 { ut_3 256	—

II.

So far we have been dealing with primary beats and beat-tones; but there are also secondary beats and secondary beat-tones, which are produced by the interference of primary beat-tones. An example of a secondary beat is afforded by the following experiment. Recurring to the preceding table of experiments, it may be observed that when the two shrill notes, ut_6, sol_6, giving the interval of the fifth, are sounded together, the inferior and superior beat-tones are both present and of the same pitch. If, now, one of the two forks is lightly loaded with pellets of wax to put it out of adjustment, we shall get beats, not between the primary tones, but between the beat-tones. Suppose we add enough wax to reduce the vibration of sol_6 from 3,072 to 3,070. Then the positive remainder is 1,022 and the negative remainder is 1,026, the former being ut_5 flattened two vibrations, the latter the same note sharpened to an equal amount. As a result there will be heard four beats per second—secondary beats. Similarly, the intervals 2 : 5, 2 : 7, if slightly mistuned, will, like the fifth, yield secondary beats. Or, to put it in another way, there may be secondary beats from the (mistuned) beat-tones that are related (as in our experiment) in the ratio 1 : 1 or in the ratios 3 : 4, 3 : 5, etc., and even by those of 1 : 2, 4 : 5, 4 : 7, etc.

I have given you an example of secondary beats; now for an example of a secondary beat-tone. This is afforded by one of the previous experiments, in which were sounded ut_6 and the 11th harmonic of ut_3. In this experiment, as in that which followed with the 13th harmonic, two (primary) beat-tones were produced, of 768 and 1,280 vibrations respectively. These are related to one another by the interval 3 : 5. If we treat these as tones that can themselves interfere, they will give us for their positive remainder the number 256, which is the frequency of ut_4. As a matter of fact, if you listen carefully you may, now that your attention has been drawn to it, hear that note, in addition to the two primary tones and the two beat-tones to which you listened previously.

In von Helmholtz's Tonempfindungen he expresses the opinion that the distinctness with which beats are heard depends upon the narrowness of the interval between the primary tones, saying that they must be nearer together than a minor third. But, as we have seen, using bass sounds of a sufficient degree of intensity and purity, as is the case with those of the massive forks, beats can be heard with every interval from the mistuned unison up to the mistuned octave. Even the interval of the fifth, ut_1 to sol_1, gave strongly marked beats of 32 per second. When this number is attained or exceeded, the ear usually begins to receive also the effect of a very low continuous tone, the beats and the beat-tone being simultaneously perceptible up to about 60 or 70 beats, or as a roughness up to 128 per second. If, using forks of higher pitches, but of narrower interval, one produces the same number of beats, the beat-tone is usually more distinct. Doubt-

less this arises from the greater true intensity of the sounds of higher pitch. With the object of pursuing this matter still more closely, Dr. Koenig constructed a series of 12 forks of extremely high pitch, all within the range of half a tone, the lowest giving si_6 and the highest ut_7. The frequencies and the beats and beat-tones given by seven of them are recorded in Table III.

TABLE III.

Frequencies of forks.		Ratio.	Beats (calc'd).	Resulting sound.
ut_7 4096 and	si_6 3840	16 : 15	256	ut_3
4096	3968	32 : 31	128	ut_2
4096	4032	64 : 63	64	ut_1
4096	4048	256 : 253	48	sol_{-1}
4096	4056	512 : 507	40	mi_{-1}
4096	4064	128 : 127	32	ut_{-1}
4096	4070	158 : 157	26	—

The first of these intervals is a diatonic semitone; the second of them is a quarter-tone; the third is an eighth of a tone; nevertheless, a sensitive ear will readily detect a difference of pitch between the two separate sounds. The last of the intervals is about half a comma.

These forks are excited by striking them with a steel hammer. Some of the resulting beat-tones will be heard all over the theater; but, in the case of the very low tones of 40 and 32 vibrations, only those who are close at hand will hear them. The case in which there are 26 beats is curious. Most hearers are doubtful whether they perceive a tone or not. There is a curious *fluttering* effect, as though a tone were there, but not continuously.

We have seen, then, that the beat-tones correspond in pitch to the number of the beats; that they can themselves interfere and give secondary beats; and that the same number of beats will always give the same beat-tone irrespectively of the interval between the two primary tones. What better proofs could one desire to support the view that the beat-tones are caused, as Dr. Young supposed, by the same cause as the beats, and not, as von Helmholtz maintains, by some other cause? Yet there are some further points in evidence which are of significance and lend additional weight to the proofs already adduced.

Beats behave like primary impulses in the following respect, that when they come with a frequency between 32 and 128 per second, they may be heard, according to circumstances, either discontinuously or blending into a continuous sensation.

It has been objected that, whereas beats imply interference between two separate modes of vibration arising in two separate organs, combination-tones, whether summational or differential or any other, must

take their origin from some one organ or portion of vibratile matter vibrating in a single but more complex mode. To this objection an experimental answer has been returned by Dr. Koenig in the following way. He takes a prismatic bar of steel, about 9 inches in length, and files it to a rectangular section, so as to give, when it is struck at the middle of a face to evoke transversal vibrations, a sound of some well-defined pitch. By carefully adjusting the sides of the rectangular section in proper proportions, the same steel bar can be made to give two different notes when struck in two directions respectively parallel to the long and short sides of the rectangle. A set of such tuned steel bars are here before you. Taking one tuned to the note ut_6=2,048, with re_6=2,394, Dr. Koenig will give you the notes separately by striking the bar with a small steel hammer when it is lying on two little bridges of wood, first on one face, then on the other face. If, now, he strikes it on the corner, so as to evoke both notes at once, you immediately hear the strong boom of ut_3=256, the inferior beat-tone. If Dr. Koenig takes a second bar tuned to ut_6 and si_6=3,840, you hear also ut_3, this time the superior beat-tone. If he takes a bar tuned to ut_6 and the 11th harmonic of ut_3 (in the ratio 8:11) you hear the two beat-tones sol_4 and mi_5 (in ratios of 3 and 5 respectively), precisely as you did when two separate forks were used instead of one tuned bar.

Dr. Kœnig goes beyond the mere statement that beats blend to a tone, and lays down the wider proposition that any series of maxima and minima of sounds of any pitch, if isochronous and similar, will always produce a tone the pitch of which corresponds simply to the frequency of such maxima and minima. A series of beats may be regarded as such maxima and minima of sound; but there are other ways of producing the effect than by beats. Dr. Kœnig will now illustrate some of these to you.

If a shrill note, produced by a small organ-pipe or reed, be conveyed along a tube, the end of which terminates behind a rotating disk pierced with large, equidistant apertures, the sound will be periodically stopped and transmitted, giving rise, if the intermittences are slow enough, to effects closely resembling beats, but which, if the rotation is sufficiently rapid, blend to a tone of definite pitch. Dr. Kœnig uses a large zinc disk with 16 holes, each about 1 inch in diameter. In one set of experiments this disk was driven at 8 revolutions per second, giving rise to 128 intermittences. The forks used were of all different pitches from $ut_3 = 256$ to $ut_7 = 4096$. In all cases there was heard the low note ut_2 corresponding to 128 vibrations per second. In another series of experiments, using forks ut_2 and ut_3, the number of intermittences was varied from 128 to 256 by increasing the speed, when the low note rose also from ut_2 to ut_3.

From these experiments it is but a step to the next, in which the intensity of a tone is caused to vary in a periodic manner. For this pur-

pose Dr. Kœnig has constructed a siren-disk (Fig. 1), pierced with hol[illegible] arranged at equal distances around seven concentric circles; but t[illegible] sizes of the holes are made to vary periodically from small to larg[illegible] In each circle are 192 equidistant holes, and the number of maxima [illegible] the respective circles was 12, 16, 24, 32, 48, 64, and 96. On rotati[illegible]

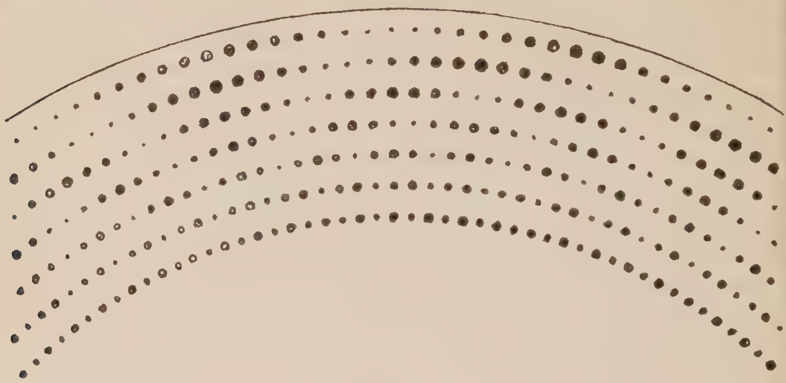

FIG. 1.

this disk, and blowing from behind through a small tube opposite t[illegible] outermost circle, there are heard, if the rotation is slow, a note c[illegible] responding to the number of holes passing per second and a beat c[illegible] responding to the number of maxima per second. With more ra[illegible] rotation two notes are heard—a shrill one, and another 4 octaves low[illegible] in pitch, the latter being the beat-tone. On moving the pipe so th[illegible] wind is blown successively through each ring of apertures, there [illegible] heard a shrill note, which is the same in each case, and a second no[illegible] (corresponding to the successive beat-tones) which rises by intervals [illegible] fourths and fifths from circle to circle.

These attempts to produce artificially the mechanism of beats we[illegible] however, open to criticism; for in them the phase of the individu[illegible] vibrations during one maximum is the same as that of the individu[illegible] vibrations in the next succeeding maximum; whereas in the actu[illegible] beats produced by the interference of two tones the phases of the ind[illegible]

FIG. 2.

vidual vibrations in two successive maxima differ by half a vibratio[illegible] as may be seen by simple inspection of the curves corresponding to [illegible] series of beats. When this difference was pointed out to Dr. Kœni[illegible] he constructed a new siren disk (Fig. 2), having a similar series [illegible]

›les of varying size, but spaced out so as to correspond to a difference ' half a wave between the sets. With this disk, beats are distinctly ;oduced with slow rotation, and a beat-tone when the rotation is ore rapid.

Finding this result from the spacing out of apertures to correspond position and magnitude to the individual wavelets of a complex train ' waves, it occurred to Dr. Kœnig that the phenomena of beats and ' beat-tones might be still more fully re-produced if the edge of the disk ere cut away into a wave-form corresponding precisely to the case of .e resultant wave produced by the composition of two interfering aves. Accordingly he calculated the wave-forms for the cases of sev-al intervals, and having set out these curves around the periphery ' a brass plate, cut away the edge of the plate to the form of the de-red wave. Two such wave-disks, looking rather like circular saws ith irregular teeth, are depicted in Figs. 3 and 4. These correspond to

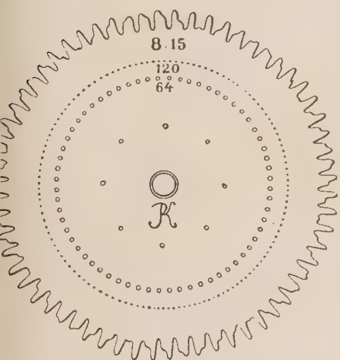

FIG. 3.

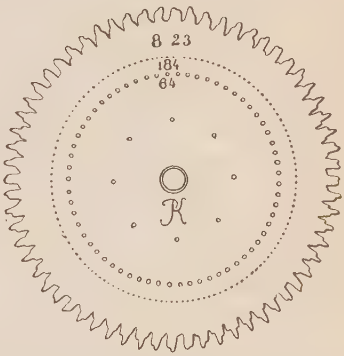

FIG. 4.

ıe respective intervals 8 : 15 and 8 : 23. A number of such wave-.sks corresponding to other intervals lie upon the table; these two ill however suffice. In the first of these the curve is that which ould be obtained by setting out around the periphery a series of 120 mple sinusoidal waves, and a second set of 64 waves, and then com-›unding them into one resultant wave. In order to permit of a com-ırison being made with the simple component sounds, two concentric ngs of holes have been also pierced with 120 and 64 holes respectively. egarding these two numbers as the frequency of two primary tones, ere ought to result beats of frequency 8 (being the negative re-ainder corresponding to the superior beat). An interior set of 8 holes also pierced, to enable a comparison to be made. To experiment ith such wave disks they are mounted upon a smoothly running hirling-table, and wind from a suitable wind-chest is blown against .e wave edge from behind through a narrow slit set radially. In is way the air-pressures in front of the wave-edge are varied by the .sh of air between the teeth. It is a question not yet decided how

far these pressures correspond to the values of the ordinates of the curves. This question, which involves the validity of the entire principle of the wave-siren, can not here be considered in detail. Suffice it to say that for present purposes the results are amply convincing.

The wave-disk (Fig. 3) has been clamped upon the whirling-table, which an assistant sets into rotation at a moderate speed. Dr. Kœnig blows first through a small pipe through one of the rows of holes, then through the other. The two low notes sound out separately, just a major tone apart. Then he blows through the pipe with a slotted mouthpiece against the waved edge; at once you hear the two low notes interfering, and making beats. On increasing the speed of rotation the two notes become shrill, and the beats blend into a beat-tone. Notice the pitch of that beat-tone: it is precisely the same as that which he now produces by blowing through the small pipe against the ring of 8 holes. With the other wave-disk, having 184 and 64 holes in the two primary circles, giving a wave form corresponding to the interval 8: 23, the effects are of the same kind, and when driven at the same speed gives the same beat-tone as the former wave-disk. It will be noted that in each of these two cases the frequency of the beat-tone is neither the difference nor the sum of the frequencies of the two primary tones.

A final proof, if such were needed, is afforded by an experiment, which though of a striking character, will not necessarily be heard by all persons present, being only well heard by those who sit in certain positions. If a shrill tuning-fork is excited by a blow of the steel mallet and held opposite a flat wall, part of the waves which it emits strike on the surface, and are reflected. This reflected system of waves, as it passes out into the room, interferes with the direct system. As a result, if the fork, held in the hand, be moved toward the wall or from it, a series of maxima and minima of sound will successively reach an ear situated in space at any point near the line of motion, and will be heard as a series of beats; the rapidity with which they succeed one another being proportional to the velocity of the movement of the fork. The fork Dr. Kœnig is using is ut_6, which gives well marked beats, slow when he moves his arm slowly, quick when he moves it quickly. There are limits to the speed at which the human arm can be moved, and the quickest speed that he can give to his, fails to make the beats blend to a tone. But if he will take sol_6, vibrating $1\frac{1}{2}$ times as fast, and strike it, and move it away from the wall with the fastest speed that his arm will permit, the beats blend into a short low growl, a non-uniform tone of low pitch, but still having true continuity.

The first portion of my account of Dr. Kœnig's researches may then be summarized by saying that in all circumstances where beats, either natural or artificial, can be produced with sufficient rapidity, they blend to form a beat-tone of a pitch corresponding to their frequency.

III.

I now pass to the further part of the researches of Dr. Kœnig which relates to the *timbre* of sounds. Prior to the researches of Dr. Kœnig it had been supposed that in the reception by the ear of sounds of complex timbre the ear took no account of, and indeed was incapable of perceiving, any differences in phase in the constituent partial tones. For example, in the case of a note and its octave sounded together, it was supposed and believed that the sensation in the ear, when the difference in phase of the two components was equivalent to one-half of the more rapid wave, was the same as when that difference of phase was one-quarter, or three-quarters, or zero. I had myself, in the year 1876, when studying some of the phenomena of binaural audition, shown reasons for holding that the ear does nevertheless take cognizance of such differences of phase. Moreover, the peculiar rolling or revolving effect to be noticed in slow beats is a proof that the ear perceives some difference due to difference of phase. Dr. Kœnig is however the first to put this matter on a distinct basis of observations. That such differences of phase occur in the tones of musical instruments is certain; they arise inevitably in every case where the sounds of subdivision are such that they do not agree rigidly with the theoretical harmonics. Fig. 5 depicts a graphic record taken by Dr. Kœnig from a vibrating steel wire, in which a note and its octave had been simultaneously excited. The two sounds were scarcely perceptibly different from their true interval, but the higher note was just sufficiently sharper than the true harmonic octave to gain about one wave in 180. The graphic trace has in figure 5 been split up into five pieces

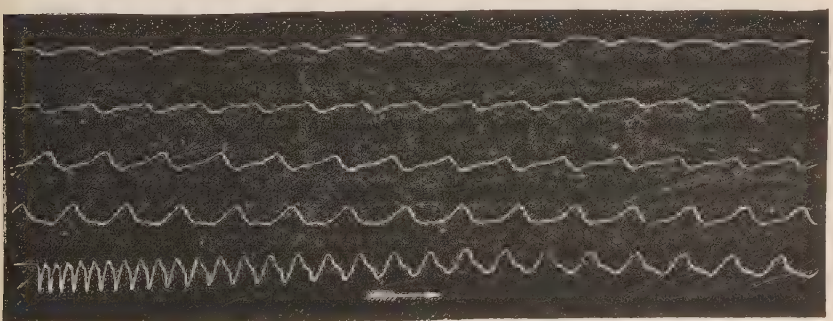

Fig. 5.

to facilitate insertion in the text. It will be seen that as the phase gradually changes the form of the waves undergoes a slow change from wave to wave. Now, it is usually assumed that in the vibrations of symmetrical systems, such as stretched cords and open columns of air, the sounds of subdivision agree with the theoretical harmonics. For example, it is assumed that when a stretched string breaks up into a nodal vibration of four parts, each of a quarter its length, the

vibration is precisely four times as rapid as the fundamental vibration of the string as a whole. This would be true if the string were absolutely uniform, homogeneous, and devoid of rigidity. Strings never are so; and even if uniform and homogeneous, seeing that the rigidity of a string has the effect of making a short piece stiffer in proportion than a long piece, can not emit true harmonics as the sounds of subdivision. In horns and open organ pipes the width of the column (which is usually neglected in simple calculations) affects the frequency of the nodal modes of vibration. Wertheim found the partial tones of pipes higher than the supposed harmonics.

These things being so, it is manifestly insufficient to assume, as von Helmholtz does in his great work, that all timbres possess a purely periodic character; with the necessary corollary that all timbres consist merely in the presence, with greater or less intensity, of one or more members of a series of higher tones corresponding to the terms of a Fourier series of harmonics. When, therefore, following ideas based on this assumption, von Helmholtz constructs a series of resonators, accurately tuned to correspond to the terms of a Fourier series (the first being tuned to some fundamental tone, the second to one of a frequency exactly twice as great, the third to a frequency exactly three times, and so forth), and applies such resonators to analyze the timbres of various musical and vocal sounds, he is trying to make his resonators pick up things which in many cases do not exist—upper partial tones which are exact harmonics. If they are not exact harmonics, even though they exist, his tuned resonator does not hear them, or only hears them imperfectly, and he is thereby lead into an erroneous appreciation of the sound under examination.

Further, when in pursuance of this dominant idea he constructs a system of electro-magnetic tuning-forks, accurately tuned to give forth the true mathematical harmonics of a fixed series, thinking therewith to reproduce artificially the timbres not only of the various musical instruments but even of the vowel sounds, he fails to reproduce the supposed effects. The failure is inherent in the instrument; for it can not reproduce those natural timbres which do not fall within the circumscribed limits of its imposed mathematical principle.

Nothing is more certain than that in the tones of instruments, particularly in those of such instruments as the harp and the pianoforte, in which the impulse, once given, is not sustained, the relations between the component partial tones are continually changing, both in relative intensity and in phase. The wavelets, as they follow one another, are ever changing their forms; in other words, the motions are not truly periodic—their main forms may recur, but with modifications ever changing.

To estimate the part played in such phenomena by mere differences of phase—to evaluate, in fact, the influence of phase of the constituents upon the integral effect of a compound sound—Dr. Kœnig had

recourse to the *wave-siren*, an earlier invention of his own, and of which the wave-disks which have already been shown are examples.

In the first place, Dr. Kœnig proceeded synthetically to construct the wave-forms for tones consisting of the resultant of a set of pure harmonics of gradually decreasing intensity. The curves of these, up to the tenth member of the series, were carefully compounded graphically: first with zero difference of phase, then with all the upper members shifted on one quarter, then with a difference of a half-wave, then with a difference of three-quarters. The results are shown in the top line of curves in Fig. 6, wherein it will be noticed that the curve for difference

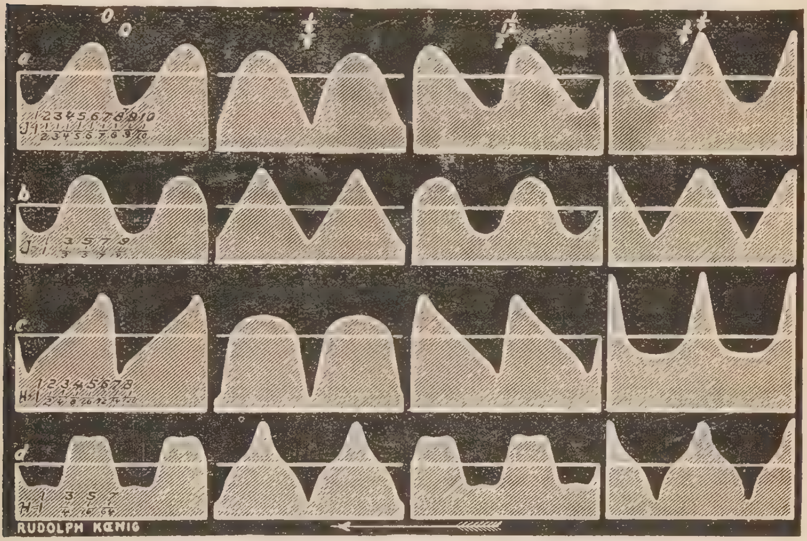

FIG. 6.

of phase $= \frac{1}{2}$ is like that for zero difference, but reversed, left for right; and that the curve for difference of phase $= \frac{3}{4}$ is like that for difference $= \frac{1}{4}$, but inverted. Now, according to von Helmholtz, the sounds of all these four curves should be precisely alike, in spite of their differences of form and position. To test the matter, these carefully plotted curves were set out upon the circumference of a cylindrical band of thin metal, the edge being then cut away, leaving the unshaded portion, the curve being repeated half a dozen times, and meeting itself after passing round the circumference. For convenience, the four curves to be compared are set out upon the separate rims of two such metallic cylindrical hoops, which are mounted upon one axis, to which a rapid motion of rotation can be imparted, as shown in Fig. 7. Against the dentilated edges of these rims, wind can be blown through narrow slits connected to the wind chamber of an organ table. In the apparatus (Fig. 7) the four curves in question are the four lowest of the set of six. It will be obvious that as these curves pass in front of the slits

from which wind issues, the maximum displacement of air will result when the slit is least covered, or when the point of greatest depression of the curve crosses the front of the slit. The negative ordinates of the curve correspond therefore approximately to condensations. Air is

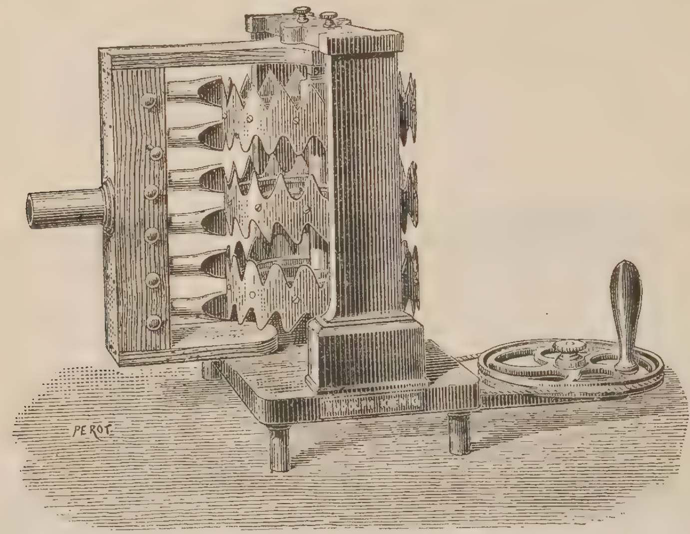

FIG. 7.

now being supplied to the slits; and when I open one or other of the valves which control the air passages, you hear one or other of the sounds. It must be audible to everyone present that the sound is louder and more forcible with a difference of phase of $\frac{1}{4}$ than in any other case, that produced with $\frac{3}{4}$ difference being gentle and soft in tone, whilst the curves of phase 0 and $\frac{1}{2}$ yield tones of intermediate quality. Dr. Kœnig found that if he merely combined together in various phases a note and its octave (which was indeed the instance examined by me binaurally in 1876), the loudest resultant sound is given when the phase difference of the combination is $\frac{1}{4}$, and the mildest when it is $\frac{3}{4}$.

Returning to Fig. 6, in the second line are shown the curves which result from the superposition of the odd members only of a harmonic series of decreasing amplitude. On comparing together the curves of the four separate phases, it is seen that the form is identical for phases 0 and $\frac{1}{2}$, which show rounded waves, whilst for phases $\frac{1}{4}$ and $\frac{3}{4}$ the forms are also identical, but with sharply angular outline. These two varieties of curve are set out on the two edges of the highest metallic circumference in the apparatus depicted in Fig. 7. The angular waves are found to yield a louder and more strident tone than the rounded waves, though, according to von Helmholtz, their tones should be alike.

A much more elaborate form of compound wave siren was constructed by Dr. Kœnig for the synthetic study of these phase relations. Upon a single axis, one behind the other, is mounted a series of 16 brass disks,

cut at their edges into sinusoidal wave forms. These represent a harmonic series of 16 members of decreasing amplitude, there being just 16 times as many small sinuosities on the edge of the largest disk as there are of large sinuosities on that of the smallest disk. A photograph of the apparatus is now thrown upon the screen. It is described fully by Dr. Kœnig in his volume on "Quelques Expériences," and was figured and described in *Nature*, July 20, 1882, vol. XXVI, p. 277. Against the edge of each of the 16 wave disks wind can be separately blown through a slit. This instrument therefore furnishes a fundamental sound with its first fifteen pure harmonics. It is clear that any desired combination can be obtained by opening the appropriate stops on the wind-chest; and there are ingenious arrangements to vary the phases of any of the separate tones by shifting the positions of the slits. The following are the chief results obtained with this instrument. If we first take simply the fundamental tone and its octave together, the total resultant sound has the greatest intensity when the difference of phase $\delta=\frac{1}{4}$ (*i. e.*, when the maximum displacement of air occurs at the same instant for both waves); and at the same time the whole character of the sound becomes somewhat graver, as if the fundamental tone predominated more than in other phases. The intensity is least when $\delta=\frac{3}{4}$. If, however, attention is concentrated on the octave note while the phase is changed, its intensity seems about the same for $\delta=\frac{1}{4}$ as for $\delta=\frac{3}{4}$, but weaker in all other positions. The compound tones formed only of odd members of the series have always more power and brilliancy of tone for phase differences of $\frac{1}{4}$ and $\frac{3}{4}$, than for 0 and $\frac{1}{2}$; but the quality for $\frac{1}{4}$ is always the same as for $\frac{3}{4}$, and the quality for 0 is always the same as for $\frac{1}{2}$. This corresponds to the peculiarity of the corresponding wave form, of which the fourth line of curves in Fig. 6 is an example. For compound tones corresponding to the whole series, odd and even, there is in every case minimum intensity, brilliancy, and stridence with $\delta=\frac{3}{4}$, and maximum with $\delta=\frac{1}{4}$. Inspection of the first and third lines of curves in Fig. 6 shows that in these wave forms that phase which is the most forcible is that in which the maximum displacement and resulting condensation is sudden and brief.

Observing that wave-forms in which the waves are asymmetrical—steeper on one side than on the other—are produced as the resultant of a whole series of compounded partial tones, it occurred to Dr. Kœnig to produce from a perfect and symmetrical sinusoidal wave curve a complex sound by the very simple device of turning into an oblique position the slit through which the wind was blown against it. In Fig. 8 is drawn a simple symmetrical wave form, *eglnprtv*. If a series of such wave forms is passed in front of a vertical slit, such as *ab*, a perfectly simple tone, devoid of upper partials, is heard. But by inclining the slit, as at *ab′*, the same effect is produced as if the wave form had been changed to the oblique outline *e′g′l′n′p′r′t′v′*, the slit all the while remaining upright. But this oblique form is precisely like that obtained

as resultant of a decreasing series of partial tones (Fig. 6, a). If the slit be inclined in the same direction as the forward movement of the waves, the quality produced is the same as if all the partial tones coincided at their origin, or with $\delta = 0$; while if inclined in the opposite

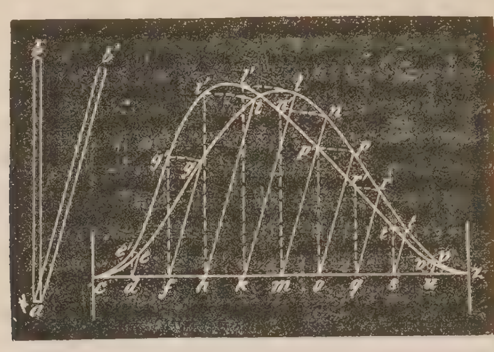

FIG. 8.

direction the quality is that corresponding to $\delta = \frac{1}{2}$. It is easy to examine whether the change of phase produces any effect on the sound. Before you is rotating a simple wave disk, and air is being blown across its edge through a slit. Dr. Kœnig will now tilt the slit alternately backward and forward. On tilting the slit forward to give $\delta = 0$, you hear a purer and more perfect sound; and on tilting it back, giving $\delta = \frac{1}{2}$, a sound that is more nasal and forcible.

All the preceding experiments agree then in showing that differences of phase do produce a distinct effect upon the quality of compound tones; what then must we say as to the effect on the timbre of the presence of upper partial tones or sounds of subdivision that do not agree with any of the true harmonics? A mis-tuned harmonic—if the term is permissible—may be looked upon as a harmonic which is undergoing continual change of phase. The mistuned octave which yielded the graphic curve in Fig. 5, is a case in point. The wavelets are continually changing their form. It is certain that in a very large number of musical sounds, instrumental and vocal, such is the case.

It was whilst experimenting with his large compound wave siren that Dr. Kœnig was struck by the circumstance that under no conditions, and by no combination of pure harmonics in any proportion of intensity or phase, could he reproduce any really strident timbres of sound, like those of harmonium reeds, trumpets, and the like; nor could he produce satisfactory vowel qualities of tone. Still less can these be produced satisfactorily by von Helmholtz's apparatus with electro-magnetic tuning forks, in which there is no control over the phases of the components. The question was therefore ripe for investigation whether for the production of that which the ear can recognize as a *timbre*, a definite unitary quality of tone, it was necessary to suppose that all the successive wavelets should be of similar form. Or, if the forms of

the successive wavelets are continually changing, is it possible for the ear still to grasp the result as a unitary sensation?

If the ear could always separate impure harmonic or absolutely inharmonic partials from their fundamental tone, or if it always heard pure harmonics as an indistinguishable part of the unity of the timbre of a fundamental, then we might draw a hard and fast line between mere mixtures of sound and timbres, even as the chemist distinguishes between mere mixtures and true chemical compounds. But this is not so; sometimes the ear can not unravel from the integral sensation the inharmonious partial; on the other hand, it can often distinguish the presence of truly harmonious ones. Naturally, something will depend on the training of the ear; as is the case with the conductor of an orchestra, who will pick out single tones from a mixture of sounds which to less perfectly trained ears may blend into a unitary sensation.

Dr. Kœnig accordingly determined to make at least an attempt to determine synthetically how far the ear can so act, by building up specific combinations of perturbed harmonics or inharmonic partials, giving rise to waves that are multiform, as distinguished from the uniform waves of a true periodic motion. The wave siren presented a means of carrying this attempt to a result. On the table before me lie a number of wave disks constructed with this aim. This will be successively placed upon the whirling table, and sounded; but I must warn you that the proper effects will only be perceived by those who are near the apparatus, and in front of it.

Upon the edge of the first of the series there has been cut a curve graphically compounded of 24 waves as a fundamental, together with a set of four perturbed harmonics of equal intensity. The first harmonic consists of 49 waves $(2 \times 24 + 1)$, the second of 75 waves $(3 \times 24 + 3)$, the third of 101 $(4 \times 24 + 5)$, the fourth of 127 $(5 \times 24 + 7)$. The resulting curve possesses 24 waves, no two of them alike in form, and some highly irregular in contour. The effect of blowing air through a slit against this disk is to produce a disagreeable sound, quite lacking in unitary character, and indeed suggesting intermittence.

The second wave disk is constructed with the same perturbed harmonics, but with their amplitudes diminishing in order. This disk produces similar effects, but with more approach to a unitary character.

In the third disk there are also 24 fundamental waves, but there are no harmonics of the lower terms, the superposed ripples being perturbed harmonics of the fifth, sixth, and seventh orders. Their numbers are $6 \times 24 + 6$, $7 \times 24 + 7$, and $8 \times 24 + 8$, being, in fact, three harmonics of a fundamental 25. This disk gives a distinctly dual sort of sound, for the ear hears the fundamental quite separate from the higher tones, which seem in themselves to blend to a unitary effect. There is also an intermittence corresponding to each revolution of the disk, like a beat.

The fourth disk resembles the preceding; but the gap between the

fundamental and the three perturbed harmonics has been filled by the addition of three true harmonics. This disk is the first in this research which gives a real timbre, though it is a peculiar one. It preserves, however, a unitary character, even when the slit is tilted in either direction. The 24 waves in this disk all rake forward like the teeth of a circular saw, but with multiform ripples upon them. The quality of tone becomes more crisp when the slit is tilted so as to slope across the teeth, and more smooth when in the reverse direction.

The fifth disk, which is larger, has 40 waves at its edge. These are cut with curves of all sorts, taken hap-hazard from various combinations of pure harmonics in all sorts of proportions and varieties, no two being alike, there maxima and minima of the separate waves being neither isochronous nor of equal amplitude. This disk gives an entirely unmusical effect, amid which a fundamental tone is heard, accompanied by a sort of rattling sound made up of intermittent and barely recognizable tones.

The sixth disk is derived from the preceding by selecting eight only of the waves, and repeating them five times around the periphery. In this case each set of eight acts as a single long curve, giving beats, with a slow rotation and a low tone (accompanied always by the rattling mixture of higher tones) when the speed is increased.

The seventh disk was constructed by taking 24 waves of perfect sinusoidal form, and superposing upon them a series of small ripples of miscellaneous shapes and irregular sizes, but without essentially departing from the main outline. This disk gives a timbre in which nothing can be separated from the fundamental tone, either with vertical or tilted slit.

The eighth and last disk consists of another set of 24 perfect waves, from the sides of which irregular ripples have been carved away by hand, with the file, leaving however the summits and the deepest parts of the hollows untouched, so that the maxima and minima are isochronous and of equal amplitude. This disk gives also a definite timbre of its own, a little raucous in quality, but still distinctly having a musical unity about it.

We have every reason therefore to conclude that the ear will recognize as possessing true musical quality, as a timbre, combinations in which the constituents of the sound vary in their relative intensity and phase from wave to wave.

What, then, is a *timbre?* Dr. Kœnig would be the first to recognize that these last experiments, though of deepest interest, do not afford a final answer to the question. We may not yet be in a position to frame a new definition as to what constitutes a timbre, but we may at least conclude that, whenever that definition can be framed, it will at least include several varieties, including the non-periodic kinds with multiform waves, as well as those that are truly periodic with uniform waves. We must not on that account however, rush to the conclusion that the

theory of von Helmholtz as to the nature of timbre has been overthrown. The corrections introduced into lunar theory by Hansen and Newcombe have not overturned the splendid generalizations of Newton. What we can and must confess is that we now know that the acoustic theory of von Helmholtz is, like the lunar theory of Newton, correct only as a first approximation. It has been the distinctive merit of Dr. Kœnig to indicate to us the magnitude of the correcting terms, and to supply us not only with a rich store of experimental facts but with the means of prosecuting the research synthetically, beyond the point to which he himself has attained.

In thanking Dr. Kœnig for the courtesy which he has shown to this society in bringing over his apparatus and in demonstrating its use to us, we must join in congratulating him on the patience, perspicacity, and skill with which he has carried out his researches. We know that his exceptional abilities as experimentalist and constructor have done more than those of any other investigator to make the science of experimental acoustics what it is to-day; and we must unite in wishing him long life and prosperity to complete the great work on which already he has advanced so far.

THE CHEMICAL PROBLEMS OF TO DAY.*

By Victor Meyer.
Translated by L. H. Friedburg.†

When, a short time ago, I was called upon to speak before you, I gladly and zealously approached the work which such an occasion seemed to call forth. It seemed to me that it would be an effort worthy of this assemblage of scientific men to recall the permanent additions that chemistry has made in our day to the treasure of human knowledge and to enumerate the problems which seem to lie nearest us in the future.

A science which, as such, is hardly older than the great European revolution, the centennial of which we witnessed a few months ago, and which in this short time has caused changes in our spiritual and material life hardly less than those of the political revolution, such a science, I have thought, may without temerity boast of its achieve ments.

And yet the chemist approaches such a task with a certain hesitation from which the astronomer, the physicist, and the mathematician are free. Has it not been in our own day that the most prominent orator amongst German naturalists, one who astonishes us by the comprehensiveness of his knowledge, has adopted as his own Kant's judgment on chemistry, namely, that "chemistry is a science, but not a science in the highest sense of the word; that is, a knowledge of nature reduced to mathematical mechanics." And this dictum is accepted, not as a blemish upon our science, but with the fullest and most perfect recognition of the immense achievements which modern chemistry has registered as its own.

But all of the marvellous successes of the atomic theory and of the doctrine of structure, the synthesis of the most complicated organic compounds, the blessings of an enlarged pharmacopœia, the potent revolution in technological processes, the new and systematic methods

* An address delivered at Heidelberg at the first general session of the sixty-second meeting of the Association of German Naturalists and Physicians, September 18, 1889.

† From the *Deutsche Rundschau*, November, 1889. (Re-printed from the *Journal of the American Chemical Society*, September, 1889, vol. XI, pp. 101–120.)

of production which have been characterized by an eminent technologist as "the gaining of gold from rubbish"—all this seems trifling to the mind that looks down from its standpoint of mathematical mechanics when compared with the work of a promised Newton of chemistry, who some day will represent chemical reactions in the thought and in the language of mathematical physics.

And if he who looks from a height is justified in the expression that to-day chemistry, in the recognition of ultimate causes, stands yet below astronomy of the time of Kepler and Copernicus, must not the chemist lose courage if he attempts, before an illustrious assemblage, to raise a song of praise to his science, to glorify what she has done and what in the future she seems chosen to do? If in spite of this the attempt be made, it must be with that resignation which rests upon the belief that "we should consider everything, but aim only at that which is possible."

Though we share, with full conviction, the expectations of a Newtonian period in chemistry, we hardly venture to hope that that period is near, and even the most enlightened representatives of the newer physical chemistry seem but precursors of that distant era.

Perhaps the chemist, immersed in the daily work of his science, fails to take the comprehensive view of one who from a distant height looks down upon the same. But those who are surrounded by the whirl of hourly renewed work recognize all the more clearly the immense amount that remains still to be achieved before those distant aims can be realized. This epoch, so rich in path-finders in the department of physics, has rarely directed the highest order of research into the territory of our science, and especially have the more complicated chemical phenomena been avoided.

If in a period that has witnessed the discoveries of Helmholtz, Robert Mayer, Joule, Clausius, and van't Hoff, the revolutionizing progress of knowledge has been limited to physics, and if only modest applications of what was gained have been made in related studies, then the epoch seems not yet to be at hand in which chemical processes can be thought of as we think of the movements which we feel as sound, light, or heat.

A humiliating statement! But, strange to say, the chemist of to-day has hardly time to complain of this resignation imposed upon him, and this for reasons easily understood.

If without question it is the aim of all natural science to understand phenomena so fully that they may be described in a mathematical form, and, as far as they are unknown, may be predicted, a science which is so far distant from this aim as to look merely for the *path* that shall some day lead to it, must be considered as in its infancy. In the present stage our way of thinking and acting has this peculiarity. In every science imagination must stand as another power alongside of knowledge and reasoning. But the influence of imagination upon knowledge is all the greater the further this latter is distant from the men-

tioned ideal. And thus it happens that in the chemistry of to-day imagination and intuition have a larger scope than in other sciences, and that occupation with the same, besides the pure scientific satisfaction that it yields, brings an enjoyment which, in a certain sense, reminds one of the activity of an artist. He however who only knows chemistry as a tradition of perfectly clear facts, or who thinks to see the real soul of chemical study in measuring the *physical* phenomena which accompany chemical transformations, feels no breath of this enjoyment.

The feeling is only disclosed to him who ventures into that ocean of the unknown that is spread out before us in the *organic chemistry* of the day; to him who is not appalled by a wilderness, populated with thousands of indivuals, of which every one shows a peculiar, fully unknown originality, and to him who attempts to become better acquainted with some of them, even if he is at a loss for a means of approaching them. To proceed with success in this direction is only granted to the genius; the method that leads onward can not be learned, and it has only been practiced with success by a small number of chosen ones.

Indeed, in the experimental study of organic chemistry, the "presentiment" of happenings, the actuality of which is not indicated by any law to be expressed in words, has shown surprising results; here the thought is aided by a something, which we may meanwhile term "chemical feeling," a name which will disappear as soon as the progressive approach of chemistry to the mathematical physical basis shall have disclosed its meaning and shall have tabulated it amongst the methods which lead to the recognition of the new. The effect of this peculiar chemical method of study is not here to be dwelt upon in detail. Let it suffice to say that without it, the most brilliant discoveries in organic chemistry would not have been made; just as little as a Kekulé would without it, have been able—in contradiction of numerous data in chemical literature never before doubted—to affirm the non-existence of isomeric monochlorbenzol and of such bodies as were said to consist of a benzol ring and but *one* bi-valent atom. Those significant hypotheses by means of which the knowledge of aromatic substances has been revealed to us, could not have been made solely upon the ground of exact observation; they required at the same time a pronounced chemical instinct. There was no logical reason in declaring the existence of a phenylene oxide as an impossibility, since the ethylene oxide did exist; he who nevertheless ventured to do so, and at the same time ran directly in the face of experience, was surely led by a feeling which the present status of chemistry forbids us to replace by a process of thought.

But to return from the field of organic to that of general chemistry. Before we can arrive at a mathematico-physical treatment of chemical phenomena in general, two fundamental problems must be solved; an hypothesis which allows a control by experiment (even within the same

limits which to this day are imposed upon physics in regard to the law of gravitation), must answer these questions: *What is Chemical Affinity?* and *What is Valency?*

By means of laborious detail work, chemistry tries to approach the solution of these enigmas; but he who pursues chemical methods, who stands in the midst of chemical work—which aims only, as at a far distant task, at the discovery of a sure *path*—still sees such obstacles to be cleared away that he gives up the hope of living to see the new chemical era. He finds satisfaction in the consciousness of having exerted his best abilities in the elucidation of some minor and precursory principles.

If now we begin to consider—within the appointed limits—the most important achievements of chemistry, we can not, at this place and at this hour of our meeting, be in doubt as to what is to be mentioned in the first place. The hospitable city which shelters us boasts of an advantage which is envied her by every other alma mater; here, chemistry for more than a human lifetime has been represented by Robert Bunsen, of glorious name, and the very days which find us here assembled, follow immediately the moment in which this hero of science has retired from his academical occupation. Who does not think, at such an hour, of the great teacher around whom ardent pupils from all parts of the globe were accustomed to congregate? But who, being called upon to-day to speak of the results of chemistry within the walls of Heidelberg, would not before all direct an eye upon that one discovery which has lifted chemistry beyond terrestrial research, which has enabled her, like astronomy, to search the universe and to dissect the starry heavens, chemically, by the subtle appliances of analysis? If "old Heidelberg" has become a pearl amongst German cities by its history, by its numerous traditions, by the incomparable beauty of its situation,—if its university is the ideal of the German academical youth, we may well regard as an immortal leaf in its wreath of honor, along with these glorious titles, the union of those two great men who first met in this city in the most courageous enterprise of the penetrating mind; who have pursued with astonishing success the investigation which has made spectral analysis the most potent of scientific weapons, and has rendered their names a charm calling forth the admiration of the older minds and kindling in the minds of mere school boys the flame of enthusiasm in the study and exploration of nature. The immeasurable results of that discovery—the consequences of which extend every day over new territories—are known in the widest circles, and to mention them to-day in detail would be but carrying owls to Athens. It behooves us in this place to mention reverently the names of Bunsen and Kirchhoff, to think of them with gratitude, and to hope that men, their equals, may not be entirely wanting in the next generation! The younger one of them—whose scientific fertility was only equalled by his greatness of soul and the charming modesty of his heart—has

been taken away from us before old age had naturally limited him. Bunsen we still rejoice to call ours, who now, allowing the tools of his work to drop from his hand, looks forth to the evening of his life in quiet, happy leisure. May he be permitted for a long time to look back upon a life filled with greatest scientific achievements; may his calm, friendly eye rest for many years upon the incomparable picture of his beloved Heidelberg.

We have mentioned spectral analysis, though it has been almost for an age the common property of science. Let us also cast a grateful retrospect upon a deeply furrowing revolution—of which chemistry also, for several decades, has boasted as a substantial possession—upon the development of the *doctrine of structure*, that solid theoretical foundation from which the proud edifice of modern organic chemistry rises. A generation has grown up around us which has received as a matter of fact this doctrine which still seems new to us older ones. But those far-seeing men, whose eyes recognized the immensely simple in the seemingly impenetrable complication of the carbon compounds, are still actively alive amongst us, and it is their happy lot to reap in their own activity what once they sowed in juvenile work Here the eye is directed upon the master of chemical research—August Wilhelm von Hofmann; before all upon his researches upon the organic nitrogenous bases,—researches which do not find their equal in organic chemistry and which, even more perfectly than Dumas' fundamental discovery of trichoracetic acid, allowed the fundamental conception of substitution to expand into the living consciousness of chemists, at first, curiously, by supporting the theory of types in organic compounds and then by promoting the transition to the structural or constitutional view, which at present embraces, with unparalleled perfection, the whole territory of organic compounds.

But the suggestion of this doctrine, which finds its crowning success in the recognition of the inner aggregation of the atoms, is associated for all time with the name of a man who, although a master of rare art in experimenting, knew how to surpass what he had achieved at the laboratory table, by the convincing power of his speculative work. We can not here dispute the part which other eminent chemists have taken in the development of the doctrine of structure—there are, Butlerow, Cooper, Erlenmeyer, Frankland, Kolbe, Odling, Williamson—but the glorious guide in this great and victorious movement forward, he, to whose eyes was disclosed not only the tetra-valence of carbon, but also the solution of the problem of the constitution of organic compounds, in the recognition of the property of carbon atoms to be linked to *each other* by their valencies; he is the *philosopher* of organic chemistry—August Kekulé. The name of this discoverer, who also started upon his high and soaring flight from Heidelberg, is justly mentioned *alone* when we want to recall in a word the putting forth and the development of the leading chemical theories.

The researches in this direction are so numerous and so toilsome, and yet the result is so surprisingly simple! The carbon atom is endowed with four, the oxygen atom with two, the hydrogen atom with one point of attack for the chemical affinity. The cause of the aggregation of the atoms within the molecule lies in the mutual saturation of these units of affinity or valencies. It is the number of valencies which decides the possibility of the existence of a compound. Amongst the legion of imaginable combinations of these three elements only those are capable of existence in which every valency is saturated by that of another atom. Through this knowledge a new method of inquiry was opened, in particular for organic chemistry, the immense territory of which for many years seemed totally to absorb the working power of chemists. But then dawned the first signs of a further development. Hardly a decade had elapsed since the general admission of the doctrine of valency when a fundamental deepening of the same was announced, which our science owes to two savants, working independently of each other—to Le Bel and van't Hoff. These chemists, considering those substances which turn the plane of polarization of light, arrived at views which soon led to a result until then thought to be out of reach, a conception of the aggregation of the atoms within the molecules in space. Thus a field of study was created which van't Hoff called "*la chimie dans l'espace*" and which we now call *Stereo-chemistry*.

It was recognized that the carbon atom stretched out its four valencies in definite directions, and this in a symmetrical manner. The combination of a carbon atom with four other atoms, for example, methane, CH_4, is representable by the picture of a tetrahedron in the stereometric center of which the carbon atom is situated, while the hydrogen atoms occupy its four corners.

Numerous cases of isomerism, until then not understood, could be explained in this manner and were regarded as stereo-chemical ones. The cause of optical activity was found to consist in the presence of an a-symmetric carbon atom, that is, one which is combined with four different groups.

Also the stereometric forms of a few simple molecules were considered; it was recognized, *e. g.*, that a compound of three carbon atoms linked together by one bond respectively could not contain those atoms in a straight line, but that they must lie in the angles of a triangle the sides of which form an angle equal to that in which the directions of valency of the carbon atom intersect each other.

By the applications of these considerations to more complicated molecules, which contain a chain of atoms closed within itself, Adolph von Baeyer has enlarged our theory in a manner full of consequence.

Kekulé in times past had recognized that carbon shows a particular disposition to form *closed* chains of six atoms. The discoveries of Baeyer and his followers, as well as Fittig's work on lactones, taught that such closed chains or rings formed of fewer atoms also exist. But

while rings of six or five atoms easily form, it is more difficult to combine fewer atoms, four or three, to a closed chain. The cause of this fact Baeyer recognized as lying in the stereometric conditions. The angles which the sides of a regular hexagon and pentagon form with each other very nearly coincide with those in which the directions of the valencies of the carbon atom intersect each other, and thus in linking five or six atoms together the circle, so to speak, closes itself, while if more or less atoms are present this can only be arrived at by strong deviation of the directions of affinity.

But still more surprising discoveries were hidden in van't Hoff's theory. The gifted Dutch thinker had penetrated to the idea that two atoms which are linked together by a single valency rotate freely around an axis the direction of which coincides with that of the linking valency, but that this rotation is stopped as soon as double linking takes place. This latter is an immediate consequence of the tetrahedric conception. If I stretch out my two fore-fingers and let their points touch each other, then the hands can rotate around them as an axis; but if I stretch both thumbs and both fore-fingers and allow their corresponding points to touch each other, then a system results in which rotation is impossible.

These two propositions of van't Hoff, having remained almost unnoticed for a decade, have lately come into great prominence. In a series of important researches Johannes Wislicenus has proved that applying these propositions and at the same time considering the specific affinities of the groups or elements present, the stereometric aggregation of the atoms in certain molecules can be determined with probability. In an ingenious manner he has utilized the addition phenomena shown by carbon atoms trebly linked together for an interpretation of a stereometric aggregation of the atoms in the compounds formed.

Wislicenus, applying van't Hoff's ideas with courage and strictness, has advanced organic chemistry in an important manner and has opened a field for experimental research, which heretofore had been avoided with a precaution suggestive of timidity.

New discoveries came from other sides. An intimate research into the oxims of benzil lead to the surprising result that the validity of the second proposition of van't Hoff is not without exception. Cases were noticed in which the free rotation of carbon atoms united by a *simple* bond, which van't Hoff disclosed, did not obtain. Further inquiry into this subject led to a renewal of the question, "What does *chemical valency* really mean?" A question to which the mind incessantly demands an answer. It had long since been suggested that valency had some relation to the electric behavior of the atoms. The chemistry of the day expresses Faraday's fundamental electrolytic law thus: An electric current which flows through several fused electrolytes severs in each of them the same number of *valencies*, not of *atoms*.

It was found by von Helmholtz that those quantities of electricity which, during the electrolytic process, move with the ions are dis-

tributed among the valencies. Riecke, in virtue of his pyro-electric researches, was led to the view that the atoms are surrounded by certain systems of positive and negative electric poles.

Uniting these results with those of purely chemical experimentation, we arrive at the idea that the valencies do not appear as *points* of attack proper, but as having *linear* dimensions. The carbon atom represents itself as a sphere, surrounded by an envelope of æther which contains the valencies. The latter seem to be determined by the presence of two opposite electric poles which rest at the ends of a very short straight line. Such a system is called a *di-pole.* The attachment of two valencies to each other consists in the attraction of their opposed poles. It is evident that in a radial position of the di-poles they form an axis around which the atoms are able to rotate, but that this rotation is upset in case of a tangential position. In what has been said so far and through further considerations in regard to the electrical charge of the atoms and of the di-poles a reason is found for the repulsion of the four valencies and consequently for the tetrahedric grouping of the same.

The fact that the valencies can deviate from this position now becomes intelligible; we perceive why the valencies of *one* atom can not unite with one another, while those of different atoms can combine; it is clear that there can exist two kinds of simple linking, one of which admits of rotation, while the other does not; finally, that in cases of manifold linking the free rotation must be annulled. Hence this hypothesis opens to us an understanding of the most important properties of chemical valency.

So much may be said of the problems relating to the theory of valency.

But the doctrine of substitution has likewise experienced a peculiar enlargement. Dumas first showed that the properties of organic compounds are generally little changed when the hydrogen of the same is replaced by univalent elements or groups. Now it has been learned from later experiments that even much more radical changes in the composition do not materially influence the properties of the substance. If for example we replace in the hydro-carbon *benzol*—two carbon and two hydrogen atoms by one atom of sulphur, the resulting product, *thiophen*, resembles benzol chemically and physically so closely as to be mistaken for it. We learn from this that the sulphur atom is able to take upon itself the functions of four atoms of entirely different nature. Similar facts have been found in regard to oxygen and to the imido group, which is equivalent to it.

Turning away from these researches to cast a glance upon general chemical studies which lie some years behind us, we must above all consider one of the most far reaching discoveries of our epoch, the revelation of the *natural system of the chemical elements.* We owe this to the far-seeing Demetrius Mendelejeff. By the side of the titanic figure of the Russian scholar we see the Englishman, Newlands, and our own countryman, Lothar Meyer, successfully co-operating in the foundation

and the structure of this work. What these men created has since become generally known; *they showed that the properties of the elements are functions of their atomic weights.* Mendelejeff taught us to predict the existence and the properties of chemical elements as yet unknown with a certainty that reminds us of Le Verrier's prediction of the discovery of the planet Neptune. We can say with confidence that even to-day numerous elements, the qualities of which, as well as the place which they will occupy in the system, can be minutely foretold, wait merely to be discovered.

The natural system has imposed upon us a problem of the greatest significance in the new determination of the atomic weights, the numerical values of which are now of increased interest. But numerous other problems are presented by the new system of the elements. Above all we are at a loss to discern the cause of the inner nexus of the elements as the system offers it. Also by diligent work the less studied elements must be properly brought within the system. Fortunate circumstances may allow us to discover the numerous elements indicated by the periodic law. Here let us note a peculiar coincidence. We know to-day about seventy elements, but Mendelejeff's table indicates so far—two small periods of seven elements each, and five large ones of seventeen elements, respectively. To these must be added hydrogen, forming a "group" in itself.

By addition of these figures, $(2 \times 7) + (5 \times 17) + 1$, we obtain exactly the number 100.

It is true that no one can say whether the missing elements will really be discovered, or if further new periods might not be indicated by which this number 100 would be exceeded. But, as far as positive data are at hand, they indicate exactly the number mentioned and nothing points beyond it,—an odd coincidence which seems to ally the number of the existing elements with the number of our fingers.

The discovery of the system of the elements leads us back to the question whether the chemical elements are separate worlds in themselves or whether they represent different forms or conditions under which *one* ultimate substance exists, a question that has occupied the philosophical mind since very early times. The same question was raised anew by the discovery of spectral analysis. Whosoever regards the numerous lines of the spectrum of a metal will hardly be convinced that the metal from which they emanate should be an eternally undecomposable element. In a similar manner the compound nature of the elements is indicated by comparison of the regularities in numbers of the atomic weights with the homologous series of organic chemistry.

In the pursuit of this question, which, since Prout's hypothesis and the surprises offered by Stas's determinations of atomic weights, has not been allowed to rest, positive results are not to be found. The decomposition of substances called elements into simpler ones has not been accomplished.

Nevertheless something has been achieved, since an increased interest has been drawn towards pyro-chemical research.

To-day new methods of experiment permit of a comparatively easy determination of the vapor density and consequently of the molecular state of the substances at the highest temperatures.

Numerous inorganic compounds, above all the very elements, have been studied in regard to their vapor density at a white heat.

While many of them, as oxygen, nitrogen, sulphur, and mercury, remain unchanged under such conditions, the molecules of chlorine, bromine, and iodine, respectively, were split into two atoms, in conformity with Avogadro's surmise of the compound nature of elementary molecules.

In the same manner, the vapor density, and hence the molecular condition of the less volatile substances, zinc, thallium, antimony, and bismuth, was suc essfully determined at a white heat.

Careful research resulted in the exposure of the old fallacy of the existence of a sulphur molecule containing six atoms.

But how many of the problems which crowd around us at this point are for the time being entirely beyond the reach of the experimenter!

To-day pyro-chemical work is limited to a temperature of 1700° C., because vessels of porcelain and platinum, to the use of which we are limited, fuse above that temperature. The possibility of performing quantitative experiments at these temperatures seemed to us some years ago to be an unexpected progress, but to-day we complain that the trivial cause of a want of proper vessels forbids us to increase the temperature up to 2000° or 3000° C. There is no doubt that we should arrive at new unthought-of facts, that the splitting of still other elementary molecules would be possible, that a new chemistry would be revealed to us, if—being provided with vessels of infusible material, we could work at temperatures at which water vapor could not exist and at which detonating gas would be a non-inflammable mixture!

Let us now enter other fields of physical chemistry. Golden fruit, daily increasing, has been harvested upon this field during these latter days. Again we see van't Hoff take the lead. His keen eye has enabled us to penetrate the nature of *solution*, which forms the beginning of a new epoch in molecular physics. The quintessence of his discoveries may be thus expressed:

"Solutions of different substances in the same liquid, which contain in the same volume *an equal number of molecules* of the dissolved substance, show the same *osmotic pressure, the same vapor pressure, and the same freezing point.*"

This surprising generalization offers the possibility of determining *the true molecular weight of substances* by experimenting upon them in *solution*, while heretofore this has only been possible by transforming them into the gaseous state, hence only for volatile substances, since

dilute solutions behave in regard to the molecular state of the dissolved substance like *gases.*

In this manner new methods are given for the determination of molecular weights, which we are now able to determine by means of measurements relating to the *freezing point*, the *vapor pressure*, or the *osmotic pressure* of a solution of the substance to be tested.

These results are of the highest possible practical importance for chemistry, since they widen in an unexpected manner the possibility of the determination of molecular weights, and in a still higher degree we are surprised by the elucidation which they offer in regard to *the nature of solution.* Clausius had already admitted, within narrower limits, that in solutions of electrolytes some of the dissolved molecules were decomposed into their ions, but now this has been proved in a larger measure, particularly by Arrhenius. What a change our conceptions will have to undergo if we have to accustom ourselves to regard a dilute solution of sodium chloride as one containing, not undecomposed molecules of this salt, but separated atoms of sodium and chlorine!

We owe these revolutionizing innovations to the investigations of van't Hoff, Arrhenius, Ostwald, Planck and de Vrie, but in regard to experimental research especially to the splendid work of Raoult, which during recent years has effected this mighty theoretical progress.

Thus we see physical chemistry moving on in weighty development. Special laboratories are opened for her, and a special journal also has been started which is open alike to the records of experiment and to theoretical discussion. Through the foundation of this organ physical chemistry has been furthered in a most active manner. All the questions of the time and all those in dispute belonging to this department of science receive in this paper a thorough discussion. Dynamical-chemical questions are successfully studied, a significant impetus is given to the study of structure and affinity (widened as our knowledge of the nature of solutions has made necessary), by means of the study of the relations between chemical nature and electric conduction.

The inquiry into the intimate relations that exist between physical and chemical properties, which was inaugurated half a century ago by Hermann Kopp, is now being deepened and widened.

It is true that the great hopes which sprang from the study of thermo-chemical questions have so far been only partly fulfilled, but consecutive measurements offer more clearness also in this case.

There is no field of our science in which we may expect greater revolutions in the time near at hand than in that of physical chemistry! The value of these for general chemistry will be greater in proportion as the representatives of the same will recognize their task in this: Above all to remain upon the *chemical* standpoint and to improve *chemistry* by the application of *physical* modes of thought and experiment. Those who tried to further the progress of chemistry by the use of physical methods, but with insufficient consideration for chemical rela-

tions, have been led into serious errors. The respect due to work of the highest merit, continued for years, has thus been lessened. Apparently this has even been overdone, and it is much to be deplored if the interest of chemists for physical chemistry should be diminished because some of its representatives are inclined to over-rate the value of their results. He who swims in the midst of high waves is unable at times to see over the crests.

Innumerable, also, are the problems which meet us in the domain of organic chemistry.

After the astonishing harvest of synthetical results which has been reaped here, hardly any problem of synthesis seems unapproachable. Since the artificial preparation of alizarin by Graebe and Liebermann, of indigo by von Baeyer, of conine by Ladenburg, of uric acid by Horbaczewski and particularly by Behrend, since Emil Fischer and Kiliani have elucidated the chemistry of the sugar group and Wallach that of the terpenes, we may well look hopefully for a clearer knowledge of the bodies comprised under the name albumin, and to its synthesis.

But even such success tends only to render us more modest, since it shows us at the same time how narrow are the limits within which chemical synthesis moves. Assuming even that the preparation of albumin had been achieved, how infinitely far we should still be from a conception of the nature of *organized* bodies! Perhaps science is separated by an impassable chasm from the artificial preparation of a simple cell. Such an achievement lies at least beyond the sphere of chemistry.

But shall we really never succeed in sounding the process of *assimilation*, which, in spite of its simplicity, presents itself to us so enigmatically? Will it be found impossible to prepare artificially in our laboratories, from carbon dioxide and water, sugar and starch, a process which nature performs unceasingly in the green parts of plants?

The chemist however should not step prematurely upon the field of biology while so many great problems remain untouched in his own peculiar sphere of investigation.

The *method* of research in organic chemistry, in spite of the brilliant successes already recorded, forces us even to-day to confess that only a very minute proportion of known substances is within its reach. In order to isolate an organic substance we are generally confined to the purely accidental properties of *crystallization* or *volatilization*. Have not those thousands of amorphous substances which cannot be characterized by any chemical property and which the chemist is forced to lay aside because he is unable either to purify them or to transform them into volatile or crystallizable bodies,—have they not the same claim upon our interest as their more beautiful and more manageable comrades?

The most significant progress of organic chemistry does not consist in single discoveries, nor in further expansion of synthetical success.

What we want is : *new methods for recognizing the individuality of substances.* The black substances of earthy nature, the innumerable formless and resinous products in the bodies of plants and animals, the coloring matter which gives beauty to flowers, all of these to-day mock our efforts to know them; they will form a new and inexhaustible field for the prosecution of chemical research, when *methods* shall have been found with which to begin this research.

And as in organic chemistry, so in *mineral chemistry* every step leads to questions which we have as yet no means of answering. The synthesis of minerals and of rocks has made important progress, it is true, and this as well as the application of the doctrine of structure to the study of mineral species gradually leads to the understanding of their constitution; but we are as yet unable to use, in the study of minerals, the method of *analytical decomposition* which has been so successfully used to study the constitution of organic substances, and above all we lack the least knowledge in regard to the *true molecular weight* of minerals.

Quite recently we have been presented with no less than three new and fruitful methods for the determination of the molecular weight, but not *one* of them gives us an indication of the true molecular weight of the most simple oxides, such as silicic anhydride or calcium oxide.

We know to-day very well that silicic anhydride can not have the formula SiO_2, that this must be multiplied by a very large factor; but of the numerical value of this latter we have no indication. And thus also in mineral chemistry we must aim not exclusively at finding new *facts*, but *new methods of research* in the first place, if a period of new discoveries is to be attained in this branch of our science.

But how can we conclude this brief review without mentioning also the *applications of chemistry to the industrial arts*, the progress of which has mainly contributed to spread the splendor of our science most widely? The infinite variety of the tar colors, surpassing the colors of flowers in number and brightness, is daily increased by new discoveries. The technology of these dyes and pigments forms the most brilliant triumph of purely scientific laboratory work applied to manufactures. This industry in the simplest manner and on the largest scale performs the synthesis of compounds the complex nature of which is indicated by the names they bear. The unscientific man is frightened when a beautiful and brilliant dye is referred to as *Hexamethylmethoxytriamidotriphenylcarbinol;* for the initiated there lies in this unpleasant name a full account of the synthesis and the constitution of the dye.

Industry has learned to derive not only colors, but healing medicines also from coal tar. Antipyrin, discovered by Knorr, upon the basis of Emil Fischer's fundamental research upon the hydrazines, brings to thousands suffering from fever, relief at least—if not cure. Let us hope that the time is not far distant when *real* fever curatives, which like the natural alkaloids of the cinchona bark, not only temporarily *sup-*

press the disease, but really *cure* it, may be prepared by synthesis. Until then be patient and do not chide chemistry if, for the time being, she offers only silver instead of gold.

Events in this field of the great chemical industries are significant. We are the witnesses of a great combat taking place between the older process of Le Blanc for the preparation of soda and the new one of Solvay called the ammonia-soda process. The intelligence and inventive genius of manufacturers have added under the pressure of this competition a large number of improvements to the manufacture of sulphuric acid and of soda, and new and valuable methods for the preparation of chlorine. Here, more than in any other branch of chemical industry, the struggle for existence is fierce.

The manufacture of iron, that most important chemical industry, is transformed by innovations. The imposing changes wrought by the older process of Bessemer, by the new one of Thomas, are they not based purely upon chemical reactions? The grandest application of a a complicated chemical reaction to a great manufacture is, perhaps, the dephosphorizing of pig-iron by lining the Bessemer converter with basic material, an invention which we owe to Thomas and Gilchrist. From this again, agriculture derives an advantage in the use of the Thomas slag containing the phosphorus which heretofore rendered iron ore less valuable. This then is truly a transformation of stone into bread, similar to the older manufacture of soluble fertilizers from mineral phospbates. Nevertheless, the era of bliss which was prophesied three years ago at the Berlin meeting of naturalists by our illustrious colleague, Ferdinand Cohn, has not yet dawned. He held that all struggles for existence amongst men, arising from want of food, (the bread question,) will be done away with, when chemistry shall have learned to prepare starch from carbon dioxide and water. But since time immemorial the farmer is occupied in this very chemical industry, and it would hardly be great progress if the farm were merely replaced by a chemical factory. But we may reasonably hope that chemistry will teach us *to make the fiber of wood a source of human food.*

Indeed, if we consider how small is the quantity of starch which the grain furnishes us, and further that the wood fiber has exactly the same chemical composition as starch, we see the possibility of increasing the production of food indefinitely by solving this problem: *To transform cellulose into starch.*

If this problem were solved we should find an inexhaustible source of human food in the wood of our forests, in grass, and even in straw and chaff. The beautiful researches of Hellriegel have recently disclosed the fact, which in former times was disputed, that certain plants transform atmospheric nitrogen into albumin and that this process can be improved by suitable treatment.

The increase of albumin in plants, according to a plan, together with the production of starch out of cellulose—this would in reality signify the abolition of the bread question.

May it some day be granted to chemistry through such a discovery to inaugurate a golden age for humanity.

I have tried to give a review of the most important problems which are set before chemical science. I have mentioned a goodly number, but the short time of one hour permits me to touch but slightly upon the greater ones. There are so many problems before us, which await an immediate solution as to justify what I said in the beginning; that to-day the chemist has no time to complain because the epoch of a mathematical treatment of his science has not yet arrived.

Nevertheless, the brilliant successes which have been gained, the wonderful results which are immediately within our reach, have not the power to turn our eyes from this final problem.

The Newton prophesied to Chemistry by Emil du Bois Reymond, may he appear at a later period; until he comes, may many a generation honorably plow on in the sweat of its brow! We must remember that nature is not understood by us until we are able to reduce its phenomena to simple movements, mathematically traceable.

The time will come, even for chemistry, when this highest kind of treatment will prevail. The epoch in which the foremost impulse of its research was a serenely creative imagination will then have passed; the joys, but also the pangs and struggles, peculiar to youth, will have been overcome.

Re-united to Physics, her sister science, from whom her ways at present are separated, Chemistry will run her course with firm and unfaltering steps.

THE PHOTOGRAPHIC IMAGE.*

By Prof. RAPHAEL MELDOLA, F. R. S.

The history of a discovery which has been developed to such a remarkable degree of perfection as photography has naturally been a fruitful source of discussion among those who interest themselves in tracing the progress of science. It is only my presence in this lecture theater, in which the first public discourse on photography was given by Thomas Wedgwood at the beginning of the century, that justifies my treading once again a path which has already been so thoroughly well beaten. If any further justification for trespassing upon the ground of the historian is needed, it will be found in the circumstance that in the autumn of last year there was held a celebration of what was generally regarded as the jubilee of the discovery. This celebration was considered by many to have reference to the public disclosure of the Daguerreotype process, made through the mouth of Arago to the French Academy of Sciences on August 10, 1839. There is no doubt that the introduction of this process marked a distinct epoch in the history of the art, and gave a great impetus to its subsequent development. But while giving full recognition to the value of the discovery of Daguerre, we must not allow the work of his predecessors and contemporaries in the same field to sink into oblivion. After the lapse of half a century we are in a better position to consider fairly the influence of the work of different investigators upon modern photographic processes.

I have not the least desire on the present occasion to raise the ghosts of dead controversies. In fact, the history of the discovery of photography is one of those subjects which can be dealt with in various ways, according to the meaning assigned to the term. There is ample scope for the display of what Mr. Herbert Spencer calls the "bias of patriotism." If the word "photography" be interpreted literally as writing or inscribing by light, without any reference to the subsequent permanence of the inscription, then the person who first intentionally caused a design to be imprinted by light upon a photo-sensitive compound must be regarded as the first photographer. According to Dr. Eder, of Vienna, we must place this experiment to the credit of Johann

* Friday evening lecture delivered at the Royal Institution, on May 16, 1890. (From *Nature* July 10, 1890, vol. XLII, pp. 246–250.)

Heinrich Schulze, the son of a German tailor, who was born in the Duchy of Madgeburg, in Prussia, in 1687, and who died in 1744, after a life of extraordinary activity as a linguist, theologian, physician, and philosopher. In the year 1727, when experimenting on the subject of phosphorescence, Schulze observed that by pouring nitric acid, in which some silver had previously been dissolved, on to chalk, the undissolved earthy residue had acquired the property of darkening on exposure to light. This effect was shown to be due to light, and not to heat. By pasting words cut out in paper on the side of the bottle containing his precipitate, Schulze obtained copies of the letters on the silvered chalk. The German philosopher certainly produced what might be called a temporary photogram. Whatever value is attached to this observation in the development of modern photography, it must be conceded that a considerable advance was made by spreading the sensitive compound over a surface instead of using it in mass. It is hardly necessary to remind you here that such an advance was made by Wedgwood and Davy in 1802.* The impressions produced by these last experimenters were unfortunately of no more permanence than those obtained by Schulze three quarters of a century before them.

It will perhaps be safer for the historian of this art to restrict the term photograph to such impressions as are possessed of permanence. I do not of course mean absolute permanence, but ordinary durability in the common-sense acceptation of the term. From this point of view the first real photographs, *i. e.*, permanent impressions of the camera picture, were obtained on bitumen films by Joseph Nicéphore Niepce, of Châlons-sur-Sâone, who, after about 20 years' work at the subject, had perfected his discovery by 1826. Then came the days of silver salts again, when Daguerre, who commenced work in 1824, entered into a partnership with Niepce in 1829, which was brought to a termination by the death of the latter in 1833. The partnership was renewed between Daguerre and Niepce de St. Victor, nephew of the elder Niepce. The method of fixing the camera picture on a film of silver iodide on a silvered copper plate—the process justly associated with the name of Daguerre—was ripe for disclosure by 1838, and was actually made known in 1839.

The impartial historian of photography who examines critically into the evidence will find that quite independently of the French pioneers experiments on the use of silver salts had been going on in this country, and photographs, in the true sense, had been produced almost simultaneonsly with the announcement of the Daguerreotype process by two Englishmen whose names are as household words in the ranks of science. I refer to William Henry Fox Talbot and Sir John Herschel. Fox Talbot commenced experimenting with silver salts on paper in

* "An Account of a Method of Copying Paintings upon Glass, and of making Profiles by the Agency of Light upon Nitrate of Silver. Invented by T. Wedgwood, Esq. With Observations by H. Davy." *Journ. Royal Institution*, 1802, p. 170.

1834, and the following year he succeeded in imprinting the camera picture on paper coated with the chloride. In January, 1839, some of his "photogenic drawings"—the first "silver prints" ever obtained—were exhibited in this Institution by Michael Faraday. In the same month he communicated his first paper on a photographic process to the Royal Society, and in the following month he read a second paper before the same society, giving the method of preparing the sensitive paper and of fixing the prints. The outcome of this work was the "Calotype" or Talbotype process, which was sufficiently perfected for portraiture by 1840, and which was fully described in a paper communicated to the Royal Society in 1841. The following year Fox Talbot received the Rumford medal for his "discoveries and improvements in photography."*

Herschel's process consisted in coating a glass plate with silver chloride by subsidence. The details of the method, from Herschel's own notes, have been published by his son, Prof. Alexander Herschel.† By this means, the old 40-foot reflecting telescope at Slough was photographed in 1839. By the kindness of Professor Herschel, and with the sanction of the Science and Art Department, Herschel's original photographs have been sent here for your inspection. The process of coating a plate by allowing a precipitate to settle on it in a uniform film is however impracticable, and was not further developed by its illustrious discoverer. We must credit him however as being the first to use glass as a sub-stratum. Herschel further discovered the important fact that while the chloride was very insensitive alone, its sensitiveness was greatly increased by washing it with a solution of silver nitrate. It is to Herschel also that we are indebted for the use of sodium thiosulphate as a fixing agent, as well as for many other discoveries in connection with photography which are common matters of history.

Admitting the impracticability of the method of subsidence for producing a sensitive film, it is interesting to trace the subsequent development of the processes inaugurated about the year 1839. The first of photographic methods—the bitumen process of Niepce—survives at the present time, and is the basis of some of the most important of modern photo-mechanical printing processes. [Specimens illustrating photo-etching from Messrs. Waterlow & Sons exhibited.] The Daguerreotype process is now obsolete. As it left the hands of its inventor it was unsuited for portraiture on account of the long exposure required. It is evident moreover that a picture on an opaque metallic plate is incapable of re-production by printing through, so that in this respect the Talbotype possessed distinct advantages. This is one of the most important points in Fox Talbot's contributions to photogra-

* For these and other details relating to Fox Talbot's work, necessarily excluded for want of time, I am indebted to his son, Mr. C. H. Talbot, of Lacock Abbey.

† *Photog. Journ. and Trans. Photog. Soc.* June 15, 1872.,

phy. He was the first to produce a transparent paper negative from which any number of positives could be obtained by printing through. The silver print of modern times is the lineal descendant of the Talbotype print. After 40 years' use of glass as a substratum we are going back to Fox Talbot's plan, and using thin flexible films—not exactly of paper, but of an allied substance—celluloid. [Specimens of Talbotypes, lent by Mr. Crookes, exhibited, with celluloid negatives by the Eastman Company.]

If I interpret this fragment of history correctly, the founders of modern photography are the three men whose labors have been briefly sketched. The jubilee of last autumn marked a culminating point in the work of Niepce and Daguerre and of Fox Talbot. The names of these three pioneers must go down to posterity as coequal in the annals of scientific discovery. [Portraits by Mr. H. M. Elder shown.] The lecture theater of the Royal Institution offers such tempting opportunities to the chronicler of the history of this wonderful art that I must close this treatment of the subject by reminding myself that in selecting the present topic I had in view a statement of the case of modern photography from its scientific side only. There is hardly any invention associated with the present century which has rendered more splendid services in every department of science. The physicist and chemist, the astronomer and geographer, the physiologist, pathologist, and anthropologist will all bear witness to the value of photography. The very first scientific application of Wedgwood's process was made here by the illustrious Thomas Young, when he impressed Newton's rings on paper moistened with silver nitrate, as described in his Bakerian lecture to the Royal Society on November 24, 1803. Professor Dewar has just placed in my hands the identical slide, with the Newton rings still visible, which he believes Young to have used in this classic experiment. [Shown.]

Our modern photographic processes depend upon chemical changes wrought by light on films of certain sensitive compounds. Bitumen under this influence becomes insoluble in hydro-carbon oils, as in the heliographic process of the elder Niepce. Gelatine mixed with potassium dichromate becomes insoluble in water on exposure to light, a property utilized in the photo-etching process introduced in 1852 by Fox Talbot, some of whose original etchings have been placed at my disposal by Mr. Crookes. [Shown.] Chromatized gelatine now plays a most important part in the autotype and many photo-mechanical processes. The salts of iron in the ferric condition undergo reduction to the ferrous state under the influence of light in contact with oxidizable organic compounds. The use of these iron salts is another of Sir John Herschel's contributions to photography (1842), the modern "blue print" and the beautiful platinotype being dependent on the photo-reducibility of these compounds. [Cyanotype print developed with ferricyanide.]

Of all the substances known to chemistry at the present time, the salts of silver are by far the most important in photography on account of the extraordinary degree of sensitiveness to which they can be raised. The photographic image with which it is my privilege to deal on this occasion is that invisible impression produced by the action of light on a film of a silver haloid. Many methods of producing such films have been in practical use since the foundation of the art in 1839. All these depend on the double decomposition between a soluable chloride, bromide, or iodide, and silver nitrate, resulting in the formation of the silver haloid in a vehicle of some kind, such as albumen (Niepce de St. Victor, 1848), or collodion on glass, as made practicable by Scott Archer in 1851. For 20 years this collodion process was in universal use; its history and details of manipulation, its development into a dry plate process by Colonel Russell in 1861, and into an emulsion process by Bolton and Sayce in 1864, are facts familiar to every one.

The photographic film of the present time is a gelatino-haloid (generally bromide) emulsion. If a solution of silver nitrate is added to a solution of potassium bromide and the mixture well shaken, the silver bromide coagulates and rapidly subsides to the bottom of the liquid as a dense curdy precipitate. [Shown.] If instead of water we use a viscid medium, such as gelatine solution, the bromide does not settle down, but forms an emulsion, which becomes quite homogeneous on agitation. [Shown.] This operation, omitting all details of ripening, washing, etc., as well known to practical photographers, is the basis of all the recent photographic methods of obtaining negatives in the camera. The use of this invaluable vehicle, gelatine, was practically introduced by R. L. Maddox in 1871, previous experiments in the same direction having been made by Gaudin (1853–61). Such a gelatino-bromide emulsion can be spread uniformly over any sub-stratum—glass, paper, gelatine, or celluloid—and when dry gives a highly sensitive film.

The fundamental problem which 50 years' experience with silver haloid films has left in the hands of chemists is that of the nature of the chemical change which occurs when a ray of light falls on such a silver salt. Long before the days of photography, far back in the sixteenth century, Fabricius, the alchemist, noticed that native horn silver became colored when brought from the mine and exposed. The fact presented itself to Robert Boyle in the seventeenth century, and to Beccarius, of Turin, in the eighteenth century. The change of color undergone by the chloride was first shown to be associated with chemical decomposition in 1777 by Scheele, who proved that chlorine was given off when this salt darkened under water. I can show you this in a form which admits of its being seen by all. [Potassium iodide and starch paper were placed in a glass cell with silver chloride, and the arrangement exposed to the electric light till the paper had become blue.] The gas which is given

off under these circumstances is either the free halogen or an oxide or acid of the halogen, according to the quantity of moisture present and the intensity of the light. I have found that the bromide affects the iodide and starch paper in the same way, but silver iodide does not give off any gas which colors the test paper. All the silver haloids become colored on exposure to light, the change being most marked in the chloride, less in the bromide, and least of all in the iodide. The latter must be associated with some halogen absorbent to render the change visible. [Strips of paper coated with the pure haloids, the lower halves brushed over with silver nitrate solution, were exposed.] The different degrees of coloration in the three cases must not be considered as a measure of the relative sensitiveness; it simply means that the products of photo-chemical change in the three haloids are inherently possessed of different depths of color.

From the fact that halogen in some form is given off, it follows that we are concerned with photo-chemical decomposition, and not with a physical change only. All the evidence is in favor of this view. Halogen absorbents, such as silver nitrate on the lower halves of the papers in the last experiment, organic matter, such as the gelatine in an emulsion, and reducing agents generally, all accelerate the change of color. Oxidizing and halogenizing agents, such as mercuric chloride, potassium dichromate, etc., all retard the color change. [Silver chloride paper, painted with stripes of solutions of sodium sulphite, mercuric chloride, and potassium dichromate, was exposed.] It is impossible to account for the action of these chemical agents, except on the view of chemical decomposition. The ray of light falling upon a silver haloid must be regarded as doing chemical work; the vibratory energy is partly spent in doing the work of chemical separation, and the light passes through a film of such haloid partly robbed of its power of doing similar work upon a second film. It is difficult to demonstrate this satisfactorily in the lecture room on account of the opacity of the silver haloids, but the work of Sir John Herschel, J. W. Draper, and others has put it beyond doubt that there is a relationship of this kind between absorption and decomposition. It is well known also that the more refrangible rays are the most active in promoting the decomposition in the case of the silver haloids. This was first proved for the chloride by Scheele, and is now known to be true for the other haloids. It would be presumption on my part in the presence of Captain Abney to enlarge upon the effects of the different spectral colors on these haloids, as this is a subject upon which he can speak with the authority of an investigator. It only remains to add that the old idea of a special "actinic" force at the more refrangible end of the spectrum has long been abandoned. It is only because the silver haloids absorb these particular rays that the blue end of the spectrum is most active in promoting their decomposition. Many other instances of photo-chemical decomposition are known in which the less refrangible rays are the most

active, and it is possible to modify the silver haloids themselves so as to make them sensitive for the red end of the spectrum.

The chemical nature of the colored products of photo-chemical decomposition is still enshrouded in mystery. Beyond the fact that they contain less halogen than the normal salt, we are not much in advance of the knowledge bequeathed to us by Scheele in the last century. The problem has been attacked by chemists again and again, but its solution presents extraordinary difficulties. These products are never formed—even under the most favorable conditions of division and with prolonged periods of exposure—in quantities beyond what the chemist would call "a mere trace." Their existence appears to be determined by the great excess of unaltered haloid with which they are combined. Were I to give free rein to the imagination I might set up the hypothesis that the element silver is really a compound body invariably containing a minute percentage of some other element which resembles the compound which we now call silver in all its chemical reactions, but alone is sensitive to light. I offer this suggestion for the consideration of the speculative chemist.* For the colored product as a whole, *i. e.*, the product of photo-decomposition with its combined unchanged haloid, Carey Lea has proposed the convenient term "photo-salt." It will avoid circumlocution if we adopt this name. The photo-salts have been thought at various times to contain metallic silver, allotropic silver, a sub-haloid, such as argentous chloride, etc., or an oxy-haloid. The free-metal theory is disposed of by the fact that silver chloride darkens under nitric acid of sufficient strength to dissolve the metal freely. The acid certainly retards the formation of the photo-salt, but does not prevent it altogether. When once formed the photo-chloride is but slowly attacked by boiling dilute nitric acid, and from the dry photo-salt mercury extracts no silver. The assumption of the existence of an allotropic form of silver insoluble in nitric acid can not be seriously maintained. The sub-haloid theory of the product may be true, but it has not yet been established with that precision which the chemist has a right to demand. We must have analyses giving not only the percentage of halogen, but also the percentage of silver, in order that it may be ascertained whether the photo-salt contains anything besides metal and halogen. The same may be said of the oxy-haloid theory; it may be true, but it has not been demonstrated.

The oxy-haloid theory was first suggested by Robert Hunt† for the

* I have gone so far as to test this idea experimentally in a preliminary way, the result being, as might have been anticipated, negative. Silver chloride, well darkened by long exposure, was extracted with a hot saturated solution of potassium chloride, and the dissolved portion, after precipitation by water, compared with the ordinary chloride by exposure to light. Not the slightest difference was observable either in the rate of coloration or in the colors of the products. Perhaps it may be thought worth while to repeat the experiment, using a method analogous to the "method of fractionation" of Crookes.

† "Researches on Light," 2d ed., 1854, p. 80.

chloride; it was taken up by Sahler, and has recently been revived by Dr. W. R. Hodgkinson. It has been thought that this theory is disposed of by the fact that the chloride darkens under liquids, such as hydro-carbons, which are free from oxygen. I have been repeating some of these experiments with various liquids, using every possible precaution to exclude oxygen and moisture; dry silver chloride heated to incipient fusion has been sealed up in tubes in dry benzene, petroleum, and carbon tetrachloride, and exposed since March. [Tubes shown.] In all cases the chloride has darkened. The salt darkens moreover in a Crookesian vacuum.* By these experiments the oxy-chloride theory may be scotched, but it is not yet killed; the question now presents itself, whether the composition of the photo-salt may not vary according to the medium in which it is generated. Analogy sanctions the supposition that when the haloid darkens under water or other oxygen-containing liquid, or even in contact with moist or dry air, that an oxychloride may be formed and enter into the composition of the photo-salt. The analogy is supplied by the corresponding salt of copper, viz, cuprous chloride, which darkens rapidly on exposure. [Design printed on flat cell filled with cuprous chloride by exposure to electric light.] Wöhler conjectured that the darkened product was an oxychloride, and this view receives a certain amount of indirect support from these tubes [shown], in which dry cuprous chloride has been sealed up in benzene and carbon tetrachloride since March; and although exposed in a southern window during the whole of that time the salt is as white as when first prepared. Some cuprous chloride sealed up in water and exposed for the same time is now almost black. [Shown.]

When silver is precipitated by reduction in a finely divided state in the presence of the haloid, and the product treated with acids, the excess of silver is removed and colored products are left which are somewhat analogous to the photo-salts proper. These colored haloids are also termed by Carey Lea photo-salts because they present many analogies with the colored products of photo-chemical change. Whether they are identical in composition it is not yet possible to decide, as we have no complete analyses. The first observations in this direction were published more than 30 years ago in a report by a British Association Committee,† in which the red and chocolate-colored chlorides are dis-

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* Some dry silver chloride which Mr. Crookes has been good enough to seal up for me in a high vacuum darkens on exposure quite as rapidly as the dry salt in air. It soon regains its original color when kept in the dark. It behaves, in fact, just as the chloride is known to behave when sealed up in chlorine, although its color is of course much more intense after exposure than is the case with the chloride in chlorine.

† These results were arrived at in three ways. In one case hydrogen was passed through silver citrate suspended in hot water, and the product extracted with citric acid. "The result of treating the residue with chloro-hrydic acid, and then dissolving the silver by dilute nitric acid, was a rose-tinted chloride of silver." In another experiment the dry citrate was heated in a stream of hydrogen at 212° F., and the product, which was partly soluble in water, gave a brown residue, which furnished "a

tinctly described. Carey Lea has since contributed largely to our knowledge of these colored haloids, and has made it appear at least highly probable that they are related to the products formed by the action of light. [Red photo-chloride and purple photo-bromide and iodide shown.]

The photographic image is impressed on a modern film in an inappreciable fraction of a second, whereas the photo-salt requires an appreciable time for its production. The image is invisible simply because of the extremely minute quantity of haloid decomposed. In the present state of knowledge it can not be asserted that the material composing this image is identical in composition with the photo-salt, for we know the composition of neither the one nor the other. But they are analogous in so far as they are both the result of photo-chemical decomposition, and there is great probability that they are closely related, if not identical, chemically. It may turn out that there are various kinds of invisible images, according to the vehicle or halogen absorbent—in other words, according to the sensitizer with which the silver haloid is associated. The invisible image is revealed by the action of the developer, into the function of which I do not propose to enter. It will suffice to say that the final result of the developing solution is to magnify the deposit of photo salt by accumulating metallic silver thereon by accretion or reduction. Owing to the circumstance that the image is impressed with such remarkable rapidity, and that it is invisible when formed, it has been maintained, and is still held by many, that the first action of light on the film is molecular or physical, and not chemical. The arguments in favor of the chemical theory appear to me to be tolerably conclusive, and I will venture to submit a few of them.

The action of reagents upon the photographic film is quite similar to the action of the same reagents upon the silver haloids when exposed to the point of visible coloration. Reducing agents and halogen absorbents increase the sensitiveness of the film: oxidizing and halogenizing agents destroy its sensitiveness. It is difficult to see on the physical theory why it should not be possible to impress an image on a film, say of pure silver bromide, as readily as on a film of the same haloid imbedded in gelatine. Everyone knows that this can not be done. I have myself been surprised at the extreme insensitiveness of films of pure bromide prepared by exposing films of silver deposited on glass to the action of bromine vapor. On the chemical theory we

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very pale red body on being transformed by chlorhydic and nitric acids." In another experiment silver arsenite was formed, this being treated with caustic soda, and the black precipitate then treated successively with chlorhydic and nitric acids: "Silver is dissolved, and there is left a substance - - - [of] a rich chocolate or maroon, etc." This on analysis was found to contain 24 per cent. of chlorine, the normal chloride requiring 24.74 and the subchloride 14.08 per cent. The committee which conducted these experiments consisted of Messrs. Maskelyne, Hadow, Hardwick, and Llewelyn. B. A. Rep., 1859, p. 103.

know that gelatine is a splendid sensitizer—*i. e.*, bromine absorbent. There is another proof which has been in our hands for nearly 30 years, but I do not think it has been viewed in this light before. It has been shown by Carey Lea, Eder, and especially by Abney, who has investigated the matter most thoroughly, that a shearing stress applied mechanically to a sensitive film leaves an impression which can be developed in just the same way as though it had been produced by the action of light. [Pressure marks on Eastman bromide paper developed by ferrous oxalate.] Now that result can not be produced on a surface of the pure haloid; some halogen absorbent, such as gelatine, must be associated with the haloid. We are concerned here with a chemical change of that class so ably investigated by Professor Spring, of Liége, who has shown that by mere mechanical pressure it is possible to bring about chemical reaction between mixtures of finely divided solids.* Then again, mild reducing agents, too feeble to reduce the silver haloids directly to the metallic state, such as alkaline hypophosphites, glucose or lactose and alkali, etc., form invisible images which can be developed in precisely the same way as the photographic image. All this looks like chemical change, and not physical modification pure and simple.

I have in this discourse stoically resisted the tempting opportunities for pictorial display which the subject affords. My aim has been to summarize the position in which we find ourselves with respect to the invisible image after fifty years' practice of the art. This image is, I venture to think, the property of the chemist, and by him must the scientific foundation of photography be laid. We may not be able to give the formula of the photo-salt, but if the solution of the problem has hitherto eluded our grasp it is because of the intrinsic difficulties of the investigation. The photographic image brings us face to face—not with an ordinary, but with an extraordinary class of chemical changes due entirely to the peculiar character of the silver salts. The material composing the image is not of that definite nature with which modern chemical methods are in the habit of dealing. The stability of the photosalt is determined by some kind of combination between the subhaloid or oxy-haloid, or whatever it may be, and the excess of unaltered haloid which enters into its composition. The formation of the colored product presents certain analogies with the formation of a saturated solution; the product of photo-chemical decomposition is formed under the influence of light up to a certain percentage of the whole photo-salt, beyond which it can not be increased,—in other words, the silver haloid is saturated by a very minute percentage of its own product of photodecomposition. The photo-salt belongs to a domain of chemistry—a no-

* The connection between the two phenomena was suggested during a course of lectures delivered by me two years ago ("Chemistry of Photography," p. 191). I have since learnt that the same conclusion had been arrived at independently by Mr. C. H. Bottamley, of the Yorkshire College, Leeds.

man's land—peopled by so-called "molecular compounds," into which the pure chemist ventures but timidly. But these compounds are more and more urging their claims for consideration, and sooner or later they will have to be reckoned with, even if they lack that definiteness which the modern chemist regards as the essential criterion of chemical individuality. The investigation may lead to the recognition of a new order of chemical attraction, or of the old chemical attraction in a different degree. The chemist who discourses here upon this subject at the end of the half century of photography into which we have now entered will no doubt know more about this aspect of chemical affinity; and if I may invoke the spirit of prophecy in concluding, I should say that a study of the photographic film with its invisible image will have contributed materially to its advancement.

A TROPICAL BOTANIC GARDEN.*

BY M. TREUB.

A short time ago botanic gardens were arraigned by the rector of one of the largest universities of Europe in a serious discourse. The orator, a celebrated phyto-physiologist, complained that these gardens no longer keep pace with the botanical science of the day. In the middle ages and until the middle of the sixteenth century botanic gardens were collections of officinal plants. Since that period they have become truly scientific institutions. Abandoning pure speculation, attention was given to living things themselves, particularly to plants. Patrons and scientists combined their efforts to bring from the most distant countries rare or unknown specimens. In the gardens, depositories of this wealth, the difficult task was attempted of presenting, on a reduced scale, the entire vegetable world, and of bringing together (as far as possible), all existing vascular plants. In spite of the constantly increasing number of plants introduced into Europe, this general plan was for a long time followed, and not until the beginning of the present century, was it felt that the method must be changed. In the first place it should have been recognized that it was impossible to collect in a single garden, however large and well managed, anything like the enormous number of vascular plants distributed on our globe. Besides, (and this is a more serious argument,) the conditions offered to introduce plants in gardens are so far from natural, that exotic cultivated plants can not be considered as furnishing a proper basis of comparison in scientific researches, as these are at present understood. Too many plants in conditions too abnormal is briefly the criticism made by the orator.

These institutions, attacked from so high a place, have not failed of defenders. While recognizing that part of the criticism is well founded, it is urged that if the object in view was varied somewhat by insisting —more than has heretofore been done—upon the adoption of a common plan, the botanic gardens of Europe would easily avoid the dangers with which they are menaced. It is not necessary that we take any part in this controversy, for the objections—whether well-founded or not—do not apply to botanic gardens of the tropics, as we will endeavor to show in the following pages.

* Translated from the *Revue des Deux Mondes.* January 1, 1890, vol. XCVII, pp. 162-183.

The number of botanic gardens situated in the tropical zone is much greater than might be supposed. According to a recent enumeration there are not less than fifteen in the British possessions. In the French colonies they are found at St. Denis in Reunion Island, at La Point-à-Pître in Guadeloupe Island, at St. Pierre in Martinique, at Pondicherry, and at Saigon. Spain has one at Havana, and one at Manila; and Holland has a single one at Buitenzorg in the island of Java. There are also tropical botanic gardens in South America, and these bring the total number to a considerable figure. Still it must be admitted that some are not botanic gardens properly so-called, but rather agricultural stations and gardens of acclimation. There are others however, that while not abandoning tropical agriculture, merit the names of great scientific establishments. As the chief of this kind, those of Calcutta, of Buitenzorg in Java, and of Peradeniya in Ceylon (in chronological order) should be cited.

The royal garden of Calcutta was founded in 1786 by Col. Robert Hyde, who was its first director. Among his successors are found the celebrated names of Wallich and Griffith, the greatest naturalist of our century in the extreme East. The garden of Calcutta has now been for several years under the wise and able direction of Dr. G. King, to whose care the herbarium of Calcutta owes its great reputation. The royal garden of Peradeniya in the Island of Ceylon was founded in 1821. Situated near Kandy, at an altitude of nearly 500 metres [1,600 feet], having a moist and hot climate, occupying more than 60 hectares [150 acres], and connected as it is with the post of Colombo by a railway, the garden of Peradeniya possesses conditions most favorable in every respect. For many years it was under the direction of Dr. Thwaites, a man of real merit, but who thought a botanic garden in a tropical country should be in some manner a reduced copy of the virgin forest. This system, more original than meritorious, excludes any methodical arrangement of plants and necessarily restricts the number of specimens. Dr. H. Trimen, the successor of Dr. Thwaites, as soon as he arrived in Ceylon, 9 years ago, realized the disadvantages of the plan of his predecessor. To distribute over an area of 60 hectares, without any order, a great number of plants, for the most part not labelled, was to fatally embarrass the scientific use of the rich collections that had been brought together. So Dr. Trimen did not hesitate to adopt a new arrangement of plants according to the natural system and to label them as far as it was possible to do so. With branch establishments upon the plain and upon the mountain, the garden of Peradeniya has before it a brilliant future. The third of the gardens mentioned, that of Buitenzorg in the island of Java, was founded in 1817. We will briefly relate its history and show by a study of its present organization that a new era is commencing for large tropical gardens, and that their influence will constantly increase in the future evolution of the science of plants.

I.

On the 29th of October, 1815, a squadron quitting the roadstead of Texel in the north of Holland set sail for the East Indies. The passengers (for they carried them upon these ships of war), must have rejoiced that they had left the storms and fogs of the North Sea for the sunny coasts of Malaysia. The squadron took to Java the commissioners-general to whom the sovereign of Holland had committed the task of assuming in his name the government of the Dutch East Indies. Being a man of broad views, the new king had attached to the commission a distinguished naturalist, Reinwardt, professor in the Atheneum of Amsterdam, in order that the study of the marvellous natural products which constitute the wealth of the Dutch possessions in the south of Asia might be settled upon a solid basis.

The squadron did not enter the straits of Sunda until the last of April in the following year. The high functionaries, sailing after a long voyage between charming islets, set like emeralds in thin silver fillets of breakers, breathing the faint odors from the neighboring coasts, must at last land and take up their task. The future indeed reserved for them many disappointments, and it was only after long and tedious diplomatic manœuvers that the English authorities, on the 19th of August, 1816, decided to turn over to the plenipotentiaries of the king of Holland the rule of the Dutch Indies. Baron Van der Capellen the commissioner who was to perform the functions of governor-general shortly installed himself at Buitenzorg, taking Reinwardt with him.

Buitenzorg, the residence of the viceroy of the Dutch Indies, is situated 58 kilometres [36 miles] from Batavia, in 106° 53′ 5″ east longitude and 6° 35′ 8″ south latitude, upon one of the long ridges that extend to the north of the great mountain of Salak. An enchanting site, possessing a beautiful and healthful climate, it is not surprising that the governors-general established themselves there instead of at Batavia, however large and beautiful that "city of villas" might be. This preference, accorded to Buitenzorg by the representatives of the king, was the cause of the creation of a botanical establishment at that point. In fact, upon the request of Reinwardt, the commissioners-general decided—by a decree of April 15, 1817—to found a botanic garden at Buitenzorg upon an uncultivated territory belonging to the domain and ceded by Baron Van der Capellen. On this territory, contiguous to the park and to the palace garden, work was commenced on the 15th of May by some fifty native workmen, under the direction of two chief gardeners, one of whom, brought out by Reinwardt, had been employed in the same capacity in Holland, while the other was a pupil of the royal garden of Kew. It would have been difficult to find in the whole island of Java a place more appropriate for a garden of this kind, for owing to certain conditions, Buitenzorg unites to other advantages that of having no dry season, properly speaking. It is evident that

only a small number of plants could endure a period of almost continuous drought for 4 or 5 months, such as is habitual to the east of Java. Even the climate of Batavia, where 2 or 3 months without heavy rains are not rare, would be less suitable for a botanic garden than Buitenzorg, where they complain if in the middle of the dry season, rain is absent for 3 consecutive weeks. These frequent and heavy rains are doubly advantageous for the garden; Buitenzorg owes to them its ever luxuriant vegetation (never ceasing, as one may say), and they cause a lowering of the mean temperature which makes it possible to cultivate many plants from the virgin forests of the mountains, although the altitude of Buitenzorg is only 280 metres [900 feet]. In order to give an idea of the mass of water which is ordinarily shed upon the "*Sans Souci*" of Java,* it will be sufficient to say that at Buitenzorg there falls a mean quantity of 4,600 millimetres [180 inches] of rain per year, while in Holland, one of the most rainy countries of Europe, there falls per year but 660 millimetres [26 inches]. No settled plan was at first adopted, and the archives contain no indication of any kind relative to the earliest management of the garden. We merely know that its founder, Reinwardt, took advantage of many voyages made by him to send plants to Buitenzorg. Yet the first catalogue of the "Botanic Garden of the State," the name officially adopted, published some months after the departure of Reinwardt, enumerates only 912 species of plants. Reinwardt returned to Europe in June, 1822, to occupy a chair in the University of Leyden. Upon his recommendation the Government placed at the head of the garden a botanist of exceptional merit, Dr. C. L. Blume, who thus became the first director of the "*Hortus Bogoriensis*,"† and whose scientific renown was cradled in the garden at Buitenzorg. Blume displayed a remarkable activity as director. He commenced in 1825 the publication of a work upon the flora of Dutch India; with a feverish activity he brought out during 1825 and the early part of 1826, seventeen parts, describing more than 1,200 new species, a great number of genera, and several families of plants entirely unknown up to that time. The garden profited directly from the work of Blume, because the collection of living plants was enriched by a numerous series of species discovered by him. On the other hand, Blume succeeded in attaching to the garden, besides a considerable force and the two chief gardeners, a third European gardener, and a draftsman. In short, the young institution came out brilliantly in every respect, and it seemed to promise a remarkable future. A cruel reverse however soon proved the uncertainty of these favorable prognostications. Blume, after having nearly broken down, had to return to Europe in 1826, to re-establish his health. Almost at the same time Baron Van der Capellen was re-placed by the Viscount

* [The literal translation of the word Buitenzorg is without (beyond) care.]

† *Hortus Bogoriensis*, the scientific name of the garden, is derived from Bogor, the native name of Buitenzorg.

du Bus de Gisignies. The former had neglected nothing to stimulate the colony, but in doing this, grand seigneur that he was, he had no thought of cost. So Du Bus was sent out as commissioner-general, with an order to diminish the expenses, and to re-establish the balance of the colonial budget. He executed the orders received, and the expenses were immediately reduced, but how many useful institutions were nearly or quite suppressed! The botanic garden of Buitenzorg was the first victim of the new measures. It was nearly wiped out. In August, 1826, the posts of director and draftsman were abolished and but one European gardener was left. By a decree of the following year the special appropriation for the garden was discontinued, and it was decided that thereafter the "Botanic Garden of the State" should be kept up by a part of the sum allowed to the governors-general for the maintenance of their Park of Buitenzorg.

Happily there are providential interventions, thanks to which, struggling institutions resist the most murderous attacks. Such an intervention occurs when there arises a firm and persevering man who is able to demonstrate for yet another time, that will triumphs over the most vigorous decrees due to the necessities of the moment, and destined to disappear with the circumstances which brought them forth. Such a man arose and the intervention was effected. General Count van den Bosch, successor to the Viscount Bus de Gisignies, who landed at Batavia in January, 1830, brought with him from Holland an assistant gardener, a young man who had occupied an inferior position in a country house near The Hague. Toward the end of the year the only chief gardener remaining at the garden fell sick, set out for Europe, and died on the voyage. The assistant gardener of the governor general was selected to replace him. His name was J. E. Teysmann. Half a century later this simple gardener, who was given no other instruction than that of the primary schools, received a testimonial as brilliant as it was rare of the esteem he had won in the scientific world.

Besides diplomas of honor, medals struck with his effigy, felicitations from all parts of the world, there was given him an album, in which more than a hundred botanists, together with Darwin and De Candolle, offered him their greetings, and this album had inscribed upon it, on a plate of gold, the following:

"*Celeberrimo indefessoque, J.-E. Teysmann cum dimidium per sæculum Archipelagi indici thesaurum botanicum exploravit, mirantes collegæ.*"

To have attained this eminence a man must have possessed extraordinary qualities, and Teysmann certainly had them. A man of strong character in every respect, he to the end of his life united with great energy and an active intelligence the ardent desire to seize any occasion for self-instruction, for extending his knowledge of his specialty, and particularly for enlarging his views.

From 1830 to 1837, nothing is heard of either the Garden of Buitenzorg or of the chief gardener. The botanic garden existed during that

period only in name, and the chief officer considered that the first ten years he passed in Java was only a term of apprenticeship. Still it was during that period, in 1837, that the colonial government decided on a measure which was finally to bring about most fortunate consequences.

The executive member of a so-called natural history commission, to whom was assigned the scientific direction of Buitenzorg, was then Diard, of French nationality, and it was he who warmly urged upon the governor the appointment of Mr. Hasskarl, who had recently landed at Batavia and who wished a position. Diard succeeded in obtaining a provisional appointment for Mr. Hasskarl, first as gardener, then as botanist, and in the latter capacity he was charged with the systematic arrangement of the plants of the garden. This idea of Diard, carefully carried out, by Mr. Hasskarl, contributes much more to the scientific value of the garden than does the great number of species cultivated. Extensive arborescent groups, composed of the largest plants, were thus arranged in the natural order, and the botanist during the five years that he was attached to the garden was able to determine a large number of species and to compose the second catalogue of the garden, published in 1844, embracing over 3,000 plants, among which were many entirely new.

Diard and Mr. Hasskarl went to Europe on leave, and Teysmann again remained alone and in very difficult circumstances, for after the departure of Diard the control of the botanic garden passed to a military man, the steward of the governor general's palace. This extraordinary arrangement continued, and for about 30 years soldiers controlled the *Hortus Bogoriensis.* Under such conditions a new period of decline, if not of complete eclipse, of the garden would have been inevitable had it not been for the presence of the energetic Teysmann. The more difficulties he encountered the more he displayed his rare qualities in the interests of the institution to which he felt himself attached for life. Travelling much throughout the whole archipelago, he continually sent plants and seeds to the Buitenzorg. Upon his return he was constantly in the breach, fighting for the interests of his garden, not even recoiling from conflicts with his military chief, conflicts that it must be confessed were frequent. The result of this line of conduct was that in 1864, with the aid of Binnendijk, who came to Java in 1850, Teysmann issued the third catalogue of the garden, in which the number of species under permanent culture exceeded 8,000.

Finally, in 1868, the long periods of vicissitudes came to a close. The garden again became a scientific institution of the state, with a special director and appropriation, and entirely independent of the stewards of the palace, with whom it was to have, hereafter, only neighborly relations. This return to the primitive organization was due to the influence of Teysmann, who himself maintained continuous relations with the garden by numerous consignments of seeds and plants

gathered during voyages to the remotest parts of the Dutch possessions. The government appointed as director Dr. Scheffer, of the University of Utrecht, a pupil of Mignel, the author of the Flora of the Dutch East Indies. The new director began his scientific researches as soon as he was installed at Java. A few years later he obtained from the government a subsidy for the publication of a scientific collection entitled *Annals of the Botanic Garden at Buitenzorg.* During the administration of Dr. Scheffer two changes of great importance took place. The collections belonging to the service of the Mines, contained in a large museum opposite the garden, were transferred to Batavia, and the government gave the building to the botanic garden for its herbarium, its collections, and its library. A second, not less important, was the founding, in 1876, of a garden and school of agriculture. The latter has since been abandoned. The considerable extension given to the garden ought to have implied an increase in the scientific staff. Unfortunately this was not understood, and Dr. Scheffer remained alone up to the time of his death, which took place in 1880, when he was 32 years old. The period since the death of Dr. Scheffer can not be said to belong to the domain of history, and we will therefore content ourselves with casting a rapid glance over the present organization of the garden.

The interest attached to the history of any institution depends, above all, upon the importance and extent which that institution presents at the time when it is considered. The reader will judge if that is the case with the establishment of which we are writing.

The State Botanic Garden at Buitenzorg comprises three different gardens. There is first, the botanic garden proper, in the center of the city, occupying an area of 36 hectares [89 acres], wedged in between the park of the governor-general, a little river, the Tjiliwong, and the postal road. It is traversed throughout its width by a large and fine avenue called the Avenue of the Kanaries, after the native name of the trees that border it, beautiful trunks of *Canarium Cammune*, attaining a height of about 30 metres [100 feet]. Upon this avenue, which borders a great pond enlivened by a pretty island, carriages and footmen freely pass. From it roads practicable for carriages, in part open to the public, pass in all directions and form the arteries to which are attached a perfect maze of foot-paths of different sorts. Plants of one family are, as we have said, found together. They form scattered groups, or rather they occupy one or more divisions bounded by the paths. Each division has at one of the angles a list of the genera it contains. Each species is represented by two specimens, one of which carries a label bearing the scientific name, the native name if there is one, and usually stating the products of the plant. In consideration of the great number of climbing plants of tropical countries, Teysmann had the happy idea of putting them together in a special part of the garden, where they also are arranged according to their natural affinities.

There is here offered a wide field for interesting observations. Including herbaceous plants, the total number of species is about 9,000. In the middle of the garden there is a range of nurseries where young plants are cultivated, partly under shelters that protect them from the heat of the sun or from the injurious effect of beating rains. Some plants require special care, notably a certain number of ferns, arums, and orchids. These are placed in two buildings that resemble the hot-houses of Europe, with the difference, however, that at Buitenzorg they serve to keep the plants cool and not to give them a more elevated temperature. The garden has its own carpenters who construct buildings of this sort; a small detail which will give an idea of the scale upon which everything is organized. The native force is composed of about 100 individuals, among whom are 3 employés having special knowledge of botany, much more than we would expect to find among Malays. This force works under the orders of a chief gardener and a second gardener. The garden is open night and day, an arrangement which is only possible in the East where they are not yet sufficiently advanced to consider that property is robbery. At the two principal entrances there are gate-keepers but no gates.

The agricultural garden, the second division of the *Hortus Bogoriensis*, is situated about a league from the center of Buitenzorg and covers not less than 70 hectares [173 acres]. The arrangement of the place and the distribution of the plants at once shows that the aim is exclusively practical. Everything is regular, the roads and foot-paths intersecting at right angles, the divisions thus formed of almost uniform size, the plants in each division all of the same species and the same age. While in the scientific division each species is represented by but two specimens of each species, here there are a hundred, but only cultivated plants that are or may become useful to agriculture or colonial industries; the different species and varieties of coffee, of tea, of sugar-cane, of rubber and gutta-percha trees, the *Erythroxylon Coca* which furnishes cocaine, trees which produce tannin and oils, forage plants, etc. A special part of the garden is reserved for officinal plants. There is a gardener-in-chief to direct the work, and a force of 70 native workmen.

The third garden is found at a considerable distance from Buitenzorg on one of the slopes of the neighboring volcano of Gede. With an area of 30 hectares [74 acres], at an altitude of 1,500 metres [5,000 feet], it possesses a climate marvelously adapted for the cultivation of plants of the indigenous mountain flora, as well as those of Australia and Japan. About 10 natives work there under the orders of a European gardener. The three gardens which together constitute the State Botanic Garden at Buitenzorg have a total area of nearly 140 hectares [346 acres].

The museum, situated opposite the botanic garden proper, is a building 44 metres long [144 feet], specially constructed for the purpose to

which it is now applied, although it was originally used for mineralogical collections. It is composed of a hall occupying the body of the principal story, and of two wings. On the floor of the hall are upright closets along the wall, and glass cases in the center containing collections both botanical and technical. Part of the exhibits are dried and part are preserved in spirits. The herbarium occupies the gallery which runs around the entire hall, 4 metres above the floor. The dried plants are not, as in Europe, placed in portfolios, but in tin boxes in order that they may be better protected against insects and moisture, those great enemies of collections in tropical countries. As a matter of course, corrosive sublimate, naphthaline and carbon bisulphide are considered at Buitenzorg as important allies in this constant fight against insects. The number of tin boxes containing the herbarium exceeds 1,200. Each box contains, on an average, 100 specimens. One of the wings of the building is set apart for the service of the museum, a division which has for its chief the adjunct director of the garden assisted by a naturalist. The other wing, a little more than 10 metres long and nearly 11 metres wide, is wholly devoted to the library, which contains more than 5,000 volumes. This is a considerable number when it is remembered that it is a special botanical library, although books of general natural history and transactions of academies of sciences such as those of Paris, Berlin and London, are not wanting. In the matter of descriptive botany an attempt is made to obtain, besides classical and indispensable works, whatever relates to the flora of the extreme Orient. The books on general botany are supplemented by the most recent treatises and publications on morphology, anatomy, physiology, and vegetable paleontology. But the special wealth of the library of the garden at Buitenzorg is the series, generally complete, of all the reports and botanical reviews of the first rank at present published in Dutch, French, English, and Italian. The special isolation of a botanical garden situated at equally remote distances from the scientific centres of the Old and the New World makes it necessary to attend carefully to the maintenance of the library, keeping it well up to the advances of science.

There are three laboratories, and there will soon be a fourth, for in accordance with the proposition of the colonial government accepted by the mother country, the force in the garden of Buitenzorg is to be increased by two new functionaries, a botanist and a chemist, whose task it will be to furnish by patient and careful investigations scientific data as to the useful plants of tropical countries and their culture. The laboratory intended for the chemist is not yet opened. Behind the museum in a special building is the pharmacological laboratory where a pharmacal chemist temporarily attached to the garden carries on investigations upon alkaloids and other curious and useful substances which tropical plants contain. Considering the small amount of exact knowledge that we have concerning these substances this happy inno-

vation can not but produce results of great practical utility as well as of great scientific interest.

Two botanical laboratories are placed in the main botanic gardens, behind the range of nurseries. One of these, a large hall 6 metres wide and 20 long, is reserved for foreign scientists who come to pass some time at the *Hortus Bogoriensis* to make investigations and to study the tropical flora in its home. This laboratory is lighted by five windows at each of which there is a work table. Closets placed against the opposite wall contain the necessary utensils, optical and other apparatus, flasks, vases, etc., and the so-called micro-chemical reagents. Besides, there is a small collection of working books so that investigators need not have to depend upon the main library. It is also proposed to facilitate the researches of visitors, by placing in the hall a herbarium consisting entirely of specimens of plants cultivated in the garden, so that in cases of doubt the rapid identification of any such plant may be made without having recourse to the herbarium of the museum. This special laboratory herbarium is at present only begun. The arrangement of the hall is simple, offering at once the advantages of good light and plenty of room. This last point is an essential thing in hot countries, where open space is necessary, especially in a laboratory for research. Even at Buitenzorg, where the evenings, nights, and mornings are fresh, the mean temperature in the middle of the day is from 28° to 29° C. [82° to 84° F.]. There are even days during the dry season when for 2 or 3 hours in the latter part of the day the mercury rises to 31° C. [88°F.].

The second botanical laboratory, about 100 paces distant, backed up against the office of the garden and communicating with it, is reserved for the director and the new functionary, the botanist who is expected from Europe.

The fourth laboratory, that of agricultural chemistry will shortly be established in the garden of agriculture. In the near vicinity of the botanical laboratory are the offices and a small photographic and lithographic workshop for the draftsman photographer. The offices, formerly badly arranged in two small rooms of the museum, have just been transferred to a special building, given up for that use by the Government, a new proof of the solicitude the government of the Dutch East Indies and of the mother country always feels for the Garden of Buitenzorg.

II.

What are the principles of the organization we have just described, and how does it work? What are the advantages peculiar to large botanical gardens in the tropics, and why is there reason to expect them to exercise a great influence over the future development of botany? Before answering these questions an understanding must be reached on an essential point; that is to say, the different way in which

pure and applied science is studied in Europe on the one hand and in a tropical country on the other. When among European peoples science took the marvellous flight which characterizes our century, a *differentiation* soon commenced. Purely scientific studies and investigations remained as formerly more or less directly attached to the universities and faculties, in a word, to superior instruction, properly so called. But at the same time the remarkable useful applications which accompanied the progress of science necessitated the creation of special institutions, polytechnic schools, technical laboratories, experimental gardens, agricultural stations, etc. Both of these sister branches, pure and applied science, equally demanded indefatigable workers, trained in method and gifted in intelligence. While having a totally different object, they remain in relation and continual contact. Still the specialization exists and it may be easily foreseen that it will increase. It is the same or will be the same in colonies where the climatic conditions permit the European to fix his permanent habitation, but it is not the case for European colonies in tropical countries. There the colonists do not come with the intention of remaining permanently. On the contrary, from the time of their arrival in the distant country, however beautiful and fertile it may be, they are firmly resolved to return to their native land. The majority of them, having acquired social position or the wished-for fortune, hasten to return home, almost certain to find that the recollections of childhood and youth are deceptive, and that the climate and social organization in Europe are far from reaching the ideal which they had formed during their sojourn at the antipodes.

Recently the question has been much discussed whether Europeans can found colonies (in the strict sense of the word) in tropical countries, reside there for several successive generations, and raise there a pure blooded race. The celebrated Professor Virchow is one of those who deny with great authority and energy the possibility of a true acclimation of a European race in a tropical country. If a naturalist who has dwelt in the beautiful island of Java for some years, and who is a fervent admirer of it, may be allowed to have an opinion on this mooted question, I must avow that everything goes to show that M. Virchow is right. But whatever opinion may be held concerning the theoretical possibility of this acclimation, the plain fact is this, that in the Dutch East Indies, and so far as I know in other tropical countries also that have been under European control for some centuries, the pure race has not succeeded in becoming acclimated.

This point once understood, it will be clearly seen why (with rare exceptions) universities, faculties of sciences, and similar institutions have hitherto been wanting in tropical colonies. Families send their sons to Europe to study and take their degrees. The teaching body of the university, with its laboratories, its libraries, its cabinets, and its collections, does not there exist; and yet it is especially in a tropical colony that material interests, so important there, ought to cause great value to be

placed upon applied science. This is a contradiction at once apparent, and which becomes still more obvious if we pass from the general case to the special one of botany, which is of the first importance, because of the great influence it has upon tropical agriculture. The time has passed, and we should be glad of it, when the high price of colonial products, the want of co-operation, excessively cheap labor, and sometimes also oppression of the native population, made all special knowledge superfluous to anyone who chose to take the chance of making his fortune in agriculture. We are already far from the period when the grossest empiricism was usually sufficient, permitting the acquirement of wealth by those destitute of education and often even of intelligence.

To insure solid results, tropical agriculture—no less than that of temperate countries—demands judgment and special knowledge, and the need is felt of establishing it also on a firm scientific basis. It has it is true been said, adopting a practical view of the very narrowest kind, that the contradiction we have just pointed out, did not necessarily exist, since it was only necessary to take for a scientific basis the results of the researches of European scientists, only that the application will be somewhat different in the tropics. This is a very grave error, especially since it relates to the phenomena of life. It is vain for us to compare as to their effects upon vegetation, the dry season with winter, and the rainy season with spring and summer. The forms and functions in which vegetable life manifests itself in an equatorial country are quite different from those in the temperate zone. The essential laws which rule life are the same, but the manifestations of it are quite different. It is therefore for the immediate interest of tropical colonies to possess scientific establishments for the study of life in its forms and in its functions. As institutions of this kind depending upon universities or faculties do not exist, it is evident that botanic gardens established by the state are indispensable. These gardens serve a double purpose, scientific and practical, but it should not be forgotten that it is in science only that they must have their root. The scientific institution forms the trunk on which the branches are grafted. If the trunk is hampered ever so little in its growth and loses its vigor, the branches will certainly suffer, and in the end may perish. Thus everything which lowers the scientific tone of a tropical botanic garden is contrary not only to the advancement of science, but also to the direct interests of the colony.

It is neccessary to insist upon this truth because there is always am ng agriculturists a tendency to confound a botanic garden with an agricultural station or with an experimental garden. This error is excusable in persons who not understanding the *festina lente* of science, are continually wishing immediate answers to questions of vegetable pathology and physiology which they ask in the interests of the special culture in which they are engaged. This want of patience and comprehension of the *modus operandi* in scientific investigations is the principal reason why agricultural stations founded by agriculturists them-

selves are liable not to give the results expected and certainly merited by the laudable efforts of those who established them. A state establishment pursues its regular development protected against these impatient demands. It gradually extends its sphere of action for the interests of all, but without allowing the variable and often exaggerated exigencies of the moment to disturb it. The first duty of the functionaries placed by the colonial government at the head of the botanic gardens is to combat the lack of stability and continuity, the scourge of every colony. It is not only the right but the duty of governments to demand that the persons to whom they have entrusted these posts shall not have variable and narrow views, excusable in others, but never in a naturalist. The latter has had the benefit of an enlightened scientific education, and there is expected of him a certain breadth of view which should be the result of his own researches. These general principles admitted, let us see how they are carried out in the particular case under consideration. The government of the Dutch East Indies authorizes the director of the garden at Buitenzorg to distribute gratuitously seeds and plants of useful vegetables. In 1888 there was sent to all parts of the archipelago 1,400 packages of seeds, cuttings, and young seedlings of useful plants. It is by means of the garden of agriculture that it is possible to gratify so many demands. But this garden is part of a scientific organization and would not work well if alone. The following examples will show this: When the remarkable anæsthetic qualities of cocaine were discovered, it was only necessary to go to the two specimens of *Erythroxylon coca* of the group of Erythroxylaceæ in the botanic garden proper. Enough seed could be gathered to make a little plantation in the garden of agriculture. When, a year after, a scientist urged upon the colonial secretary at The Hague that the introduction of *Erythroxylon coca* should be attempted at Java, the Buitenzorg authorities were able to answer that thousands of seed gathered in the garden of agriculture had just been distributed. The tree for a long time known as the producer of the best quality of gutta-percha, the *Palaquium* (*Isonandra*) *gutta* is believed to grow nowhere in a wild state; at all events it is almost impossible to obtain seeds. In the division of Sapotaceæ in the garden of Buitenzorg, are two specimens from 30 to 40 years old which produce every 2 years a great number of seeds. From them has come the young plantation in the garden of agriculture as well as a great number of specimens in a large separate plantation of gutta-percha trees commenced by the government some years ago under the auspices of the garden of Buitenzorg. The camphor tree of Sumatra, a plant of great value, is very difficult to obtain, first because its seed are very few, then because they lose very rapidly their germinating power, even during a short voyage. By taking special pains Teysmann succeeded in introducing the tree at Buitenzorg. In 1885 the specimens at the botanic garden began to bear fruit, and now the garden of agriculture possesses a plantation of young

Sumatra camphor trees, while there is besides a considerable number of plants to be distributed during the next rainy season. Why was it that a short time after their qualities became known, the garden of agriculture possessed new cacao trees from Nicaragua, rubber trees, forage plants, and new varieties of coffee plants from Brazil, oleiferous plants, plants for cooking and useful trees from Gaboon, rubber climbers from Zanzibar, etc.? It was only because, having the great botanic garden to depend upon, it could offer its correspondents in exchange many a plant interesting to botany or horticulture. The researches hitherto made at Buitenzorg upon the pathology and physiology of plants of general culture have been but few in number, and besides they have been more or less against the interests of the garden, an additional proof of what has just been stated. As soon as the two new functionaries, the botanist and the chemist, especially appointed for these researches shall arrive, the scientific force of the garden at Buitenzorg will be sufficiently numerous and varied to answer every need. On the one hand it will be impossible to lower the general scientific tone; on the other, patient and careful researches will give to agriculture a solid basis by which it will not fail to profit. The trunk will preserve the necessary sap for the food of the branches on which practical aims will have been grafted. That which will be accomplished in a little time for agriculture, took place a year ago for pharmacology and toxicology. Although the skillful pharmacal chemist who is the chief of this new division has only commenced his researches, the results obtained up to the present time furnish conclusive proofs both of the utility of the measure undertaken by the colonial government and of the necessity of attaching this laboratory to a great botanic garden.

At the time of the founding of the *Hortus Bogoriensis* the great utility which it would finally be to the colony was perceived, but this was not the chief motive for its creation. When the government of Holland sent Reinwardt to the Dutch East Indies it was, as expressly stated by the sovereign, "for the purpose of obtaining as thorough a knowledge of our colonies as our neighbors possess of theirs." It was the intention of the king to contribute, by encouraging scientific exploration in the colonies, toward "rendering manifest the happy rehabilitation of the Dutch name. The result of generous and elevated ideas, it is the duty of the garden of Buitenzorg never to forget its origin. To continue an emulation with the neighboring colonies, to aid in making known every possible aspect of the exuberant vegetation of the tropics, to contribute to the advancement of science independent of any direct utility, is really to render service to the colony, and in a way which, in the long run, is quite as efficacious as that which looks only towards direct practical interest. The more civilization advances the more it is demanded of nations which possess great kingdoms in far-away countries blessed by heaven that they should not forget that royalty has its responsibilities and that it can not be allowed to withdraw

itself from the noble task of adding to our knowledge of nature, independent of any direct advantage, either present or future.

A considerable part of this duty falls upon botanic gardens, especially when they possess special advantages like that of Buitenzorg. We said at the beginning that the adverse criticisms made against botanic gardens would not apply to those of the tropics because the latter are placed under quite special conditions. In fact, the short descriptions which we have just given will suffice to make it understood that judging by Buitenzorg there is no attempt at making an immense collection of plants in abnormal conditions. It is true that in many divisions of the garden growth has caused the trees to approach each other too closely, but the specimens that suffer in this way do not at all remind us of those slender, spindling specimens of hothouse growth attacked by the learned critic. As to the conditions offered to plants it is evident that there is a great difference between hothouses and a garden. Not that the *Hortus Bogoriensis* offers to all its plants a perfectly natural situation, but from that to abnormal conditions is a long way. It is sufficient to recall that aside from young plants and the very few species that are cultivated under shelter, all the plants grow in the open ground. In the second place it is evident that the great number of vegetables scattered over such a vast space implies the impossibility of giving a factitious life to any one specimen by over-care. In general it may be said that every plant introduced into Buitenzorg with which the climate does not agree ends by dying,—generally in a very short time. The plants that continue to grow in a tropical garden may develop more or less well, but it is very rare that we have to admit that they are abnormally developed, so the taxonomist and the morphologist can study the plants of the garden without fearing to fall every moment upon characters that are unnatural or disfigured by culture. In the rare cases of doubt the herbarium is there to serve as a check and to allow a comparison with neighboring species not cultivated in the garden. In view of the great number of tropical ligneous-plants, the study of living specimens has for the systematist some real advantages over the study of herbarium specimens. The latter are necessarily but small fragments, carrying, it is true, flowers and fruits, but very rarely showing polymorphism, so frequent in vegetation. The physiologist and the anatomist may make their researches on development, the play of functions, and the minute structure of the plants of the garden without the fear of being led into error by degenerations and reductions due to a life of starvation and ill-health consequent on unnatural conditions. For this sort of researches the absence of a dry season is of special advantage to the garden of Buitenzorg. The periodicity shown in the successive stages of the evolutionary cycle of a plant is there almost always due to internal causes and quite rarely to the direct influence of external causes. This is, for the phyto-physiologist, an advantage which he does not find in the temperate zone and rarely in the tropics.

We see in what favorable circumstances the botanists attached to the *Hortus Bogoriensis* and residing at Buitenzorg study in every aspect the flora of the Dutch East Indies, and in general the manifestations of vegetable life in a tropical country, but they would have very badly understood their task and shown a regrettable narrowness of ideas if they had wished to preserve for themselves the discoveries and the investigations in this vast and fertile field of study. Far from this, it is their duty to constantly urge their brethren beyond the sea to come and profit by the opportunity of studying a great number of questions it would be impossible to attack in Europe. A generous scientific hospitality offered to all, profitable to science and worthy of the colony that has the advantage of being able to offer it, is the only line of conduct proper to follow. For the purpose of carrying out a plan like this the government of the Dutch East Indies founded at Buitenzorg four years ago the laboratory of research which is at the disposal of foreign naturalists.

At length we have reached the important question, what reason is there to think that botanic gardens in the tropics have entered upon a new phase in which they will exercise great influence upon the study of botany? The answer is as simple as it is short: because they have become botanical stations similar to the zoölogical stations on the coasts of Europe. Any one interested in natural sciences must know that zoölogy owes a great part of its recent rapid advancement to these littoral stations. However unlikely it may appear we may predict that botanical gardens of the tropics will have in future a still greater importance in the advancement of botany. To effect this they must be large and favorably situated like that of Buitenzorg and of Paradeniya, where they have just followed the example of establishing a laboratory for visitors.

In order that this prognostication may be realized two things are necessary. First, that botanists shall follow the example given by their colleagues, the zoölogists, in becoming less reclusive; then, that they should have more accurate ideas as to the "perils" to which one is exposed in a sea voyage, and especially as to the "dangers" which meet a visitor to a tropical climate. Rocks, hurricanes, and shipwrecks on one side, fatal diseases, wild beasts, serpents, and venomous creatures of all kinds on the other, are so many phantoms which haunt timid imaginations and prejudiced minds. Whoever is acquainted with the great steamers that make the voyage to the Indian Ocean knows that the perils and inconveniences which it was imagined must be endured on board these well-equipped and comfortably fitted vessels have very little basis of fact. Three or four weeks of *dolce far niente* passed on board a great mail steamer, during which one enjoys the excellent fresh sea air, are advantageous to the health. It is true that it is sometimes a little tiresome, that there is at times a little monotony in the diversions offered by the flying fish and porpoises. But on the other

hand what excellent memories are preserved of the long days on board! The apprehension which has the least foundation in fact, that of the dangers which one incurs by passing a few months in a tropical country, is yet more difficult to dissipate. The false opinions on this subject, which are found in every country, have a singular tenacity of life. If one only goes to a healthy and civilized locality a sojourn of a few months in a tropical country presents no danger whatever. On the contrary, for many constitutions, autumn and winter in Europe are far from being as healthy as the climate of the tropics. Certainly it is possible that the latter may be injurious, but such an effect is only felt after a prolonged exposure.

However unfounded such fears may be they can not be overcome if there remains any doubt but that a sojourn of some months in a botanic garden in the extreme Orient will be of great use to a naturalist. The remark has sometimes been made that a botanic garden of this kind, however great and rich it may be, can not give by itself any adequate idea of the vegetation of a virgin forest which has such an irresistible attraction for the observer of living nature. This is true, but it should not be forgotten that in Java, as in many other tropical countries, primitive nature and civilization jostle each other. At Buitenzorg, the vice-regal residence, an excursion of 1, 2, or 3 days transports the botanists to a perfectly virgin forest, so near is it. Besides, a branch establishment of the garden is situated upon the mountain called Tjibodas, which touches the very edge of the forest from which it was recovered. There naturalists visiting the botanical station of Buitenzorg go to pass some time for the purpose of making observations and gathering at their ease plants from their native wilds. In order that these wilds may be safe from any injury by the natives, and that their primitive character may be preserved, the government has taken care to put an area of some 250 hectares [nearly 1 square mile] under the immediate control of the botanic garden.

There are certain obstacles to be met when one would make a voyage to the East Indies, such for example as preparing for an absence of considerable duration, a leave to be obtained or a public mission to be asked for, or objections of members of the family unaccustomed to travelling. Therefore it may be asked whether such a voyage secures to the investigator not only the certainty of establishing new facts which may be arranged on well-known lines, but also whether there is much chance of discovering new paths which when explored will lead science to new results. This question should receive a stronger affirmative answer than might be supposed by many naturalists who have never quitted Europe. In order to appreciate how fierce is the struggle for life in the tropics, and to comprehend how nature has exhausted herself in furnishing to the combatants a diversity of offensive and defensive arms elsewhere unknown, it is necessary to observe it upon the spot. One must see for one's self—to cite but one example—trees of lofty

stature covered to the top with a bosky vegetation of parasites and epiphytes, to be able to conceive how, in their own special way these wrestlers have multitudes of special adaptations of which we as yet but dimly perceive the origin and the functions. Only after having experienced the surprise caused by the sight of the luxuriant vegetation of the tropics, can the physiologist at last obtain a true idea of the wonders reserved for him in the study of vital phenomena manifesting themselves with such remarkable force. Finally, it should be borne in mind that the present climatological conditions of equatorial countries are very much like those which formerly extended over the entire surface of our globe. It is therefore indispensable that we should study tropical plants if we wish to solve the series of riddles relating to the origin and affiliation of the plant groups of our period. To the botanists who study this marvellous flora in its native situation is reserved the honor of filling out the great gaps in our present knowledge and of making discoveries whose importance and signification we can now but partially guess.

What we have just said is neither premature nor out of place. First, the results already obtained sustain it. Besides, naturalists have recently given a proof of the interest they have in extending their researches to equatorial countries. During the 4 years that the laboratory for research has been established at Buitenzorg it has been visited by fourteen naturalists, and all but one came from beyond the sea and from different countries. It is to be regretted that we have to add that no French botanist has, up to the present time, come to occupy a work table in the laboratory of the *Hortus Bogoriensis.* Without doubt the number of visitors will go on increasing, and at length they will come from all nations. He who has the honor of now directing the scientific establishment described in this article is the first to desire it. Indeed, it is with the intention of encouraging and stimulating this movement that it has been written.

TEMPERATURE AND LIFE.*

By Henry de Varigny.

Everything that lives generates heat. Wherever there is life there is simultaneously a production and liberation of heat. On the other hand, there exist for all organic life, animal or vegetable, limits of temperature, above or below which life can not be sustained and between which points only can full development be attained. Temperature is therefore an important element in all life, and it is interesting to consider in detail the facts upon which this conclusion rests. We must weigh successively two questions: namely, the generation of heat by organic life, and the influence exerted upon that life by the theometric variations to which it may be subjected—variations which necessarily react upon internal temperature, with different degrees of intensity, however, as we shall see.

I.

Every animal is a source of heat. This is distinctly appreciable in man, birds, and superior organisms in general, and the characteristic temperature of the various members of the animal kingdom presents interesting, although inconsiderable, differences. Birds generate more heat than any other organism, in so far as their temperature is shown to reach a higher point. According to various observers, it varies from 39° to 44° C., while that of man and mammals ranges between 37° and 39° C. (98° and 102° F.)

Man, mammals, and birds are called creatures of equable temperature, homeothermic—that is warm-blooded—animals. By this is meant that their individual temperature is high, that it varies but slightly, and that it does not follow the changes in the surrounding atmosphere. Another class of organisms, representatives of which are never found among birds or mammals, are called heterothermic—cold-blooded—animals; creatures of variable temperature, since, in their normal physiological state, their individual temperature follows closely the changes in the atmosphere about then. The temperature of reptiles, batrachians, fishes, mollusks, crustacea, insects, etc., is almost identical

* Translated from the *Revue des Deux Mondes,* May 1, 1889; vol. XCIII, pp, 176–201.

to that of the water or air surrounding them. All animals except mammals and birds are cold-blooded animals. It is to be noted however that certain mammals, usually rodents, are in turn warm-blooded and cold-blooded. These are hibernating animals, which after the fall of the external temperature below a certain point, become torpid and fall asleep, their own temperature being hardly higher than that of the air about them. Of these we shall speak again later.

Without the aid of certain instruments heterothermic animals would appear to generate no heat whatever; for to our senses, their temperature is the same as that of their surroundings. In the case of reptiles, however, the temperature exceeds that in which they are placed, (the difference being estimated when the external temperature was at a point between 5° and 15° C.) as much as 6, 7, and 8 degrees, though it more frequently varies from 1 to 4 degrees. In the case of batrachians it is less, scarcely exceeding 2 or 3 degrees under the same conditions. The difference is still less appreciable in fishes, and it reaches its lowest point in invertebrates, in which the temperature only occasionally shows an excess of one-fourth or one-half of a degree centigrade over the temperature of their surroundings. Insects, particularly those which live in communities, generate at times considerable heat. Thus Réaumur observed the temperature in a bee-hive raised to 12°.5 C. when the external air was at —3°.7 C. In short, heterothermic animals generate little heat, but its production is constant.

What is the cause of this calorification? This is the question into which we are now to inquire. The strangest ideas have been entertained in regard to it. One investigator makes a mysterious principle of animal heat, the seat of which is the heart, where it develops so high a degree of temperature that touching this organ by chance results in a painful burn. The author of this theory has evidently never practised vivisection, for, as a matter of fact, the heart is one of the coldest of the organs, in mammals its temperature rarely exceeding 39° or 40° C. According to J. Hunter, the celebrated surgeon and anatomist, this mysterious principle of animal heat resides in the stomach. Barthez and his followers attribute it to an entirely different cause; more reasonable (in that it excludes the supernatural and mysterious), but no less erroneous. Their belief is that it is due to the commingling of the several liquid and solid portions of the organism. It was Lavoisier who laid the foundations of the true theory of calorification. Having made an exact calculation of the nature and properties constituting the atmosphere in its normal condition, he demonstrated in an irrefutable manner that air, expelled by a living creature, contains carbonic acid in larger quantities than the air which he inhales. A combination has therefore taken place between the oxygen in the air and the carbon contained in the organism. "Pure air, in passing through the lungs, effects a combination analogous to that which takes place in the combustion of charcoal. Now, in the combustion of charcoal there is a

liberation of matter from the fire, consequently there must be likewise a liberation of matter from the combustion in the lungs." That is to say, since the lungs evolve carbonic acid, a generation of heat must follow, for the reason that heat is under all circumstances an accompaniment of combustion. A living organism produces heat because it burns. The study of a century goes to show the accuracy of this conclusion.

According to Lavoisier the lungs appear to be the seat of respiratory combustion and calorification. On this point however he is guarded in what he says, and this reserve is justifiable, as, in point of fact, their rôle is quite a different one from that which he supposes. Lagrange, a short time after Lavoisier, combatted this supposition, stating that if the lungs were actually the seat of these combustions, the heat generated would be of such intensity that this organ would suffer injury sufficiently serious to be incompatible with life. This, however, is an exaggeration. The production of heat has been estimated, and, even supposing the lungs to be the exclusive seat of this function, the temperature of this organ would not be intense enough to be injurious. The most exact researches have shown what is the work assigned to the lungs in the process of calorification. This organ which, owing to its innumerable cells, representing a surface of 150 or 200 square metres (this, although astonishing, is indisputable), only serves to bring in contact the blood and the air. The net-work of capillaries, separated from the air by a fine layer of cells, represents a surface equal to about three-fourths of that of the entire lungs, and forms a sanguineous coating of 100 or 150 square metres. This has little depth, it is true, only containing 2 litres of blood. This however signifies little, for in order to secure absorption, it is extent of surface rather than depth which is required; the latter being of slight consequence. Moreover if there are at a given moment 2 litres of blood in the lungs it is estimated by a simple calculation that the total quantity of blood passing through the lungs in the course of 24 hours is about 20,000 litres. In fact, the anatomy of the lungs is admirably arranged to give them this absorbing capacity, and experience shows that their rôle is exactly that for which their organization is best adapted. The blood which permeates the lungs absorbs the oxygen in the inhaled air, by reason of a chemical affinity between the hæmoglobine of these red globules and that gas, and carries it throughout the body. It is in the recesses of the tissues over all parts of the organism that this oxygen, separating itself from the hæmoglobine, unites with the carbon of the tissues, and *ignites* in order to give birth to heat and carbonic acid; necessary results of all combustion. The acid which is taken up by the blood is finally expelled through the lungs.

Calorification is thus the result of combustions which take place at all points of the animal economy. It is in complete dependence upon the relations of two other functions—respiration (that is to say, the supply of oxygen for burning) and alimentation (the supply of carbon or of

combustibles). We shall have occasion to refer to this point later on. Calorification is produced not only in the lungs, as Lavoisier believed up to a certain point, but in all the tissues of the organism, the proof of it being that the tissues respire in a condition of life. Exception is made, however, of cutaneous growths, such as hair and nails, these being lifeless portions of the organism. If the tissues respire it is because there is a combination of oxygen and carbon, hence combustion, hence heat. The demonstration of the respiration of the tissues is easily furnished by experiment. Let an animal be killed and fragments of muscle, liver, brain, bone, etc., detached. Let these be placed in a test tube containing oxygen, and inverted on mercury. At the end of a space of time, which varies in length, and in proportion differing according to the tissues, there will be found in the test tube carbonic acid which has replaced a part of the oxygen, and which establishes in an indisputable manner the respiration which has taken place.

In short, animal heat results from combustion of the carbon in the tissues with the oxygen of the air, this element being introduced into the blood by the action of the lungs, and carried by this liquid throughout every portion of the body. Combustion takes place in all the tissues (and in the blood itself, although but slightly) in varying degrees, being greater in extent in the muscles, brain, and glands, and less so in the bones and other anatomical portions of the structure.

Is calorification, then, the result of combustion and oxydation only? It was for a long time so believed, but in reality other influences enter into this function. The organism is, in fact, the theater of chemical phenomena, infinite in variety. The materials derived from the food are assimilated by various chemical processes, and the action of elimination is accomplished by phenomena of no less variety. All the combinations, decompositions, reductions, etc., which the different materials undergo, give rise invariably to the generation or absorption of heat. In plain language, all chemical action produces heat or cold, according to circumstances, and this production is in conformity with chemical laws which are now fully understood.

Among numerous chemical phenomena of this sort in the organism—phenomena which have been thoroughly studied by M. Berthelot—special reference may be made to hydrations, decompositions, combinations, and fermentations. All these phenomena take place in the bodies of living creatures, and all play their part in the process of calorification. Calorification is then the result of multiplied chemical actions which occur at all points of the organism, actions of which some generate, while others absorb heat, but among which those of the former evidently predominate. Among the heat-giving phenomena oxydations are the most important, but it is well to remember that this is not their only attribute, as Lavoisier believed.

The simple fact that respiration is not carried on with the same activity in all the tissues indicates *a priori* that there must be an appre-

iable difference in their temperature. This is in spite of the fact that n living organisms the equal distribution of temperature is favored by he contact of heated portions with those which are less so, either directly or indirectly, by the circulation of the blood. In spite of this tendency o establish an equality of temperature, it is easy to distinguish those of the highest temperature. They are naturally those of most activity, rom a chemical standpoint, and whose respirations are most frequent. The liver, brain, glands, heart, and muscles belong to this class. The heat generated by these organs is in proportion to the chemical activity and to the amount of work which they themselves perform. Every organ is, in fact, warmer when in a state of activity than when in a state of repose.

Calorification is thus the result of chemical phenomena which take place in the recesses of the tissues. These phenomena, which are numerous and active in animals of the higher class (homeotherms), are much less so in cold-blooded animals; but this point is not important, the difference being in degree, not in kind.

Here a question arises: Why does man have an equable temperature at the poles, where the temperature is 30 degrees below zero, and in Sahara, where there are 40 degrees of heat? Why are not man and warm-blooded animals influenced to a greater degree by the temperature n which they live, and how are they enabled to contend with these extremes of temperature? In several ways, from a physiological standpoint, for at this time we are not to consider the means devised by man for his protection. To enable him to endure extreme heat, he is supplied with a sudatory apparatus which acts as soon as the internal temperature begins to rise. The action of external heat brings the sudatory glands into activity, and evaporation of the perspiration produces refrigeration to a marked degree. Note, by the way, that this evaporation is only possible in an atmosphere relatively dry, and is less in proportion to the humidity of the atmosphere surrounding the body. On this account one suffers more from heat when the air is full of moisture than when it is dry. Humidity impedes and retards evaporation, and n consequence also refrigeration.

Certain animals are endowed with this sudatory apparatus for the same purpose as man, but many of them are entirely without it. Among the latter class are birds, dogs, rabbits, etc. In what way are these protected against heat? As far as we know, no researches have been made on this point in regard to birds; but concerning dogs, M. Ch. Richet has reached very interesting conclusions. In this animal refrigeration is effected by means of the respiratory organs, for it is by this means only that they can bring about a copious evaporation. The dog perspires through his lungs, as is the case with all creatures which have this organ, even man himself, but with the dog this is the only means of effecting perspiration, and it is therefore employed to a far greater extent. When a dog is heated he thrusts out his tongue in

order to facilitate the passage of air through the mouth. He breathes quickly, sometimes with great rapidity, in order to induce a more abundant exhalation of moisture. It is much to be desired that a study of refrigerating mechanisms be pursued in behalf of those beings which have no perspiring capabilities, as such a study would be fruitful in interesting results.

When the internal temperature of man is at a low point, sufficient refrigeration is effected simply by the flow of blood, which is always towards the surface. Influenced by external heat the cutaneous tubes expand, by this means they are able to contain a larger quantity of blood, and radiation from the skin is thus increased, resulting in a cooling tendency, which spreads through the entire system by reason of the circulation of the blood, which is also accelerated, and thus facilitates refrigeration.

From a physiological point of view the organism is less fully equipped for protection against extreme cold. Cold however is less dangerous to organic life than heat, and for this reason nature has prepared it more perfectly to meet the latter. A sensation of cold stimulates animal life to activity, and by this very result produces warmth. Moreover animals of cold climates have in the winter a heavier growth of fur, which serves as a protection. In addition to this resource, we shall point out the fact that cold contracts the tubes of the skin which diminishes refrigeration; respiration is accelerated and with it organic combustion. The need of food is greater and it is eaten in larger quantities, all of which introduces into the system a greater quantity of combustible material. Observe for a moment the immense importance of the nervous system in its effect upon bodily temperature. This fact has been clearly demonstrated by many experiments in physiology, as well as by clinical observations.

To epitomize, the heat of animals is generated by chemical phenomena which takes place within the organism. With some species these phenomena are very active and the temperature proportionately high. In addition they are furnished with a regulating apparatus so arranged that within certain broad limits oscillations in the external temperature modify only to a slight degree, or insensibly, their internal temperature. These are the homeothermic species. With the others (the heterotherms) in which chemical phenomena are feeble and inactive, there is a temperature correspondingly low. These, moreover, have no protection against the influence of the outside temperature, following closely its variations. Their own temperature is, in fact, the result of their environment, more than of the chemical phenomena within. This difference between warm and cold blooded animals is considerable, for in the case of the former, under average normal conditions, the external temperature has no, or little, action upon the temperature of the animal.

Calorification is a general phenomena among animals from protozoan

to man. There are differences in degree, but the fact is universal. It remains for us to prove that this is not only the rule with animals, but is also true wherever there is vegetable life, constituting in fact an inherent function in all animate matter.

Plants respire, consequently they generate heat. This is an ascertained fact of which proof has been given by numerous experiments. The cholorophyllic function, which effects a decomposition of carbonic acid in the oxygen which is exhaled, and in carbon which is incorporated in the tissues, has, for a considerable time, obscured the true manner of respiration, making it appear that vegetables respire in an entirely different way from animals. The process is the same in the two classes of organisms. To assure ourselves of the fact, however, it is necessary to eliminate the chlorophyllic function by having recourse to a particular arrangement; experimenting upon plants without chlorophyll, or upon chlorophyllic plants kept in darkness—the chlorophyllic function acting only in light. In taking the above precautions, we establish the fact that respiration exists among all plants—with more activity, it is true, in young plants than in older ones, in plants which are in course of development, rather than those which have already attained their full growth. This respiration, as in animals, consists of chemical phenomena. It is caused by an absorption of oxygen, and a combination of that gas with the tissues of the plant, by which heat is produced. As observation has demonstrated to us, everything that has life generates heat by reason of the chemical phenomena which accompany life. The germination of seeds, for example, does not occur without this evolution of heat. To assure ourselves of this, let a thermometer be placed in the midst of a quantity of seed in process of germination, taking care to insure the elimination of carbonic acid in proportion as it is produced—for it arrests respiration and calorification. The thermometer will be seen to rise 5°, 10°, 15°, and 20° C. The generation of heat in this case is therefore considerable. Various experiments made with seeds have substantiated the conclusion. Flowers also produce a remarkable amount of heat, the truth of which Lamarck was the first to establish. It is with flowers of certain aroïdes that experiments have been most successful, and which have furnished the most exact data. The temperature of the spathe of these plants when in full flower indicates a generation of considerable heat, presenting sometimes an excess of 5°, 10°, and 15° over the surrounding temperature. To show that this calorification is a result of respiration, let a flower be covered with oil in order to exclude the oxygen in the air, or let it be placed in an inert gas from which all oxygen has been exhausted (nitrogen for instance), and its temperature will be reduced to almost nothing; combustion is retarded if not entirely suppressed. Very delicate experiments have established beyond a doubt that a close correlation exists between the supply of oxygen and the amount of heat produced, the latter being proportionate in intensity to the quantity of oxygen absorbed.

One has a right therefore to assume that all flowers evolve a certain amount of heat, variable, it is true, for one flower differs from another but always clearly appreciable. A similar evolution is observed in the active organs of plants when they are excited to movement. It has been established in the case of germs by the means of thermo-electric needles. It is much more sensible than in the case of adult plants, in which life is less active and intense.

We see in the vegetable, as in the animal kingdom, that heat is generated, and that it is due, for the most part, to oxidations within themselves. It is possible to establish the existence of a complete likeness between these two classes of organisms. The demonstration which substantiates itself every day of the identity and unity of the fundamental laws of life, in spite of variation in form and appearance, is not one of the least benefits which have resulted from the investigations of modern science.

At the point where calorification results from chemical phenomena accompanying nutrition and respiration a close dependence springs up between it and the process of alimentation. This dependence clearly exists. The phenomena of alimentation are in consequence of the introduction of food into the organism in such a manner that it can be assimilated, portions of it immediately, and that which remains after it has undergone chemical modifications. To the former category various salts and water belong; to the latter, organic compounds, flesh, fruits, vegetables, milk, etc. Where there is a total lack or insufficiency of alimentation the animal perishes, especially when there is no reserve supply of nutriment in the form of fat. At the same time its temperature falls. This fact has been established by Chossat, who has made an exhaustive study of inanition. Animals deprived of nutriment generate less heat. Their temperature diminishes each day, and finally, at the moment of death, sinks to 10°, 15°, or 20° below the normal medium. The temperature of pigeons, for example, falls from 40° or 42° to 20° or 18°. The same phenomenon exists in the case of man or mammals. It is the same with them as with a boiler when the furnace is not fed; the fire is extinguished and heat disappears. In the vegetable kingdom there is in all probability a similar occurrence, although no visible proof is given of it as far as we know. Experiment in this case is very difficult, but an indirect proof is furnished by the fact, well known to agriculturists and botanists, that the suppression or diminution of such and such mineral salts necessary to vegetable life will result in the deterioration and relative unfruitfulness of the plant. That which diminishes their vitality and their proportions diminishes also their nutrition, and as a natural consequence their production of heat.

There is therefore between the processes of alimentation and calorification a fixed relation, and one can readily determine among the many different kinds of foods those which contribute most towards calorifi-

cation. Chemistry shows us by exact analyses that different bodies, in oxidation, evolve varying degrees of heat. Let us imagine a given quantity of oxygen introduced into the blood to assist the oxidations which are the principal though not exclusive source of animal heat. The amount of heat which will be produced by the combustion of this volume of oxygen with the material existing in the tissues will vary according to the nature of the material. Combining with certain substances the same quantity of oxygen will generate ten times more heat than will result from certain other combinations. That which is true of oxidations is also true of other chemical phenomena incident to calorification—that is to say, hydrations, de-hydrations, decompositions, combinations, etc. The production of heat varies considerably according to the chemical nature of the substances which are influenced by these modifications. It is enough to say that certain foods are more productive of heat than others. Observation has long since shown the effects, in a cold climate, of a diet rich in fats and in sugar, and experience establishes the fact that these substances develop a greater degree of heat than albuminoids. On the other hand, we all know that inhabitants of warm climates need less food and are more abstemious than those of a cold region. The need for being heated is less pronounced in their case on account of the temperature in which they live, and in which the external cooling is little or nothing in extent.

The relations which exist between the processes of calorification and respiration are no less evident. Anything that obstructs respiration obstructs also the generation of heat. This is more pronounced in the case of creatures with whom oxidation plays a very important part in the generation of heat. The deprivation or diminution of pure air very quickly results in serious disturbance, due to the irregularity occasioned in the vital functions by an insufficient exchange between the blood and the atmosphere. Supposing that life were possible during a temporary but somewhat prolonged cessation of respiration, the temperature of the body would quickly diminish. The higher class of beings may not furnish proof of this fact, being so exceedingly sensitive to the deprivation of pure air, but by the lower organisms it is clearly proven. We have seen it in depriving of its share of oxygen a flower of *arum* or of *colocasia* by dipping it either in oil or in azote, when the phenomenon of thermogenesis is considerably diminished.

In fine, the relations of calorification to the activity of the organism are quite as clear as those of which we have just spoken. These are manifest among vegetables as among animals. With the first the generation of heat is greatest during movement, or in reference to the more active portions, from the point of view of vitality and growth, and during the organization of the tissues; in germs, in which the chemical changes are rapid, numerous, intense, and in flowers during the operation of fecundation.

With animals all activity is accompanied by an elevation of the tem-

perature, local or general, according to the intensity and duration of the activity. It is thus that a muscle in the act of contracting evolves more heat than when in a state of repose, and this production is such that it easily increases the temperature of the body 2°, 3°, 5°. In the same manner, a mental or intellectual effort results in a production of considerable heat. The glands in an active state generate large quantities of heat, as is seen by the temperature of their secretions and of the venous blood, which has served in the formation of the latter. This is why the venous blood of the kidneys is warmer than the arterial blood, and according to Claude Bernard the temperature of the hepatic vein, which brings back the blood from the liver to the heart, is the highest in temperature, especially during the process of digestion, at which time the liver is very active, and the chemical processes which take place are also numerous and intense. This is sufficient to show the dependence of the generation of heat upon the chemical activities of the body.

By reason of natural and normal cessations of the phenomena which are instrumental in generating and liberating heat, it is impossible for the temperature of a being to be absolutely equable. Even with the most warm-blooded animals there are many normal variations. In a sound man, in normal condition, these variations take place within the space of about 24 hours. The temperature is highest from 10 o'clock, or midday, to 6 or 7 o'clock in the evening, reaching its lowest point between midnight and 6 o'clock in the morning. Violent exercise, of course, increases it several degrees, and the process of digestion is accompanied by a slight fever. In a word, a multitude of circumstances occur each hour which render variable, within certain limits, it is true, the generation of heat. In addition, and this is quite natural, according to the explanations given above, the temperature is not the same in all portions of the organism. Certain portions are more thermogenic than others, and others are more exposed to a loss of heat. The calorific topography of the organism is accurately known. We know that the hepatic vein is one of the warmest points of the body, its position being a protected one, and containing, as it does, blood heated by the intense chemical action which takes place in the liver. The brain has probably the same temperature as this vein. On the other hand, the skin always shows a much lower temperature (3°, 5°, or 6°) than that of the rest of the organism, suffering as it does considerable loss from radiation.

Leaving the question of external heat, we find that internal temperature is the direct result of two factors, thermic generation and waste. Heat generated is the result of chemical processes, infinite in variety, of which the body is the theatre, processes among which that of oxidation holds a predominant place. As soon as oxidation is retarded, there follows a difficulty in breathing, accompanied by a lowering of the temperature. The cause of this is the diminution itself and the

reaction it probably exercises upon the other thermogenic chemical actions. As to waste, this is incurred in accordance with well-known physical laws, and with warm-blooded animals it is sometimes facilitated and sometimes diminished by the action of the regulating mechanism placed under the dependence of the nervous system, a mechanism which in its normal condition tends to preserve for the organism a temperature nearly constant, diminishing the losses when the production of heat is feeble or insufficient in respect to the temperature of the surrounding medium, and augmenting these losses, on the contrary, when the atmosphere is too high, or when the production is so great that it tends to inflame the bodily organism.

The only difference, from the physiological standpoint, in the calorification which exists among warm-blooded and cold-blooded animals is, that with the latter the production of heat is slight and the regulating apparatus absent. These species engender little heat, and are unable to regulate their losses. They also follow the variations in the outside temperature almost to as great an extent as inanimate objects; whereas warm-blooded animals conform in a less degree to the outer atmosphere, and also with less impunity.

II.

We are now to consider between what limits of temperature organic life can be maintained. Animals of the highest temperature, protected though they are against the extremes of heat and cold, *can* be placed under conditions which render these protective means inadequate, and this in a state of nature and apart from all experimentation.

A word first on the thermic variations which occur in the inhabited zone of our planet; a zone limited in extent, comprising an average of 8 to 10 kilometres in altitude, its elevations and depressions being about equal in distance from the level of the sea; a zone exceedingly small when compared with the diameter of the earth. Beyond the limits of this region life has never existed, or at least exists no longer. We are more especially interested in that portion of the earth which can support organic life. The extreme points of temperature observed in the atmosphere are - 70° and + 56° C. The former observation was made at Iakoutsk, the latter at Mourzouk. These are said to represent very exactly the extreme limits, forming a difference of 125° or 130°C. At these far distant points human life is possible, and also that of certain animals. In the ocean the thermometric digressions are not as great. According to Wyville Thompson, the temperature of the Atlantic Ocean reaches 0° at a depth of only 4,200 metres; at 6,000 metres it registers 5°; at 800 metres, 4°, and at 2,000 metres it is 3°. About the same can be said of the Pacific Ocean. Should the temperature upon the surface or at the bottom of the sea descend lower than —1° or —2° the water freezes. It is not necessary for us to consider this

point however, since it is complicated by the introduction of a nev factor—the suffocation of the inhabitants of the water as a result o this congelation. The Mediterranean Sea is less cold, the temperatur at the bottom being about 12° or 13°. The Red Sea rises to 21°, an at the surface to 32°. The variations are less in the center, not exceed ing 34° C. It is therefore on the earth and in the air that the extreme of temperature are found. The immense influence of the rays of th sun upon temperature should be taken into account. A thermomete which registers 27° in the shade will rise to 31° when placed in th sun, and when resting upon a bit of black cloth it will reach 80°. A thermometer placed on the helmet of a cuirassier and exposed to th sun will rise to 60° or 70°, and in a compartment of a furnace it rises t 75° C. On the other hand we must not forget that life exists in region where the temperature reaches 90° and 98° C (Hooker, Flourens, etc.) This conclusion, therefore, is reached, that there are some creature which can live at +100° and others at —60° or —70°. These figure represent the extremes of temperature to which living beings are ex posed under actual terrestrial conditions, but they do not represen those which certain of these classes can resist, for certain spores o bacteria resist more than +100° and —100° C, according to recent ex periments. Let us admit at the start, to simplify matters, that life ca be sustained at —150° and at +150°. Are all these creatures able t sustain life with impunity, even for a short time, in such extremes o temperature? Possibly so, but only for a limited space of time, an surrounded by a nonconductor. This proves nothing; the only inte esting phases of this question are the facts or experiments which rela to the results obtained by organisms remaining in such extremes for prolonged length of time—interesting where they succumb, being su focated or frozen, as well as when they are able to survive by pr serving their normal temperature. We will not dwell upon those case which are both numerous and interesting, where man and animal ha endured for a few moments or seconds extremes of temperature, on considering the cases where their continuation is sufficiently prolonge for the temperature to affect them.

There is for every species of animal and vegetable, indeed even fo each variety, a thermic *optimum*, that is to say, an average of temper ture which is most favorable to its growth and development. It shou not be forgotten, however, that with all species of organic life a certa adaptation is possible, the limits of which are more or less restricte In many instances it is possible to sustain life among animals in medium which would have been fatal to them if they had been sudden introduced into it, by carefully managing the conditions and trans tions. This fact is especially recognized in chemical elements, of whic many instances have been given. It is true as well of thermic cond tions. At the same time, even when adaptations are made, new env ronment acts on the organism, influencing and modifying its structu

or functions, and it may be said that for all life there is a degree of temperature which is more favorable than any other to its perfect development. The limits of temperature thus favorable to a given class are surprisingly narrow. This is especially true in the case of microbes. The bacillus of butyric fermentation is most active at 40°. At 42° it multiplies more rapidly, but diminishes in activity. At 45° it no longer effects fermentation. For alcoholic fermentation the most favorable point is between 25° and 30°, although it ceases at zero—the freezing point, and at 100°—the boiling point. The microbe of carbuncular diseases thrives at 37° to 39°. At 41° it dies. Convincing evidence of this is given by Pasteur, who has shown that a fowl in normal condition, its temperature being from 41° to 42°, can not become inoculated with a disease of this kind. If you cool the fowl artificially by means of cold water, so that its temperature diminishes 2° or 3°, the microbe multiplies abundantly in the blood of the fowl and kills it, at least if the cooling process is continued. If that ceases, a return to the normal condition of the animal will dissipate the disease. A temperature of 35° is most favorable to lactic fermentation. The fermentation of putrid matter is less restricted. It is carried on anywhere from 0° to 40°, although the most favorable points are between 15° and 35°. Examples of this kind may be given in great numbers. What is more interesting, however, than this enumeration, is the study of the results which are induced by subjecting a given microbe to a degree of temperature higher than that which is best adapted to it, not sufficiently high, however, to be fatal to its existence. Very evident modifications are by this means produced in its physical condition. It becomes weakened, and there is a marked diminution in its vitality. This fact is the basis of the interesting processes of preventive vaccinations, of which Pasteur has given us so many striking and useful examples. Only a slight increase of temperature is needed to transform a dangerous microbe into an invaluable auxiliary in the art of healing or preventing infectious diseases. On the contrary spores of bacteria can be subjected to considerable variations of temperature without being productive of any modifications. These spores withstand admirably extremes of temperature, for instance −100° and +100°, the bacteria which spring from these losing none of their virulence. Some species of bacteria may be frozen for many months and live. This is true of the bacteria of typhoid fever, according to Fraenkel and Prudden. Contrary to the general impression, congealing does not purify impure water.

It is interesting to note that the sensibility of common leavens, as referred to their thermic variations, is repeated in soluble leavens—that is to say, with the products of the activity of certain cellules, which exhibit some of the qualities of the ordinary leavens. Thus pepsin is active anywhere between 37° and 40°. At 50° it acts in a less degree, becoming almost inactive at 90°. The pancreatic juice exercises its

chemical action most thoroughly at 40°. At 20° it acts slightly, and at 60° its action ceases entirely. In considering the tissues of complex organisms, we ascertain analogous phenomena. Protoplasms of different organisms, although they are often supposed to be identical, present very unequal opposition to thermic variations. In one case it dies at 30° or 20°, in others it lives at 0°, at —5°, at —10° (Nordenskiold). We know that eggs of birds require for their development a temperature, narrow in limits, which can not be overstepped without destroying the embryo, or producing malformations. Eggs of invertebrates are somewhat similar, but their exigencies are less restricted, and they accommodate themselves to greater differences of temperature.

Every being, to live and move, requires environment of a certain temperature. Some are less exacting, and adapt themselves to variations; others, on the contrary, can not endure even slight changes. Some seek the cold, others—heat; but all in a marked manner, as we know from the difficulties experienced in acclimating species to a new climate. A few examples will not be out of place. The polar region, with its prolonged and rigorous cold, and our high summits, always clothed with a mantle of ice, produce a fauna and flora which is peculiar to them. In these regions, where man is able to exist only at the cost of a considerable effort, there are mammals, insects, plants of all kinds, which can reach here only their full growth and perfect development. In a temperate or warm climate they lose their vitality and perish, never in reality becoming acclimated. Warm-blooded animals which live in these regions have the same temperature as their co-species in warm climates. They maintain themselves by appropriate food and a heavy growth of fur, discarded by them when the weather moderates. Captain Black has observed in Siberia when the external temperature was at —35°, that the temperature of a fox was 41°, making a difference of 76°. The reverse of these polar regions and glaciers are the hot springs. Here also we find a characteristic fauna and flora. Many observers have drawn up a list of sea weeds, infusorials, and fungi, living in the waters, the temperature of which varies from 50°, 60°, and even 90° C, and that thrive and multiply.

Between the coldest regions, which some species delight in, and the hot springs, or the tropical regions, where others attain their highest development, we find grades of organisms whose resistance to extremes of temperature is less and which prefer more temperate surroundings, manifesting a partiality for such and such a point in the thermic scale. To be assured of these preferences one has only to consult the documents showing the distribution of species and their acclimation. The most curious fact disclosed by the preceeding data is the great resistance of the protoplasm of certain creatures to temperatures, which, judging from other cases, one would suppose must be fatal. The protoplasm in certain cases can sustain a temperature of zero, or lower still, and others can live at 90° and even higher temperatures. This is

a remarkable fact which neither physiologists nor chemists are able to explain.

In short, there exists among organisms a certain number of species, vegetable or animal, able to withstand extremes of temperature, and to live normally therein, while the majority can live only in more uniform and moderate temperatures. We will now see by what means the different organisms withstand or succumb to temperatures, other than those to which they naturally accomodate themselves, and to what influences they are subjected.

Let us consider first heterothermic organisms, or cold-blooded animals, which follow the oscillations of the surrounding atmosphere, and the temperature of which rises and falls proportionately on account of the absence of the regulating apparatus by which they could control their own production and loss of heat. These organisms possess a sensibility which is regardless of variations in their temperature. They can undergo with impunity oscillations in the atmosphere about them which would endanger the life of warm-blooded animals, possibly destroying it entirely. The latter, man included, can not live a moment if their internal temperature exceeds about 45° (113° F.) The cold-blooded animals can vary their temperature within very considerable limits. The enumeration of the latter would not be particularly interesting; it is sufficient to say that the temperature of cold-blooded animals of our countries varies according to circumstances from 0° to 35° and 40°. That which arrests our attention is the summing up of the influence of different temperatures on the functions of these animals. As a matter of course, temperatures exist which are not deadly, which are consistent with the life of these creatures. We shall see later in what way the extremes of temperature act.

It is a well-substantiated fact, by means of experiments which, though not numerous, are very exact, that there is for every living creature a degree of heat which is absolutely indispensable in order that its development be as complete as possible. On this point we have had for several years, thanks to the valuable labors of Boussingault, most interesting data. Being given a certain vegetable we can estimate that the time which elapses between the appearance of its vegetation and its complete maturity is short in proportion to the height of the temperature at which it vegetates, and long in proportion to its degree of lowness, exception being made, let it be understood, of thermic conditions which are dangerous or fatal. Otherwise stated: Being given a plant which lives between 15° and 30°, of which the thermic *optimum* is 25°, its development will be slower in a constant temperature of 15° than in one of 20° to 25°, and the retardation is proportionate to the thermic difference. It seems that in whatever latitude or climate it thrives, there exists there for the plant just the quantity of heat necessary for its development. It is easy to prove that this hypothesis is exact and conforms to the facts of the case. The followin g is an example: From

the day when a seed germinates to the moment when the plant reaches its maturity an average is taken of the temperature for each cycle of 24 hours. Afterwards an average is made of these averages for all the period which has passed between the two moments mentioned above and this average is to be multiplied by the number of days which have passed. Suppose this action of the plant has taken 90 days, and that the average of averages is 17, then you obtain the figure 1530, which represents the degrees of heat furnished in 90 days,—a day being taken as a unit of time. A very interesting fact is, that, if the same observations are made with the same species of plant under different thermic conditions, or in a different climate, the same figure is obtained, although the number of days necessary to the development may vary from simple to treble, according to the climate. The study of vegetable physiology is rich in interesting facts from the standpoint which is now occupied. In this way different seeds are very differently influenced by cold. One does not germinate below 15°, while others germinate at 4°, and still others at zero. One plant developes best at a temperature which is fatal to another.

In the animal kingdom analogous facts have been observed in a very exact manner. A little fresh-water mollusk (*lymnée*) furnishes Carl Semper, the learned zoölogist of Würzburg, with very interesting facts in this connection. Below 12° this animal, although leading an active life and taking its food regularly, underwent no growth, though it was able to reproduce, its eggs developing perfectly. From 12° to 25° (which is its most favorable temperature) its assimilation was perfect, and the animal grew and developed. Semper remarks that these mollusks, subjected permanently to a temperature of 10° or 12°, remain small and cease to develop. They produce a dwarfed breed, which in their turn reproduce normally, remaining, however, smaller than the other lymnées. On the other hand, an unnaturally large species can be produced by maintaining the mollusks by artificial means at the highest point of temperature. There is still another fact which accords with that of which we have just spoken. A well-known naturalist, Mœbius, has discovered that the same species of marine mollusks common to the Baltic and to the coast of Greenland differ greatly in size. At the Baltic they are small and have a thin shell, while on the coast of Greenland they are much larger in size and are provided with a thick shell. This is explained by the fact that in the Baltic the variations of temperature are more frequent and the cold is more intense than in Greenland, in consequence of which the development of the mollusk is more difficult and intermittent.

Temperatures lower than this most favorable point have a marked effect upon animals and plants, which shows itself in the latter by a retardation of development which at the same time becomes less complete. On the contrary, temperatures not fatal, but relatively high in regard to their natural condition, favor their growth, which becomes proportionately rapid and complete. It is thus with the eggs of certain

species of crustacea, as the *apus* and *branchipus*, which develop between 0° and +30°, accomplishing their complete evolution in 24 hours at a temperature of 30°, while between 16° and 20° it takes weeks to obtain the same result. Tadpoles hatch in 10 days at a temperature of 15.5°; at 10.5° it requires 15 days. Notice how various are the requirements of different creatures' in the matter of temperature. That of 36°, so favorable to *branchipus*, is fatal to many, excepting the entire animal life of Arctic seas, and also, as I have already shown, a number of species of the Mediterranean, especially those which inhabit the seashore and can not adapt themselves to temperatures in pools heated by the summer sun.

There is therefore for every species a certain temperature at which development is most rapid and life most easy. The limits of this thermic condition vary considerably according to the species and even the variety. Subjected to the influence of a lower temperature than that which is most favorable, each animal's development is retarded, in different degrees, and often fails to attain perfection. If exposed to a higher temperature than that which is best adapted to them, disturbances are produced, alimentation becomes impaired, and the animal—or vegetable—begins to pine, as is also the case with man in excessively hot climates.

This influence of temperature on life is not only manifested in degree and rapidity of development, it also appears in other phenomena; coloration, for instance. In this way Weissmann has shown that two butterflies, *Vanessa levana* and *Vanessa prorsolevana*, differing in coloration upon certain points, have been looked upon as belonging to two distinct species, whereas in reality they represent but one. The difference is simply a question of temperature. One comes from an egg laid during the winter, and one from one laid in the summer, but it is easy to obtain at will either variety from the same egg by heating or cooling artificially, according to the case. A more important question is the influence which the temperature exerts upon sexual development. Cold retards and sometimes arrests it; a certain degree of temperature favors and accelerates it; and it is well known that sexual development in man himself is hastened by the influence of a hot climate. In Cuba, and other warm climates, a girl attains maturity at 12 years. But the temperature must not be too high either. Crustacea kept for several weeks at 19° do not acquire sexual activity, whereas at 9° or 10° it is acquired in 2 days.

Temperature thus exercises considerable influence upon all organisms. An interesting proof of these effects on the intensity of life (if it may thus be called) is furnished by a study of the influence exercised by this factor on the action of poisons and medicines. Alexander von Humboldt, and after him many investigators, have noted that this action is more instantaneous and rapid in high temperatures (which are neither fatal nor dangerous in themselves) than at a lower degree.

Occasionally in the latter case, a poison becomes perfectly inactive and inoffensive, although it would prove deadly if the temperature rose a few degrees. This fact is now well understood, and account of it is taken in dealing with toxicology. This explains the frequent contradictions between the conclusions of different investigators, because they have not experimented under the same thermic conditions, and most of them have failed to note the exact temperature. Another proof of temperature on the general functions of the organism is the proof furnished by a comparative study of the resistance of beings to asphyxia. When the temperature is low, asphyxia is slower and more difficult. A frog immersed in water, its head covered, and only cutaneous respiration possible, will survive from 6 to 8 hours with the water at 0°. At 15° or 16° it will only live a fourth of this time. To consider another phase of the same question: poisonous plants are more deadly under thermic conditions favorable to their growth than when struggling to live in an atmosphere colder or warmer than that adapted to their peculiarities.

We have been considering so far the influence of thermic variations which are not of necessity deadly. We will now turn our attention to those which are fatal in their effects, first observing that the effects vary according to the species, and also according to certain conditions, some intrinsic or inherent in the organisms, others extrinsic or relative to the conditions under which the thermic extremes occur. It is well known, for example, how unequal is the resistance of vegetables and seeds to extremes of heat and cold. Some freeze easily, others with difficulty. It depends much upon their bulk and the proportion of water contained in their tissues. Some do not die immediately after freezing, even when the thawing is rapid, others only survive when the thawing is slow and gradual. A very important factor is the condition of the vitality. We know that spores of bacteria and seeds of plants withstand degrees of temperature at which neither bacteria nor plants could live. This fact is so well known that it is only necessary to touch upon it.

It may seem strange that torpid organisms have more resistance than the higher species to adverse circumstances; yet it is true that the less active the life the less vulnerable it is, and less can exterior forces disturb the functions which are already almost dormant and torpid. Cold kills a great number of the lower organisms by reason of the disorganization of the tissues which takes place when congealed, and this disorganization is complete in proportion to the amount of water which the tissues contain. There are, however, many organisms among the cold-blooded class which die before they reach the point of freezing. Invertebrates and plants belonging to warm climates, as well as many microbes, succumb when the thermometer has only reached 0°. In which case the method of death is different, it being produced by a slackening of all the functions. Extreme heat kills plants and animals

of the cold-blooded class at different degrees of intensity, being much higher, however, than those at which warm-blooded species succumb. In the one case they are, in plain language, dried up, the heat depriving the tissues and functions of the water necessary to their existence; in the other, the vital material coagulates and life is no longer possible, this cause being the more general one. This congealing, however, is not always fatal, even in the case of animals of high organization. It has long been known that in the northern part of America and Russia travelers transport frozen fishes, rigid and brittle, which being placed in water of a temperature of 8° and 10° regain their activity, although they may have been frozen for 10 or 12 days. Science has refused to believe these statements, but careful experiments have authenticated them. In 1828 and 1829 Gaymard froze several toads thoroughly, and they returned to their normal condition and activity on being thawed. Care must be taken that both the freezing and thawing are gradual. This is the principal precaution to be taken in making experiments of this sort. The great English naturalist, Hunter, believed that the life of man could be prolonged by being frozen from time to time. He thought that if frozen and revived several times in the course of a few years the limits of life could be considerably extended. Unfortunately the experiment brought death instead of prolonging life.

Let us now consider the warm-blooded organisms, the creatures whose temperature is more stable and does not follow the thermic variations in the atmosphere about them. A mammal or a bird withstands a considerable amount of cold. If indigenous to a cold region, protected by thick fur or warm plumage, and in a position to secure the nourishment it needs, it can live in a temperature at 50° below zero, its own temperature remaining fixed and normal. It is true also of man, who by protecting himself by appropriate clothing, easily withstands quite as low points of temperature, particularly if there is an absence of wind. We all know by experience that a moderately cold temperature with wind blowing is much harder to bear than intense cold without wind. The explanation of this fact is very simple. The wind tends to constantly deprive the body of the layer of warm air, which forms between the body and the clothing, and to facilitate radiation and loss of heat by substituting for it cold air.

But what happens under experimental or natural conditions when an animal or man is subjected to the action of intense cold? The organism withstands it for a certain length of time, but this endurance has its limits, variable, it is true, according to species and conditions. A moment necessarily arises, if the cold be sufficiently severe or prolonged, when the organism is no longer in a state to generate sufficient heat to withstand the cold or, what is practically the same, when the loss is too considerable though the generation were sufficient. From that moment the temperature of the animal begins to decrease. This diminution is compatible with life up to a certain point, which varies accord-

ing to the species. Some animals can live, their temperature being as low as 15° or 20°. The temperature of a rabbit, for example, can fall from 38° or 40° to 20°. That of man may fall to 26°, 25°, and even 24° without resulting in death, according to authentic observations made by Reinke and Nicolayssen upon drunkards. It does not seem, however, according to Claude Bernard, Magendie, and other physiologists, that one can with impunity lower the temperature of warm-blooded animals below 20° C. At 20° death is almost inevitable; below that point it is certain. The nervous system is destroyed, involving the entire organism. The blood becomes weakened and unequal to perform its work.

Surgeons of large armies have left us valuable information concerning the effects of intense cold on human beings. In the case of men who are tired and jaded, intense cold is immediately fatal—especially where it is a sudden immersion in very cold water, for in this case the loss of bodily heat is great. Larrey states that in crossing the Beresina, men perished instantly upon entering the water, and Virey and Desgenettes testify to similar cases. With some death was caused by cerebral congestion, with others it was caused by anæmia of the brain. When the action of the cold is less sudden, but more prolonged, the result is otherwise. A general benumbing of the body takes place,—of the senses, the brain, the intelligence, a gradual torpor, an invincible sleep from which none awake. "Whoever seats himself, falls into a sleep, and whoever sleeps awakes no more," said Solander. Death is produced by a slow paralysis of the nervous system or by asphyxia.

Warm-blooded animals are enabled to resist the cold by reason of their very active thermogenesis, which prevents them from becoming chilled. But once let their resistance be overcome and they succumb to much higher temperatures than those which overcome cold-blooded organisms. Many of the latter can endure 10°, 5° and even 0° without perishing. The former die when once their internal temperature falls below 18° or 20°. A more forcible reason why the latter can not resist intense cold is because it destroys the portion congealed and therefore the entire organism.

Life is also difficult at high temperatures. Man and some animals can, it is true, remain several minutes in a sweating-room in which the temperature is very high—even 100°, 120°, and 130° (Tillet and Duhamel, Delaroche and Berger, etc.)—but under these conditions the stay is always very short; if prolonged beyond 10 or 15 minutes the experience would prove fatal. The perspiration is so excessive that it produces a loss of the heat which is necessary to counterbalance the temperature to which the atmosphere tends to subject the organism. There is another point to be noticed. Air is a bad conductor, and hot air heats the body incomparably less than water subjected to the influence of heat. Water, on the contrary, is an excellent conductor. It is impossible to endure for any time the contact of water at 50° and 60°.

Moist air is a better conductor than dry air, and it is still better if charged with steam. Thus man can easily remain for 10 minutes in a sweating room of dry air at 90° or 100°, but could not endure the same length of time in moist air at even a lower temperature. He would soon be overcome in the latter case at 90° or 100°. That which is true of high temperature is naturally true also of low. Dry air is not so good a conductor as moist, and moist air is inferior to water as a conductor. One can live in air at degrees of cold which would surely be fatal if the environment were a liquid. We have already stated how weak is the resistance of warm-blooded organisms to high degrees of temperature. In fact, in spite of perspiration and exhalations of vapor by the lungs, it is often impossible for the equilibrium to be maintained, and the organism becomes overheated. Its temperature can be increased very little without being fatal. It endures a decrease of 15° or 20° in its internal temperature, while an increase of more than 5° or 6° would be dangerous. If the temperature of man or mammal reaches 44° or 46° death results. Birds can exist at a point somewhat higher. First comes a period of great excitation and convulsions, from which it falls into a comatose state, followed by death. This result has not yet been elucidated as clearly as desirable. Death under all circumstances is sufficiently complex, but its complexity varies according to its conditions. There are dis-arrangements in the chemistry of the muscles, a portion of which undergoes a change. There are affections of the blood which may be lacking in oxygen though not presenting indications of any particular poison. Notwithstanding Claude Bernard, it is the thermic rigidity and the muscular injury which are most serious. These are of themselves sufficient to cause death, for their effect is to arrest respiration and circulation.

In conclusion we can say that there is, in the case of heterothermic organisms, great endurance of intense cold, and, to a certain extent, of heat, despite the very marked action of thermic variations upon their organizations. In the case of homeothermic organisms we find moderate endurance of low temperature, and very little resistance to an increase of internal temperature. For then a low temperature is accompanied with much less danger than a high one. The former has to be pronounced to entail death, whereas a slight rise of temperature beyond a certain point will produce immediate and fatal results.

Between these two classes of organisms there is another group called hibernating animals. These are, for the most part, rodents, which, at the approach of cold weather, make an underground habitation well covered with moss and other substances, where they remain motionless, rolled up like a ball, during the bad season, sleeping during the entire time, torpid, neither eating nor drinking. With these animals the internal temperature becomes very low, following somewhat the thermic variations. They scarcely breathe. Their respiratory combustions diminish, and their temperature descends to 20°, 15°, and 10°, and even

lower. Horwath has stated that the temperature of a hibernating marmot reached 2°. As soon as warm weather returns they wake up, become active, and their temperature becomes normal. They are much leaner than before their winter's sleep, having lived for several months on their own accumulation of fat. Here is an animal alternately warm blooded and cold blooded in summer and winter. The cause of this strange alternation has not yet been explained and is exceedingly complicated. With them the thermic production is relatively slight. It is cold that determines the hibernal sleep, for it is easy to produce this by subjecting the animal to prolonged cold by artificial means. No investigations to my knowledge have been made of the resistance of this species of animals to heat. I mean to say, of the elevation of internal temperature above the normal level of the summer, but it is not to be supposed that their endurance would be as great as in case of extremes of cold.

This class of hibernating animals unite the heterothermic and homeothermic species, and serve to show once again that everything in nature is related. Sudden leaps are no longer held to exist in the physiology of creatures which are similar in organic structure; science finds everywhere transitions.

Finally, all living organisms generate heat, more or less it is true, according to their activity and their structure, but all produce it. In the same manner all organisms submit to the influence of the surrounding atmosphere, although all do not follow the variations. For each there is a degree of heat which is best adapted to its perfect development. All die as soon as the external temperature reacts on the internal temperature to such an extent that the latter is carried above or below a certain point. The only difference is in the facility with which this action of the external temperature operates upon the internal temperature of the organism.

MORPHOLOGY OF THE BLOOD CORPUSCLES.*

By Charles-Sedgwick Minot.

If one goes through the very extensive literature dealing with blood corpuscles one finds the most divergent views defended, and can hardly reach clear ideas, for the conceptions do not agree among themselves, either as to their structure or as to the development of the corpuscles. According to some the red corpuscles arise from the white; according to others the white corpuscles arise from the red; and according to still others both kinds arise from indifferent cells. In regard to one point only is the majority of investigators united, namely, in the silent assumption that all blood corpuscles are of one and the same kind in spite of the absence of the nucleus in mammalian corpuscles. It is just this assumption that has caused endless confusion, and the morphology of the blood corpuscles can be cleared up only by starting with the recognition of the fundamental difference between nucleated and non-nucleated corpuscles. Further, it must be recognized that no corpuscles, neither red nor white, arise from nuclei.

The origin of red corpuscles from nuclei has been maintained several times. This notion is based upon defective observations. It is very easy in the chick, for example, to convince oneself that the first blood corpuscles are cells; in the area vasculosa, at the time of the blood formation, the red blood cells are readily seen, in part lying singly, in part in groups (blood islands), adherent to the vascular walls; the free cells are constituted chiefly by the nucleus, which is *surrounded by a very thin layer of protoplasm*, which is very easily overlooked, especially if the preparation is not suitably stained; this explains, I think, the statement made by Balfour (Works, vol. I) and others, that the blood corpuscles consist only of nuclei. By following the development along further we find that the protoplasm enlarges for several days, and that during the same time there is a progressive diminution in size of the nucleus, which however is completed before the layer of protoplasm reaches its ultimate size. The nucleus is at first granular, and its nucleolus, or nucleoli, stands out clearly; as the nucleolus shrinks it becomes

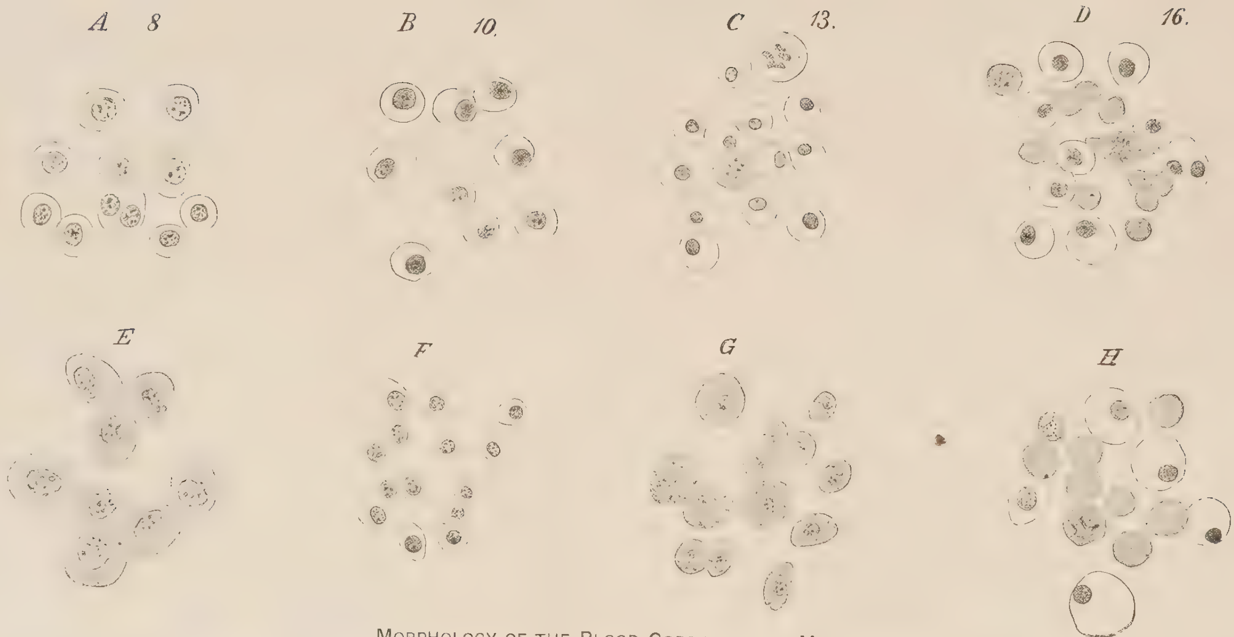
* From the *American Naturalist*, November, 1890, vol. XXIV, pp. 1020–1023.

round, and is colored darkly and almost uniformly by the usual nuclear stains. This species of blood corpuscle occurs in all vertebrates, and represents the *genuine blood cells.* According to the above description we can distinguish three principal stages: (1) young cells with very little protoplasm; (2) old cells with much protoplasm and granular nucleus; (3) modified cells with shrunken nucleus, which colors darkly and more uniformly. I do not know whether the first form occurs in any living adult vertebrate, although the assumption seems justified that they are the primitive form. On the other hand, the second stage is obviously that characteristic of the Ichthyopsida in general, while the third form is typical for the Sauropsida. Therefore the development of the blood cells in amniota offers a new confirmation of Louis Agassiz's law (Haeckel's Biogenetiches Grundgesetz).

The blood cells of mammals pass through the same metamorphoses as those of birds; for example, in rabbit embryos the cells have reached the Ichthyopsidan stage on the eighth day; two days later the nucleus is already smaller, and by the thirteenth day has shrunk to its final dimensions.

The white blood corpuscles appear much later than the red cells, and their exact origin has still to be investigated, for it has not yet been determined where they first arise in the embryo; nevertheless we may venture to assert that they arise outside the vessels. The formations of leucocytes outside of the vessels is already known with certainty to occur in later stages as well as in the adult. The sharp distinction between the sites of formation of the red and white cells appears with special clearness in the medulla of bone in birds, as we know from the admirable investigations of J. Denys (La Cellule, tome IV). The white blood corpuscles then are cells, which are formed relatively late, and wander into the blood from outside.

The non-nucleated blood corpuscles of adult mammals are entirely new elements which are peculiar to the class, and arise neither from red nor yet from white blood cells. Their actual development was first discovered (so far as I know) by E. A. Schäfer, who has given a detailed account of the process in the ninth edition of Quain's Anatomy, and has shown there a full appreciation of the significance of his discovery. Unfortunately Schäfer's important investigations have received little attention. Kuborn has recently confirmed Schäfer's results in an article (*Anatom. Anzeiger*, 1890) on the formation of blood corpuscles in the liver. One can readily study the process in the mesentery and omentum of human and other embryos. The essential point of Schäfer's discovery is that the non-nucleate corpuscles have an *intra*-cellular origin, and arise by differentiation of the protoplasm of vaso-formative cells. Several corpuscles arise in each cell without participation of the nucleus; they are therefore specialized masses of protoplasm, and may perhaps best be compared to the plastids of botanists. I venture to propose the name of blood-plastids for these structures, since the



MORPHOLOGY OF THE BLOOD CORPUSCLES BY MINOT.

The four upper figures (*A*, *B*, *C*, *D*) represent corpuscles from rabbits, the number giving the ages of the embryos in days; *E*, from a shark; *F* and *G*, from a bird; *H*, from man.

[All the figures are magnified 545 diameters.]

term corpuscle (globule, Körperchen) has no definite morphological meaning.

Sonsino (*Arch. Ital. Biol.* XI) affirms that the red blood cells transform themselves into plastids. I have, however, never been able to find the intermediate forms in my own numerous preparations. I deem it probable that he has seen merely the degenerating stages of the red cells.

The present article is an abstract of a communication made in August last to the American Association for the Advancement of Science. Since then Howells's memoir on the blood corpuscles has appeared (*Journal of Morphology*, IV, 57). The author describes the ejection of the nucleus from the red cells, and believes that this results in the formation of red plastids. The process is, I think, really degenerative, and the resemblance between the non-nucleated body of the cell and a true plastid, is not one of identity. Certainly, until proof is offered that the observations of Schäfer, Kuborn, and myself, upon the intra-cellular origin of the plastids are proved erroneous, the emigration of the nucleus of the red cells can not be held to result in producing plastids, but only to be degenerative. That the red cells degenerate and disappear has been known; Howells's valuable observations indicate the method of their destruction.

The above review shows that the vertebrate blood corpuscles are of three kinds: (1) red cells; (2) white cells; (3) plastids. The red and white cells occur in all (?) vertebrates; the plastids are confined to the mammals. The red cells present three chief modifications; whether the primitive form occurs in any living adult vertebrate I do not know; the second form is persistent in the Ichthyopsida, the third form in the Sauropsida. According to this we must distinguish:

A.—One-celled blood, *i. e.*, first stage in all vertebrates; the blood contains only red cells, with little protoplasm.

B.—Two-celled blood, having red and white cells; the red cells have *either* a large, coarsely granular nucleus (Ichthyopsida), or a smaller, darkly staining nucleus (Sauropsida, mammalian embryos).

C.—Plastid blood, without red cells, but with white cells and red plastids; occurs only in adult mammals.

Mammalian blood in its development passes through these stages, as well as through the two phases of stage B, all in their natural sequence; the ontogenetic order follows the phylogenetic.

I pass by the numerous authors whose views conflict with mine, partly because the present is not a suitable occasion for a detailed discussion, partly because those authors who have asserted the origin of one kind of blood corpuscle by metamorphosis from another have failed to find just the intermediate forms; it seems to me therefore that most, at least, of the opposing views collapse of themselves.

WEISMANN'S THEORY OF HEREDITY.*

By George J. Romanes.

The recently published translation of Professor Weismann's essays on heredity and allied topics has aroused the interest of the general public in the system of his biological ideas. But seeing that his system, besides being somewhat elaborate in itself, is presented in a series of disconnected essays, originally published at different times, it is a matter of no small difficulty to gather from the present collection of these essays a complete view of the system as a whole. Therefore I propose to give a brief sketch of his several theories, arranged in a manner calculated to show their logical connection one with another. And in order also to show the relation in which his resulting theory of heredity stands to what has hitherto been the more usual way of regarding the facts, I will begin by furnishing a similarly brief sketch of Mr. Darwin's theory upon the subject. It will be observed that these two theories constitute the logical antipodes of explanatory thought; and therefore it may be said, in a general way, that all other modern theories of heredity—such as those of Spencer, Haeckel, Elsberg, Galton, Naegeli, Brooks, Hertwig, and Vries—occupy positions more or less intermediate between these two extremes.

When closely analyzed, Mr. Darwin's theory—or "provisional hypothesis of *pangenesis*"—will be found to embody altogether seven assumptions, viz:

(1) That all the component cells of a multi-cellular organism throw off inconceivably minute germs or "gemmules," which are then dispersed throughout the whole system.

(2) That these gemmules, when so dispersed and supplied with proper nutriment, multiply by self-division, and under suitable conditions, are capable of developing into physiological cells like those from which they were originally and severally derived.

(3) That while still in this gemmular condition, these cell seeds have for one another a mutual affinity, which leads to their being collected from all parts of the system by the reproductive glands of the organism; and that, when so collected, they go to constitute the essential material of the sexual elements, ova and spermatozoa being thus nothing more

*From *The Contemporary Review*, May, 1890, vol. LVII, pp. 686-699.

than aggregated packets of gemmules which have emanated from all the cells of all the tissues of the organism.

(4) That the development of a new organism out of the fusion of two such packets of gemmules is due to a summation of all the developments of some of the gemmules which these two packets contain.

(5) That a large proportional number of the gemmules in each packet, however, fail to develop, and are then transmitted in a dormant state to future generations, in any of which they may be developed subsequently, thus giving rise to the phenomena of reversion or atavism.

(6) That in all cases the development of gemmules into the form of their parent cell depends on their suitable union with other partially developed gemmules, which precede them in the regular course of growth.

(7) That gemmules are thrown off by all physiological cells, not only during the adult state of the organism, but during all stages of its development. Or in other words, that the production of these cell seeds depends upon the adult condition of parent cells, not upon that of the multi-cellular organism as a whole.

At first sight it may well appear that we have here a very formidable array of assumptions. But Mr. Darwin ably argues in favor of each of them by pointing to well-known analogies, drawn from the vital processes of living cells, both in the protozoa and metazoa. For example, it is already a well-recognized doctrine of physiology that each cell of a metazoön, or multicellular organism, though to a large extent dependent on others, is likewise to a certain extent independent or automatous, and has the power of multiplying by self-division. Therefore, as it is certain that the sexual elements (and also buds of all descriptions) include formative matter of some kind, the first assumption—or that which supposes such formative matter to be particulate—is certainly not a gratuitous assumption. Again, the second assumption—namely, that this particulate and formative material is dispersed throughout all the tissues of the organism—is sustained by the fact that both in certain plants and in certain invertebrate animals a severed portion of the organism will develop into an entire organism similar to that from which it was derived, as for example is the case with a leaf of begonia and with portions cut from certain worms, sea-anemones, jelly-fish, etc. This well-known fact in itself seems enough to prove that the formative material in question must certainly admit (at all events in many cases) of being distributed throughout all the tissues of living organisms.

The third assumption—or that which supposes the formative material to be especially aggregated in the sexual elements—is not so much an assumption as a statement of obvious fact; while the fourth, fifth, sixth, and seventh assumptions all follow deductively from their predecessors. In other words, if the first and second assumptions be granted and if the theory is to comprise all the facts of heredity, then the remaining five assumptions are bound to follow.

To the probable objection that the supposed gemmules must be of impossibly minute size—seeing that thousands of millions of them would require to be packed into a single ovum or spermatozoön—Mr. Darwin opposes a calculation that a cube of glass or water having only one ten-thousandth of an inch to a side contains somewhere between sixteen and a hundred and thirty-one billions of molecules. Again, as touching the supposed power of multiplication on the part of his gemmules, Mr. Darwin alludes to the fact that infectious material of all kinds exhibits a ratio of increase quite as great as any that his theory requires to attribute to gemmules. Furthermore, with respect to the elective affinity of gemmules, he remarks that "in all ordinary cases of sexual reproduction the male and female elements certainly have an elective affinity for each other;" of the ten thousand species of Compositæ, for example, "there can be no doubt that if the pollen of all these species could be simultaneously placed on the stigma of any one species, this one would elect, with unerring certainty, its own pollen."

Such then in brief outline, is Mr. Darwin's theory of pangenesis.

Professor Weismann's theory of germ-plasm is fundamentally based upon the great distinction that obtains in respect of their transmissibility between characters which are congenital and characters which are acquired. By a congenital character is meant any individual peculiarity, whether structural or mental, with which the individual is born. By an acquired character is meant any peculiarity which the individual may subsequently develop in consequence of its own individual experience. For example, a man may be born with some malformation of one of his fingers or he may subsequently acquire such a malformation as the result of accident or disease. Now in the former case—*i. e.*, in that where the malformation is congenital—it is extremely probable that the peculiarity will be transmitted to his children; while in the latter case—*i. e.*, where the malformation is subsequently acquired—it is virtually certain that it will not be transmitted to his children. And this great difference between the transmissibility of characters which are congenital and characters which are acquired extends universally as a general law throughout the vegetable as well as the animal kingdom, and in the province of mental as in that of bodily organization. Of course this general law has always been well known and more or less fully recognized by all modern physiologists and medical men. But before the subject was taken up by Professor Weismann it was generally assumed that the difference in question was one of degree, not one of kind. In other words, it was assumed that acquired characters, although not so fully—and therefore not so certainly—inherited as congenital characters, nevertheless were inherited in some lesser degree; so that, if the same character continued to be developed successively in a number of sequent generations, what was at first only a slight tendency to be inherited would become by

summation a more and more pronounced tendency, till eventually the acquired character might be as strongly inherited as any other character which was *ab initio* congenital. Now it is the validity of this assumption that is challenged by Professor Weismann. He says there is no evidence at all of any acquired characters being in any degree inherited, and therefore that in this important respect they may be held to differ from congenital characters in kind. On the supposition that they do thus differ in kind, he furnishes a very attractive theory of heredity, which serves at once to explain the difference, and to represent it as a matter of physiological impossibility that any acquired character can, under any circumstances whatsoever, be transmitted to progeny.

In order fully to comprehend this theory, it is desirable first of all to explain Professor Weismann's views upon certain other topics which are more or less closely allied to, and indeed logically bound up with the present one.

Starting from the fact that uni-cellular organisms multiply by fission and gemmation, he argues that aboriginally and potentially, life is immortal; for, when a protozoön divides into two—more or less equal parts by fission, and each of the two halves thereupon grows into another protozoön, it is evident that there has been no death on the part of any of the living material involved; and inasmuch as this process of fission goes on continuously from generation to generation, there is never any death on the part of such protoplasmic material, although there is a continuous addition to it as the numbers of individuals increase. Similarly, in the case of gemmation, when a protozoön parts with a small portion of its living material in the form of a bud, this portion does not die, but develops into a new individual; and therefore the process is exactly analogous to that of fission, save that only a small instead of a large part of the parent substance is involved. Now if life be thus immortal in the case of uni-cellular organisms, why should it have ceased to be so in the case of multi-cellular organisms? Weismann's answer is that all the multi-cellular organisms propagate themselves, not exclusively by fission or gemmation, but by sexual fertilization, where the condition to a new organism arising is—that minute and specialized portions of two parent organisms should fuse together. Now it is evident that with this change in the method of propagation, serious disadvantage would accrue to any species if its sexual individuals were to continue to be immortal; for in that case every species which multiplies by sexual methods would in time become composed of indivuals broken down and decrepit through the results of accident and disease—always operating and ever accumulating throughout the course of their immortal lives. Consequently as soon as sexual methods of propagation superseded the more primitive a-sexual methods, it became desirable in the interests of the sexually-propagating species that their constituent individuals should cease to be immortal, so that

the species should always be recuperated by fresh, young, and well-formed representatives. Consequently also, natural selection would speedily see to it that all sexually-propagating species should become deprived of the aboriginal endowment of immortality, with the result that death is now a universal destiny among all the individuals of such species, that is to say, among all the metazoa and metaphyta. Nevertheless, it is to be remembered that this destiny extends only to the parts of the individual other than the contents of those specialized cells which constitute the reproductive elements, for although in each individual metazoön or metaphyton an innumerable number of these specialized cells are destined to perish during the life and with the death of the organism to which they belong, this is only due to the accident, so to speak, of their contents not having met with their complements in the opposite sex; it does not belong to their essential nature that they should perish, seeing that those which do happen to meet with their complements in the opposite sex help to form a new living individual, and so on through successive generations *ad infinitum.* Therefore the reproductive elements of the metazoa and metaphyta are in this respect precisely analagous to the protozoa: potentially, or in their own nature, they are immortal; and, like the protozoa, if they die, their death is an accident due to unfavorable circumstances. But the case is quite different with all the other parts of a multicellular organism. Here, no matter how favorable the circumstances may be, every cell contains within itself, or in its very nature, the eventual doom of death. Thus, of the metazoa and mtaphyta it is the specialized germ-plasms alone that retain their primitive endowment of everlasting life, passed on continuously through generation after generation of successively perishing organisms.

So far, it is contended, we are dealing with matters of fact. It must be taken as true that the protoplasm of the uni-cellular organisms and the germ-plasm of the multicellular organisms have been continuous through the time since life first appeared upon this earth; and although large quantities of each are perpetually dying through being exposed to conditions unfavorable to life, this, as Weismann presents the matter, is quite a different case from that of all the other constituent parts of multi-cellular organisms, which contain within themselves the doom of death. Furthermore, it appears extremely probable that this doom of death has been brought about by natural selection for the reasons assigned by Weismann, namely, because it is for the benefit of all species which perpetuate themselves by sexual methods that their constituent individuals should not live longer than is necessary for the sake of originating the next generation and fairly starting it in its own struggle for existence. For Weismann has shown, by a somewhat laborious though still largely imperfect research, that there is throughout all the metazoa a general correlation between the natural life-time of individuals composing any given species and the age at which they

reach maturity or first become capable of procreation. This general correlation however is somewhat modified by the time during which progeny are dependent upon their parents for support and protection. Nevertheless, it is evident that this modification tends rather to confirm the view that expectation of life on the part of individuals has in all cases been determined with strict reference to the requirements of propagation, if under propagation we include the rearing as well as the production of offspring. I may observe in passing that I do not think this general law can be found to apply to plants in nearly so close a manner as Weismann has shown it to apply to animals; but leaving this fact aside, to the best of my judgment it does appear that Weismann has made out a good case in favor of such a general law with regard to animals.

We have come then to these results. Protoplasm was originally immortal (barring accidents), and it still continues to be immortal in the case of unicellular organisms which propagate a-sexually. But in the case of all multicellular organisms, which propagate sexually, natural selection has reduced the term of life within the smallest limits that in each given case are compatible with the performance of the sexual act and the subsequent rearing of progeny, reserving however the original endowment of immortality for the germinal elements, whereby a *continuum* of life has been secured from the earliest appearance of life until the present day.

Now in view of these results, the question arises, Why should the sexual methods of propagation have become so general if their effect has been that of determining the necessary death of all individuals presenting them? Why, in the course of organic evolution, should these newer methods have been imposed on all the higher organisms, when the consequence is that all these higher organisms must pay for the innovation with their lives? Weismann's answer to this question is as interesting and ingenious as all that has gone before. Seeing that sexual propagation is so general as to be practically universal among multi-cellular organisms, it is obvious that in some way or other it must have a most important part to play in the general scheme of organic evolution. What then is the part that it does play? What is its *raison d'être?* Briefly, according to Weismann, its function is that of furnishing congenital variations to the ever-watchful agency of natural selection, in order that natural selection may always preserve the most favorable and pass them on to the next generation by heredity. That sexual propagation is well calculated to furnish congenital variations may easily be rendered apparent. We have only to remember that at each union there is a mixture of two germinal elements; that each of these was in turn the product of two other germinal elements in the preceding generation, and so backwards *ad infinitum* in geometrical ratio. Remembering this, it follows that the germinal element of no one member of a species can ever be the same as that of any

other member; on the contrary, while both are enormously complex products, each has had a different ancestral history, such that while one presents the congenital admixtures of thousands of individuals in one line of descent, the other presents similar admixtures of thousands of other individuals in a different line of descent. Consequently, when in any sexual union two of these enormously complex germinal elements fuse together and constitute a new individual out of their joint endowments, it is perfectly certain that that individual can not be exactly like any other individual of the same species or even of the same brood; the chances must be infinity to one against any single mass of germ-plasm being exactly like any other mass of germ-plasm; while any amount of latitude as to difference is allowed, up to the point at which the difference becomes too pronounced to satisfy the conditions of fertilization, in which case, of course, no new individual is born. Hence, theoretically, we have here a sufficient cause for all individual variations of a congenital kind that can possibly occur within the limits of fertility, and therefore that can ever become actual in living organisms. In point of fact, Weismann believes—or at any rate began by believing—that this is the sole and only cause of variations that are congenital, and therefore (according to his views) transmissible by heredity. Now whether or not he is right as regards these latter points, I think there can be no question that sexual propagation is, at all events, one of the main causes of congenital variation; and seeing of what enormous importance congenital variation must always have been in supplying material for the operation of natural selection, we appear to have found a most satisfactory answer to our question,—Why has sexual propagation become so universal among all the higher plants and animals? It has become so because it is thus shown to have been the condition to producing congenital variations, which in turn constitute the condition to the working of natural selection.

Having got thus far, I should like to make two or three subsidiary remarks. In the first place it ought to be observed that this luminous theory touching the causes of congenital variations was not originally propounded by Professor Weismann, but occurs in the writings of several previous authors and is expressly alluded to by Darwin. Nevertheless, it occupies so prominent a place in Weismann's system of theories and has by him been wrought up so much more elaborately than by any of his predecessors that we are entitled to regard it as *par excellence* the Weismannian theory of variation. In the next place it ought to be observed that Weismann is careful to guard against the seductive fallacy of attributing the origin of sexual propagation to the agency of natural selection. Great as the benefit of this newer mode of propagation must have been to the species presenting it, the benefit can not have been conferred by natural selection, seeing that the benefit arose from the fact of the new method furnishing material to the operation of natural selection, and therefore insofar as it did this,

constituting the condition to the principle of natural selection having been called into play at all. Or in other words, we can not attribute to natural selection the origin of sexual reproduction without involving ourselves in the absurdity of supposing natural selection to have originated the conditions of its own activity.* What the causes may have been which originally led to sexual reproduction is at present a matter that awaits suggestion by way of hypothesis; and therefore it now only remains to add that the general structure of Professor Weismann's system of hypotheses leads to this curious result, namely, that the otherwise ubiquitous and (as he supposes) exclusive dominion of natural selection stops short at the protozoa, over which it can not exercise any influence at all. For if natural selection depends for its activity on the occurrence of congenital variations, and if congenital variations depend for their occurrence on sexual modes of reproduction, it follows that no organisms which propagate themselves by any other modes can present congenital variations, or thus become subject to the influence of natural selection. And inasmuch as Weismann believes that such is the case with all the protozoa, as well as with all parthenogenetic organisms, he does not hesitate to accept the necessary conclusion that in these cases natural selection is without any jurisdiction. How, then, does he account for individual variations in the protozoa? And still more, how does he account for the origin of their innumerable species? He accounts for both these things by the direct action of external con-

* Since this paper was sent to press, Professor Weismann has published in *Nature* (February 6, 1890: vol. XLI, pp. 317–323) an elaborate answer to a criticism of his theory by Professor Vines (October 24, 1889: vol. XL, pp. 621–626). In the course of this answer Professor Weismann says that he *does* attribute the origin of sexual reproduction to natural selection. This directly contradicts what he says in his essays, and for the reasons given in the text, appears to me an illogical departure from his previously logical attitude. I herewith append quotations in order to reveal the contradiction:

"But when I maintain that the meaning of sexual reproduction is to render possible the transformation of the higher organisms by means of natural selection, such a statement is not equivalent to the assertion that sexual reproduction originally came into existence in order to achieve this end. The effects which are now produced by sexual reproduction did not constitute the causes which led to its first appearance. Sexual reproduction came into existence before it could lead to hereditary individual variability (*i. e.*, to the possibility of natural selection). Its first appearance must, therefore, have had some other cause [than natural selection]; but the nature of this cause can hardly be determined with any degree of certainty or precision from the facts with which we are at present acquainted."—("Essay on the Significance of Sexual Reproduction in the Theory of Natural Selection: English Translation," pp. 281–282.)

"I am still of opinion that the origin of sexual reproduction depends on the advantage which it affords to the operation of natural selection. - - - Sexual reproduction has arisen by and for natural selection as the sole means by which individual variations can be united and combined in every possible proportion."—(*Nature*, Vol. XLI, p. 322.)

How such opposite statements can be reconciled I do not myself perceive.—G. J. R., February 17, 1890.

ditions of life. In other words, so far as the uni-cellular organisms are concerned, Weismann is rigidly and exclusively an advocate of the theory of Lamarck, just as much as in the case of all the multi-cellular organisms he is rigidly and exclusively an opponent of that theory. Nevertheless, there is here no inconsistency; on the contrary, it is consistency with the logical requirements of his theory that leads to this sharp partitioning of the uni-cellular from the multi-cellular organisms with respect to the causes of their evolution. For, as he points out, the conditions of propagation among the uni-cellular organisms are such that parent and offspring are one and the same thing; "the child is a part, and usually a half, of its parent." Therefore, if the parent has been in any way modified by the action of external conditions, it is inevitable that the child should, from the moment of its birth (*i. e.*, fissiparous separation), be similarly modified; and if the modifying influences continue in the same lines for a sufficient length of time the resulting change of type may become sufficiently pronounced to constitute a new species, genus, etc. But in the case of the multi-cellular or sexual organisms the child is not thus merely a severed moiety of its parent; it is the result of the fusion of two highly specialized and extremely minute particles of each of two parents. Therefore, whatever may be thought touching the validity of Weismann's deduction that in no case can any modification induced by external conditions on these parents be transmitted to their progeny, at least we must recognize the validity of the distinction which he draws between the facility with which such transmission must take place in the uni-cellular organisms as compared with the difficulty—or, as he believes, the impossibility—of its doing so in the multi-cellular.

We are now in a position to fully understand Professor Weismann's theory of heredity in all its bearings. Briefly stated, this theory is as follows: The whole organization of any multi-cellular organism is composed of two entirely different kinds of cells, namely, the germ cells, or those which have to do with reproduction, and the somatic cells, or those which go to constitute all the other parts of the organism. Now the somatic cells in their aggregations as tissues and organs may be modified in numberless ways by the direct action of the environment as well as by special habits formed during the individual life-time of the organism. But although the modifications thus induced may be and generally are adaptive,—such as the increased muscularity caused by the use of muscles, "practice making perfect" in the case of nervous adjustments, and so on,—in no case can these so-called acquired or "somato-genetic" characters exercise any influence upon the germ-cells, such that they should re-appear in their products (progeny) as congenital or "blasto-genetic" characters. For according to the theory, the germ-cells as to their germinal contents differ in kind from the somatic cells, and have no other connection or dependence upon them than that of deriving from them their food and lodging. So much then for

the somatic cells. Turning now more especially to the germ-cells, these are the receptacles of what Weissmann calls the germ-plasm; and this it is that which he supposes to differ in kind from all the other constituent elements of the organism. For the germ-plasm he believes to have had its origin in the uni cellular organisms, and to have been handed down from them in one continuous stream through all successive generations of multi-cellular organisms. Thus, for example, suppose we take a certain *quantum* of germ-plasm as this occurs in any individual organism of to-day. A minute portion of this germ-plasm, when mixed with a similarly minute portion from another individual, goes to form a new individual. But in doing so only a portion of this minute portion is consumed; the residue is stored up in the germinal cells of this new individual in order to secure that continuity of the germ-plasm which Weismann assumes as the necessary basis of his whole theory. Furthermore, he assumes that this overplus portion of germ-plasm which is so handed over to the custody of the new individual is there capable of growth or multiplication at the expense of the nutrient materials which are supplied to it by the new *soma* in which it finds itself located; while in thus growing or multiplying it faithfully retains its highly complex character, so that in no one minute particular does any part of a many thousand-fold increase differ as to its ancestral characters from that inconceivably small overplus which was first of all intrusted to the embryo by its parents. Therefore one might represent the germ-plasm by the metaphor of a yeast-plant, a single particle of which may be put into a vat of nutrient fluid; there it lives and grows upon the nutriment supplied, so that a new particle may next be taken to impregnate another vat, and so on *ad infinitum*. Here the successive vats would represent successive generations of progeny; but to make the metaphor complete one would require to suppose that in each case the yeast-cell was required to begin by making its own vat of nutrient material, and that it was only the residual portion of the cell which was afterwards able to grow and multiply. But although the metaphor is necessarily a clumsy one, it may serve to emphasize the all-important feature of Weismann's theory, viz., the almost absolute independence of the germ-plasm. For just as the properties of the yeast-plant would be in no way affected by anything that might happen to the vat short of its being broken up or having its malt impaired, so according to Weismann the properties of the germ-plasm cannot be affected by anything that may happen to its containing *soma* short of the *soma* being destroyed or having its nutritive functions impaired.

Such being the relations that are supposed to obtain between the *soma* and its germ plasm, we have next to contemplate what is supposed to happen when, in the course of evolution, some modification of the ancestral form of the *soma* is required in order to adapt it to some change on the part of its environment. In other words, we have to consider Weismann's views on the *modus operandi* of adaptive development, with its results in the origination of new species.

Seeing that according to the theory, it is only congenital variations which can be inherited, all variations subsequently acquired by the intercourse of individuals with their environment, however beneficial such variations may be to these individuals, are ruled out as regards the species. Not falling within the province of heredity, they are blocked off in the first generation, and therefore present no significance at all in the process of organic evolution. No matter how many generations of eagles, for instance, may use their wings for purposes of flight; and no matter how great an increase of muscularity, of endurance, and of skill, may thus be secured to each generation of eagles as the result of individual exercise; all these advantages are entirely lost to progeny, and young eagles have ever to begin their lives with no more benefit bequeathed by the activity of their ancestors than if those ancestors had all been barn-door fowls. Therefore the only material which is of any count as regards the species, or with reference to the process of evolution, are fortuitous variations of the congenital kind. Among all the numberless congenital variations, within narrow limits, which are perpetually occurring in each generation of eagles, some will have reference to the wings; and although these will be fortuitous, or occurring indiscriminately in all directions, a few of them will now and then be in the direction of increased muscularity, others in the direction of increased endurance, others in the direction of increased skill, and so on. Now each of these fortuitous variations, which happens also to be a beneficial variation, will be favored by natural selection; and because it likewise happens to be a congenital variation, will be perpetuated by heredity. In the course of time, other congenital variations will happen to arise in the same directions; these will be added by natural selection to the advantage already gained, and so on, till after hundreds and thousands of generations the wings of eagles become evolved into the marvelous structures which they now present.

Such being the theory of natural selection when stripped of all remnants of so-called Lamarckian principles, we have next to consider what the theory means in its relation to germ-plasm. For as before explained, congenital variations are supposed by Weismann to be due to new combinations taking place in the germ-plasm as a result of the union of two complex hereditary histories in every act of fertilization. Well, if congenital variations are thus nothing more than variations of germ-plasm "writ large" in the organism which is developed out of the plasm, it follows that natural selection is really at work upon these variations of the germ-plasm. For although it is proximately at work on the congenital variations of organisms after birth, it is ultimately, and through them, at work upon the variations of germ-plasm out of which the organisms arise. In other words, natural selection in picking out of each generation those individual organisms which are by their congenital character best suited to their surrounding conditions of life, is thereby picking out those peculiar combinations or variations

of germ-plasm, which, when expanded into a resulting organism, give that organism the best chance in its struggle for existence. And inasmuch as a certain overplus of this peculiar combination of germ-plasm is intrusted to that organism for bequeathing to the next generation, this to the next, and so on, it follows that natural selection is all the while conserving that originally peculiar combination of germ-plasm, until it happens to meet with some other mass of germ-plasm by mixing with which it may still further improve upon its original peculiarity when, of course, natural selection will seize upon this improvement to perpetuate as in the previous case. So that on the whole we may say that natural selection is ever waiting and watching for such combinations of germ-plasm as will give the resulting organisms the best possible chance in their struggle for existence; while at the same time it is remorselessly destroying all those combinations of germ-plasm which are handed over to the custody of organisms not so well fitted to their conditions of life.

It only remains to add that, according to Weismann's theory in its strictly logical form, combinations of germ-plasm when once effected are so stable that they would never alter except as a result of entering into new combinations. In other words, no external influences or internal processes can ever change the hereditary nature of any particular mixture of germ-plasm, save and except its admixture with some other germ-plasm, which, being of a nature equally stable, goes to unite with the other in equal proportions as regards hereditary character. So that really it would be more correct to say that any given mass of germ-plasm does not change even when it is mixed with some other mass—any more, for instance, than a handful of sand can be said to change when it is mixed with a handful of clay.

Consequently, we arrive at this curious result. No matter how many generations of organisms there may have been, and therefore no matter how many combinations of germ-plasm may have taken place to give rise to an existing population, each existing unit of germ-plasm must have remained of the same essential nature of constitution as when it was first started in its immortal career millions of years ago. Or reverting to our illustration of sand and clay, the particles of each must always remain the same, no matter how many admixtures they may undergo with particles of other materials, such as chalk, slate, etc. Now inasmuch as it is an essential—because a logically necessary—part of Weismann's theory to assume such absolute stability or unchangeableness on the part of germ-plasm, the question arises, and has to be met,—What was the origin of those differences of character in the different germ-plasms of multi-cellular organisms which first gave rise, and still continue to give rise, to congenital variations by their mixture one with another? This important question Weismann answers by supposing that these differences originally arose out of the differences in the uni-cellular organisms, which were the ancestors of the primitive

multi-cellular organisms. Now as before stated, different forms of uni-cellular organisms are supposed to have originated as so many results of differences in the direct action of the environment. Consequently, according to the theory, all congenital variations which now occur in multi-cellular organisms are really the distant results of variations that were aboriginally induced in their uni-cellular ancestors by the direct action of surrounding conditions of life.

I think it will be well to conclude by briefly summarizing the main features of this elaborate theory.

Living material is essentially, or of its own nature, imperishable, and it still continues to be so in the case of unicellular organisms which propagate by fission or gemmation. But as soon as these primitive methods of propagation became, from whatever cause, superseded by sexual, it ceased to be for the benefit of species that their constituent individuals should be immortal, seeing that, if they continued to be so, all species of sexually-reproducing organisms would sooner or later come to be composed of broken-down and decrepit individuals. Consequently, in all sexually-reproducing or multi-cellular organisms, natural selection set to work to reduce the term of individual life-times within the narrowest limits that in the case of each species are compatible with the procreation and the rearing of progeny. Nevertheless, in all these sexually-reproducing organisms the primitive endowment of immortality has been retained with respect to their germ-plasm, which has thus been continuous, through numberless generations of perishing organisms, from the first origin of sexual reproduction till the present time. Now it is the union of germ-plasms which is required to reproduce new individuals of multi-cellular organisms that determines congenital variations on the part of such organisms, and thus furnishes natural selection with the material for its work in the way of organic evolution,—work therefore which is impossible in the case of uni-cellular organisms, where variation can never be congenital, but always determined by the direct action of surrounding conditions of life. Again, as the germ-plasm of multi-cellular organisms is continuous from generation to generation, and at each impregnation gives rise to a more or less novel set of congenital characters which are of most service to the organisms presenting them, is really or fundamentally at work upon those variations of the germ-plasm which in turn give origin to those variations of organisms that we recognize as congenital, therefore, natural selection has always to wait and to watch for such variations of germ-plasm as will eventually prove beneficial to the individuals developed therefrom, who will then transmit this peculiar quality of germ-plasm to their progeny, and so on. Therefore also—and this is most important to remember—natural selection as thus working becomes the one and only cause of evolution and the origin of species in all the multi-cellular organisms, just as the direct action of the environment is the one and only cause of evolution and the origin of species

in the case of all the uni-cellular organisms. But inasmuch as the multi-cellular organisms were all in the first instance derived from the uni-cellular and inasmuch as their germ-plasm is of so stable a nature that it can never be altered by any agencies internal or external to the organisms presenting it, it follows that all congenital variations are the remote consequences of aboriginal differences on the part of uni-cellular ancestors. And lastly, it follows also that these congenital variations—although now so entirely independent of external conditions of life, and even of activities internal to organisms themselves—were originally and exclusively due to the direct action of such conditions on the lives of their unicellular ancestry; while even at the present day no one congenital variation can arise which is not ultimately due to differences impressed upon the protoplasmic substance of the germinal elements, when the parts of which these are now composed constituted integral parts of the protozoa, which were directly and differentially affected by their converse with their several environments.

Such then is Weismann's theory of heredity in its original and strictly logical form. But it is now necessary to add that in almost every one of its essential features, as just stated, the theory has had to undergo—or is demonstrably destined to undergo—some radical modification. On the present occasion however, my object is merely to state the theory, not to criticise it. Therefore I have sought to present the whole theory in its completely connected shape. On a future occasion—I hope within the present year—it will be my endeavor to disconnect the now untenable parts from the parts which still remain for investigation at the hands of biological science.

THE ASCENT OF MAN.*

By Frank Baker, M. D.

The science of Anthropology, one of the younger daughters of human knowledge, is so vast in its scope that to master all of its different ramifications seems a hopeless task. Having for its object the comprehensive study of man, including his origin, his development, and his present condition, its aim is to focus and co-ordinate the general results derived from a vast number of subordinate branches. The philologist contributes information concerning the origin and growth of language and its effect upon civilization; the mythologist tells of the psychological side of the human mind and traces the rise and progress of religious ideas; the archæologist, in order to fix their places in the history of mankind, searches for the remains of peoples long since passed away. All these depend for their material upon external records, left by tradition, by writing, by sculpture, or by implements and weapons. With greatest care every ancient habitation of man is searched in order to learn from it the details of the life of its former inhabitants.

Within comparatively recent times still another avenue of information has been found, for we have learned that it is not alone by these external records that man's history can be traced, but that important facts may be obtained by studying the constitution of his body; that the changes and vicissitudes of his existence are recorded on his very bones, in characters long undeciphered, but to which the clew has at last been found. My labors have led me more particularly to this department of anthropology, and a concise summary of the main heads of this research may be of value and interest.

The views propounded by Lamarck in the early part of this century, with reference to the modification of living organisms by use and adaptation, have been remarkably confirmed in modern times. Exhaustive researches into the constitution and properties of the cells composing living tissues show that they are subject to continual change, each impulse from without being registered by some small alteration in their physical condition. Impulses of a similar kind continuously acting

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produce greater changes, and long-continued repetition notably alters even the hardest and most enduring of structures. Thus it is that bones are modified in form by muscular pull and the surfaces of teeth are shaped by incessant grinding. These alterations are more readily apparent to us because they affect very hard and easily preserved organs, but the effects are equally potent, though not so clearly recognizable, in the softer tissues of the body. Every act of our lives is certainly but surely registered within the marvellous structure of our bodies. Not a muscle can contract without an absolute change substance; in its not a nerve-cell can discharge with out some self-destruction.

Most of these changes being very minute and evanescent are quite beyond our power to accurately estimate, and were the increments of change confined to a single life-time, were each individual to stand only for himself and compelled to earn his experience by the same tedious struggle, use and adaptation would have but little power to mold mankind into races and varieties. But, by the action of a law as yet imperfectly understood, the adaptations of each individual are transmitted to its offspring; or, to speak more accurately, the offspring pass through the changes more easily and quickly than the parent did. While each has always to go back to the beginning and commence from the simple blastema of the primitive egg, the younger has the advantage of being able to adapt itself more quickly to its surroundings, provided these have not too greatly changed, and thus starts a little way ahead of its ancestor in the race for life. In consequence of this law, changes become cumulative, and a cause acting for a great length of time upon a series of successive generations finally produces a well-marked and easily observed effect in the structure of individuals; changing colors, modifying organs, shaping whole regions of the body.

Again, if after such changes have been effected, these causes cease to operate and the organs they have shaped are no longer of use, the latter become reduced in size, atrophy, and recede, remaining however in a vestigial condition for many, many generations as records of the past history of the race, as dolmens and cromlechs certify to former customs and flint arrow-heads and stone hatchets give evidence of a previous state of civilization.

The human body abounds in testimony of this sort,—indications of the pathway by which humanity has climbed from darkness to light, from bestiality to civilization,—relics of countless ages of struggle, often fierce, bloody, and pitiless.

These are found in every organ of the body, and each new investigation adds to their number. To enumerate them all would be impossible within the limits assigned me by your patience. I will therefore touch only upon a few of the more striking ones, especially those connected with the modifications of the limbs, with the erect position, and with the segmentation of the body.

The limbs, being organs of support and locomotion, show great variations in the zoölogical series, and the hand of man has long been looked upon as especially significant of his high position in the animal kingdom, one of the chief distinctions between him and the nearest brutes. To a certain extent this is correct. No other creature possesses so highly complex and effective an organ for grasping and adjusting objects, and it is pre-eminently this that has made man a tool-using animal. On comparing a human hand with that of the anthropoid apes it may be seen that this efficiency is produced in two ways: first, by increasing the mobility and variety of action of the thumb and fingers; second, by reducing the muscles used mainly to assist prolonged grasp, they being no longer necessary to an organ that is intended for delicate work, and requires constant re-adjustment. Thus some elements are added and some taken away. Now according to the theory I have enunciated, the latest elements ought to show signs of their recent origin, to be somewhat imperfectly differentiated and liable to return to their primitive state, while those going out of active use ought to be vestigial, not equal in size or force to muscular organs generally, very liable to variation or disappearance. This is what actually occurs.

Among the new elements is a special flexor muscle for the thumb, arising high up on the forearm. A very slight examination shows that this muscle has been split off from the fibers of the deep flexor that bends the terminal joints of the fingers. In most apes the two form a single muscle, and in man the thumb flexor very often shows unmistakable evidence of such origin. In about 10 per cent. of persons, part of its fibers pass over to and become blended with the parent muscle. Not infrequently I have seen the two entirely united, returning absolutely to their primitive condition. The deep and superficial flexors of the fingers show signs of a similar relationship, as they frequently blend more or less, tending to revert to the type shown in most lower animals. Indeed, if we go back to embryonic life we find all the muscles of the anterior part of the fore-arm united in what is termed the *pronato flexor mass*, recalling the original condition of musculature in the earliest animals possessing limbs.

In the category of disappearing muscles comes the *palmarus longus* a muscle of the fore-arm which in many animals is an important aid in climbing and grasping. It takes its origin from the upper arm and passes to the hand, where it expands into a large sheet of thick membrane called the palmar fascia, which splits into several slips passing to each finger. The pull of the muscle acts upon all the fingers together, keeping them bent without independence of action. Now in man the fingers have each two separate flexor tendons that can act to a certain extent independently. To insure their independence they are at the wrist enclosed in a remarkable tubular conduit or subway formed by soldering the palmar fascia to the wrist-bones. This at once destroys any effective action of the *palmaris longus* on the fingers and it becomes

a flexor of the wrist. This soldering undoubtedly took place because the muscle was no longer required as a finger-holder. Like other organs that after playing a part of considerable functional importance have come from change of habit to be of but little value, it shows the most astonishing tendency to variation. Not a week passes in a large dissecting room that some curious anomaly is not found in this muscle. Sometimes it is seen almost in its primitive condition, the palmar fascia being comparatively movable and the *palmaris longus* having some effect upon the flexion of the fingers; oftener it unites wholly or partially with some portion of the pronato-flexor mass or disappears altogether. The disappearance is usually only apparent, however. Regressive structures rarely disappear totally, for on careful search a strip of fascia can usually be found that represents the atrophied and aborted organ.

Since these two examples differ in that the first represents the development of a new muscle while the second is the atrophy of an old one, we ought to find racial differences corresponding to these two conditions. Our studies of racial anatomy are as yet far from sufficient to give us complete information upon these points, and I would especially avoid generalizing upon too meager data. It has however appeared to me that in negroes the *palmaris longus* is more inclined to assume its primitive type—that is, is less likely to vary—while the long flexor of the thumb is on the contrary more inclined to be partially, if not wholly, united with the deep flexor of the fingers.

Connected intimately with the hand are the other portions of the thoracic limb that carry it from place to place. Here again we may note many points indicating a progressive development of the member. When the arm is naturally and easily bent at the elbow it does not carry the hand to the shoulder, as might be expected, but towards the mouth. The reason for this is that the articular surfaces of the elbow-joint are not cut horizontally across the axis of the humerus, but inclined at an angle of about 20°. This obliquity does not occur in the fœtus and is less in Bushmen, Australians, and the anthropoid apes. It is associated with another peculiarity; indeed, may be said to be caused by it. This is a twisting of the humerus on its long axis, which occurs markedly in the higher races. If we hold up endwise the humerus of a European we see that the longest diameters of the upper and lower ends very nearly coincide. In the negro we find the lower diameter turned more towards the body, still more in the anthropoid apes, and again more as we descend the scale. Embryology teaches that the humerus was formerly set so that the hollow of the elbow looked towards the body rather than forward, and it seems therefore that as the functions of the limb became more various, the lower end of the bone gradually twisted outward around the long axis until its diameter described a considerable arc. This turned the hand with the palm to the front, extended its range, and adapted it for a wider usefulness. Greater twist is found in the right humerus than in the left and in the humeri of

modern times than in those of the stone age. As the torsion increased some provision became necessary for carrying the hand easily across the body to the mouth. This was effected by the inclination of the articular surfaces of the elbow-joint already mentioned.

Many movements of the arm in man are produced by muscles acting upon the shoulder-blade or scapula. As the hand was turned outward and a wider range given, these increased in extent and importance, and the scapula accordingly widened out at its vertebral border in order to give a more extensive attachment for muscles. In order to accurately estimate this change the ratio of the breadth to the length of the scapula is taken. This ratio, called the scapular index, is highest among the white races, less in the infant, in negroes, and in Australians, and still less in anthropoid apes. It is significant also that the vertebral border of the scapula is the last to form in the fœtus. We have therefore three modifications—the torsion of the humerus, the inclination of its lower articular surface, and the scapular index—all depending upon each other, all varying together *pari passu*, and all showing a progressive development both in the individual and the race.

Muscle is composed of one of the most highly organized and expensive tissues of the body. Unless fed constantly with a great supply of blood to keep up its active metabolic changes, it quickly wastes, functional activity being absolutely necessary to its proper maintenance, as any one knows who has seen how rapidly the muscles of an athlete diminish when he goes out of training. If from accident or change of habit its use altogether ceases, its protoplasm is gradually removed, its blood supply diminishes, and it shrinks to a mere band or sheet of fibrous tissue. Changes of function may therefore affect the form of muscles, one portion becoming tendinous or fascia-like; may even cause them to shift their places, by inducing a development on one side and an atrophy on another, or to disappear altogether, being replaced by fascia or ligament. A similar regression may take place in bone and cartilage a high-grade, actively metabolic tissue, difficult to maintain, being replaced by a low-grade one comparatively slow to change. It is therefore not unusual to find that muscles, bones, and cartilages performing important functions in some animals are represented by vestigial structures in those higher in the scale. Our conclusions on this subject are confirmed by finding occasional instances where the hereditary tendency has been greater than usual and the parent form is re-produced more or less completely in the higher animal. The palmar fascia at the distal end of the *palmaris longus*, to which allusion has been made, represents a former muscular portion, relics of which probably remain as some of the small thumb muscles.

Another interesting instance is the *epitrochleo-anconeus*, a small muscle at the elbow joint, used in apes to effect a lateral movement of the ulna upon the humerus. In man the ulna has become so shaped that the lateral movement is almost wholly lost, and the muscle has

accordingly degenerated, being represented by a strip of fascia. Very often however, a few muscular fibers are still found in this situation.

Several minor peculiarities that remind us of primitive conditions occur in the region of the humerus. Occasionally a *supracondyloid process* is found, throwing a protecting arch over the brachial artery and median nerve; in this resembling the *supracondyloid foramen* of marsupials. Struthers found this to be hereditary, occurring in a father and four children. A perforation of the *olecranon fossa*, the pit at the lower end of the humerus into which the beak-like end of the ulna fits when the arm is fully extended, may probably be regarded as a reversion toward the condition of anthropoid apes. This frequently occurs in South African and other low tribes and in the men of the stone age. Recently Dr. D. S. Lamb has found it remarkably frequent in pre-historic Indian humeri from the Salado Valley, Arizona.

While the region of the hand and fore-arm indicates increase of specialization, the upper part of the limb generally testifies to a regression from a former more highly developed state. The anatomy of the flying apparatus of a bird shows a series of muscular, ligamentous, and bony structures connected with its upper arm far beyond anything ever seen in man. The coracoid bone, a very important element of the shoulder girdle in birds, has become reduced in man to a little vestigial ossicle that about the sixteenth year becomes soldered to the scapula as the coracoid process. The muscles arising from this,—*pectoralis minor*, *coraco-brachialis*, and *biceps*,—are structures represented in birds by strong, flying muscles. The *subclavius*, a little slip ending at the clavicle, appears to have formerly passed to the coracoid bone or to the humerus and been employed in arm movement. The *pectoralis major* appears to represent what was formerly a series of muscles. All these have a tendency to repeat their past history, and the number of variations found among them is legion. The *biceps* show traces of its former complexity by appearing with three, four, or even five heads, by a great variety of insertions, by sending a tendon outside the joint capsule instead of through it, as is the rule. The *pectoralis major* may break up into several different muscular integers, inserted from the shoulder capsule down to the elbow. The *coraco-brachialis* shows the same instability, and by its behavior clearly indicates its derivation from a much larger and more extensive muscular sheet.

Not less significant are the ligaments about the shoulder. Many of these appear to be relics of organs found active in animals lower in the scale. Thus the *coraco-acromial* ligament spanning over the shoulder joint is probably a former extension of the acromion process; the *rhomboid*, *conoid*, *trapezoid*, and *gleno-humeral* ligaments represent regressive changes in the *subclavius* muscle, the *coraco-humeral* ligament, a former insertion of the *pectoralis minor*. Bands of the deep cervical fascia alone remain to testify to the former existence of the *levator clav-*

icular, a muscle present in most mammals and used to pull forward the shoulder girdle when walking in a quadrupedal position. In negroes I have frequently found it more or less complete. A fibrous strip uniting the *latissimus dorsi* to the *triceps* is all that remains of an important muscle, the *dorso-epitrochlearis*, passing from the back to the elbow or forearm, used by gibbons and other arboreal apes in swinging from branch to branch. Testut found this fully developed in a Bushman. I have myself seen various muscular slips that must represent some portions of it, and authors generally describe it as occurring in 5 or 6 per cent. of individuals.

The hind limbs of apes are popularly thought to be remarkably specialized. The term quadrumana or four-handed is used to characterize the class; yet it is quite true that this term involves a false conception. No animal has four exactly similar feet, still less four hands. The feet of the ape differ widely from hands; the great toe is not really opposable like the thumb, but merely separable from the others and differently set, so as to afford a grasp like that of a cramp iron. The gibbon alone has a small muscle of the foot that may be compared with the *opponens* of the thumb. That these peculiarities are also shared by man to some extent is well known. It is quite possible to train the toes to do certain kind of prehensile work, even to write, cut paper, and sew. A baby not yet able to walk can often pick up small objects with its toes. Compare the marks caused by muscular action on the sole of a baby's foot with those on the hand, and it will be seen that there are distinct signs of this prehension. Even the *opponens hallucis* of the gibbon is not infrequently found in man. The fœtal condition of the foot also approaches that of the apes, the heel being shorter and the joints so arranged that the sole can be easily turned inward. In the ape the first or great toe is turned inward and upward by shortening its metatarsal bone and setting it obliquely upon the ankle. This shortening and obliquity also occurs in the fœtus; the adult condition, in which the metatarsal bone is lengthened and set straight so as to give a longer and firmer internal border to the foot, being gradually acquired. Many savage tribes still use the foot for climbing and have a shorter metatarsal, a wider span between the first and second toes, and greater ease in inverting the sole. Connected with this ease of inversion should be mentioned a peculiar, ape-like form of the tibia that occurs in people of the stone age, in the mound builders, and in some American Indians. This is a flattened, saber-like condition of the bone known as *platycnemy*. It is apparently to give greater surface of attachment and resistance to the pull of the *tibialis anticus*, the principal muscle that turns the sole inward. It is interesting to note that this peculiarity is much more marked in some early human skeletons than in any of the anthropoid apes.

The poet says that while other animals grovelling regard the earth,

Jupiter gave to man an uplifted countenance, and ordered him to look heavenward and hold his face erect towards the stars.

> "Pronaque cum spectent animalia cetera terram,
> Os homini sublime dedit, cœlumque tueri
> Jussit, et erectos ad sidera tollere vultus."*
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> *Ovid, Metamorphoses: I*, 84–86.

The erect position is however gradually acquired. As in the sphinx's riddle, we literally go on all fours in the morning of life, and the difficulty that an infant experiences in learning to walk is strong evidence that this is an accomplishment acquired by the race late in its history. We ought (if this is the case) to find in the human body indications of a previous semi-erect posture. There is a vast amount of evidence of this character, and I can only sketch the outlines of it.

The erect position in standing is secured by the shape of the foot, by the attachment of strong muscles at points of severest strain, and by the configuration of the great joints which permits them to be held locked when a standing posture is assumed. All these features are liable to great variation; they are less marked in children and in the lower races. Let us examine them somewhat more carefully.

The Caucasian type of foot is evidently that best adapted for the erect position. The great toe is larger, stronger, and longer than the others, making a firm support for the inner anterior pier of the arch formed by the bones—an arch completed by a well-developed heel and maintained by a strong, dense band of fascia and ligament binding the piers together like the tie-rod of a bowstring truss—thus producing a light and elastic structure admirably adapted to support the weight of the body and diminish the effect of shocks. In the lower races of man all these characters are less marked. The great toe is shorter and smaller, the heel-bone less strongly made, the arch much flatter. This flattening of the arch produces the projection of the heel found in some races.

The muscles required for maintaining the erect position are those which from our predilection for human anatomy we are apt to call the *great* extensors, overlooking the fact that in other animals they are by no means as well developed as in man. Being required at the points of greatest strain, all are situated on the posterior aspect of the body—the calf, the buttock, and the back.

A very slight examination of any lower animal will show how strikingly it differs in the muscular development of these regions. The

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* Compare Milton:

> "A creature who not prone
> And brute as other creatures, but endued
> With sanctity of reason, might erect
> His stature, and upright with front serene
> Govern the rest, self-knowing."
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> *Paradise Lost: VII*, 506–510.

great muscle of man's calf, the *triceps extensor suræ*, is formed by the welding together of some four muscles separate in many lower forms. Varieties are found in man showing all grades of separation in these elements. One of the muscles, the *plantaris*, was formerly a great flexor of the toes, the plantar fascia representing its former distal extent. Like the palmaris of the arm it lost its original function by the welding of the fascia to the bones to secure the plantar arch, and its functions being then assumed by other muscles it began to dwindle, and is now represented by a mere vestigial rudiment of no functional value. It is well known that the lower races of men have smaller calves than Europeans. Again, it should be noted that as the erect position is assumed the muscles required for the flexion and independent action of the toes become reduced in character. A comparison with other forms shows that some of the small muscles now confined to the region of the foot formerly took their origin higher up, from the bones of the leg. Losing in functional importance, they have dwindled in size and gradually moved downward.

The great *glutæi* muscles of the buttock find their highest development in man. They are subject to similar variations. Certain muscles of this region, normal in apes, are occasionally found in man: a separate head of the great glutæus, derived from the ischium, and the *scansorius* or climbing muscle that assists the great flexor of the thigh (the *ilio-psoas*), may be mentioned.

The enormous size and complexity of the muscles of the back in man are well known. The erector of the spine fills up the vertebral grooves and sends up numerous tendons along the back like stays supporting the masts of a ship. The mass of this muscle is comparatively less in anthropoid apes.

Notwithstanding all these powerful muscles, it would be impossible to retain the erect position for any great length of time were we to depend upon them alone, for it requires (as before stated) a great expenditure of force to keep a muscle in active use. It becomes rapidly fatigued and then loses its power, as any one may prove by standing in any constrained position, even "in the position of a soldier," for half an hour. To provide against this, a beautiful arrangement of joints and ligaments has been developed.

When in the erect attitude the ankle-joint is so arranged that its bones are in a position of greatest stability and the center of gravity is so adjusted that it falls directly upon it. This reduces to a minimum the amount of muscular force required to keep the body erect. At the knee the center of gravity falls a little in front of the axis of the limb, and the back and sides of the joint are provided with check ligaments or straps that hold the joints locked in a position of hyper-extension, so that no muscular force whatever is used to maintain it. These ligaments are regressive structures, being vestiges of former insertions of muscles near the joint. At the hip a similar condition occurs, the

center of gravity falling behind the joint and the whole weight of the trunk being hung upon the ilio-femoral ligament, a heavily thickened portion of the joint capsule. This structure is much more marked in man than in other mammals, and is found to vary considerably in its size and strength.

The spinal column has been remarkably modified to adapt it to the erect position. Before the fifth month of uterine life the whole spine describes a single, large, dorsally directed curve like that of the quadruped, arranged to accommodate the viscera. As this would be incompatible with the erect posture, two additional curves in the opposite direction are formed: one in the region of the loins just where the center of gravity would begin to fall forward, another in the neck to counteract the heavy and unstable weight of the head. These curves are gradually acquired. While possessed by all races, and in a less degree by the higher apes, they arrive at their highest development in Europeans; while the lumbar curve of the lower races of men is much better adapted to running in a semi-erect position through the jungle or bush. Careful measurements show that the shapes of the vertebræ have been gradually modified. There is no abrupt transition from the spine of the lowest savages—Australian, Bushman, Andaman—to that of the gorilla, gibbon,and chimpanzee.

There is also evidence that the posterior limbs have moved forward upon the spinal column in order that the erect position may be assumed with less effort. In man there are between the skull and the sacrum twenty-four vertebræ. The other primates have usually twenty-six, although the gorilla, chimpanzee, and orang agree with man. Now in fœtal life the attachment of the hip-bones to the sacrum commences from below upward. Union first occurs with the third sacral vertebra, leaving twenty-six pre-sacral, then advances forward, the first sacral uniting last of all. The hip-bones actually move up along the spine a distance of two segments. Occasionally this shifting is carried still further, and but twenty-three pre-sacral vertebræ are left. Anomalies caused by an arrest of development at some stage of this process are not at all infrequent. The most common is the want of union between the hip-bones and the first sacral vertebra, thus producing apparently six lumbar vertebræ. A most beautiful specimen of this anomaly was found last winter in my laboratory.

The spine is sustained erect by stringing from vertebra to vertebra numbers of short ligaments that reduce to a minimum the muscular exertion required to support it. These are particularly numerous between the spines along the great dorsal curvature. Some of these ligaments are replaced by small muscles, very inconstant and variable, the survivals of a whole system of musculature that had for its object the moving of the separate joints of the spine, one upon another.

The head is also much modified by the erect position. In quadrupeds, its suspension requires an extensive apparatus, a large, strong,

elastic strap—the *ligamentum nuchæ*—passing from the tips of the thoracic vertebræ to the occiput, sending processes to all the neck vertebræ involved in the strain. Though need for it has in great degree ceased since the head has become poised in such a way as to involve but little expenditure of muscular force, yet relics of this great suspensory apparatus remain in man's neck in the form of thickened fascial bands.

The arrangement of the great foramen of the skull that transmits the central axis of the nervous system, the spinal cord, is necessarily different in an animal carrying its head erect. The foramen would naturally tend to be set forward more under the center of gravity and its inclination would be more nearly horizontal. Here again we see that the ideally perfect form is more nearly approached in the civilized races. It is never quite realized, and indeed the whole skull and its contents evince markedly that they are still undergoing an evolution. Again the lower races show variations that unite them with the anthropoid apes. While a negro may have a *foramen magnum* inclined 37 degrees to the horizontal, the orang may fall to 36 degrees.

But it is not only in this way that we get evidence that the erect position has been gradually acquired. Since gravity plays an important part in the functions of the visceral and circulatory systems, any marked change in the line of equilibrium must necessarily be accompanied by disturbances. These disturbances to a certain extent conflict with the acquirement of the position, as they weaken the animal. In the course of time the body may perhaps become adapted to the changed conditions, but before that perfect adaptation takes place there is a period of struggle. There is abundant evidence that such a struggle has occurred and is yet going on, the adaptation being as yet far from complete.

The most striking and important of these adaptations concerns the pelvis. When the erect posture is assumed the weight of the viscera being thrown upon this bony girdle, it becomes adapted for their support by assuming a more fixed and dish-like shape. This is naturally more pronounced in the female, since with her the pelvis must bear the additional weight of the pregnant uterus. It is evident that a solid, unyielding, laterally expanded ring of small aperture would give the most effective support in the erect position, but it is equally clear that with any such structure parturition would be impossible. In the quadruped the act of parturition is comparatively easy, the pelvis offering no serious hindrance. The shape of the female pelvis is therefore the result of a compromise between two forms, one for support, the other for ease in delivery. When we reflect that along with the acquirement of the erect position, the size of the head of the child has gradually increased, thus forming still another obstacle to delivery and to the adaptation which might otherwise have taken place, we can realize how serious the struggle has been, and no longer wonder that

deaths in child-birth are much more common in the higher races and that woman in her entire organization shows signs of having suffered more than man in the upward struggle.

In no other animal is there shown such a distinction between the pelvis of the male and that of the female, a distinction that increases as we ascend the scale. While the amount of individual variation is great, we yet see, particularly in the pelvis of the Andaman Islanders and of the Polynesian races, distinctly simian characters. The scanty material at hand indicates that a similar transition occurred between the modern and pre-historic types. The approximation of the infantile and simian forms is well known.

The pelvis alone does not suffice to support the viscera. In quadrupeds the whole weight is slung from the horizontal spine by means of a strong elastic suspensory bandage of fascia, the *tunica abdominalis.* The part of this near the thorax has in man entirely disappeared, being no longer of any use. In the groin it remains to strengthen the weak points where structures pass out from the abdominal cavity. That it often is insufficient to withstand the great pressure is testified by the great prevalence of hernia, another sign of imperfect adaptation. The frequency of uterine displacements, almost unknown in the quadruped, has also been noted. It is significant that one of the most effective postures for treating and restoring to place the disturbed organ is the so-called "knee-elbow position," decidedly quadrupedal in character.

Many other indications are found in the viscera. The urinary bladder is so arranged in man, that any concretions that may occur, do not gather near the opening of the urethra, where they might be discharged, but fall back into the cul-de-sac at the base, where they enlarge and irritate the mucous lining.* The cæcum, with its vermiform appendage, a vestigial organ finding its proper functional activity far below man, is so placed in quadrupeds that the action of gravity tends to free it from fæcal accumulations. In man this is not the case, and as a consequence inflammation of this organ or its surrounding tissues, very serious and often fatal, is by no means rare. It may be noted that the ascending colon is obliged to lift its contents against gravity, and that in a lowered state of the system this might very readily induce torpidity of function.

The gall bladder in quadrupeds also discharges at an advantageous angle. In man, although the difference is slight, it appears to be suffi-

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*Since the above was written, my attention has been called to the following remarkable passage in the works of Dr. ERASMUS DARWIN. It occurs in his "Temple of Nature," Canto ii, foot-note to line 122.

"It has been supposed by some that mankind were formerly quadrupeds as well as hermaphrodites; and that some parts of the body are not yet so convenient to an erect posture as to a horizontal one: as the fundus of the bladder in an erect posture is not exactly over the insertion of the urethea; whence it is seldom completely evacuated, and thus renders mankind more subject to the stone than if he had preserved his horizontality." (The preface to this poem is dated January 1, 1802.)

cient to cause at times retention and consequent inspissation of the bile, leading to the formation of gall-stones.

The quadruped's liver hangs suspended from the spine, but as the erect attitude is assumed it depends more and more from the diaphragm. The diaphragm in its turn develops adhesions with the fibrous covering of the heart, which is continuous with the deep fascia of the neck, so that in effect the liver hangs suspended from the top of the thorax and base of the skull. This restricts in some degree the action of the diaphragm and confines the lungs. This must have an effect upon the aëration of the blood, and consequently upon the ability to sustain prolonged and rapid muscular exertion. An extra lobe of the right lung that in animals intervenes, either constantly or during inspiration, between the heart and the diaphragm, is occasionally found in a vestigial state in man.

The vascular system abounds in evidences that it was primarily adapted to the quadrupedal position. By constant selection for enormous periods of time, the vessels have become located in the best protected situations. It is scarcely possible to injure a vessel of any size in an animal without deeply penetrating the body or passing quite through a limb. In man, on the contrary, several great trunks are comparatively exposed, notably the great vessels of the thigh, those of the forearm, and of the ventral wall.

The influence that gravity has upon the circulation is well known. The horizontal position of the great venous trunks favors the easy flow of blood to the heart without too greatly accelerating it. Man, in whom these trunks are vertical, suffers thereby from two mechanical defects,—the difficulty of raising blood through the ascending vena cava, whence come congestion of the liver, cardiac dropsy, and a number of other disorders, and the too rapid delivery through the descending cava, whence the tendency to syncope or fainting if for any cause the action of the heart is lessened. Clevenger's admirable discovery that the valves of the veins are arranged for a quadrupedal position should also be mentioned here. Evidently intended to resist the action of gravity, they should, to be effective, be found in the large vertical trunks. But in the most important of these they are wanting. Hence are caused many disorders arising from hydrostatic pressure, such as varicose veins, varicocele, hemorrhoids, and the like. Yet the values occur in several horizontal trunks, where they are, as far as we know, of no use whatever. Place man on all fours however, and it is seen that the entire system of valves is arranged with reference to the action of gravity in that position. The great vessels along the spine and the portal system being then approximately horizontal do not require valves, while all the vertical trunks of considerable size, even the intercostal and jugular veins, are provided with them. A confirmation of this view is found in the fact that the valves are variable in character and tend to disappear in the veins where they are no longer needed.

Every animal possessing a backbone may be said to be formed by the union of a series of disk-like segments arranged on a longitudinal axis. These segments are originally similar in character, but become specially modified in innumerable ways to meet the needs of the individual. Anatomists conclude, upon surveying the whole field, that this indicates a derivation of the vertebrates from some form of the annelid worms, among which a single unit produces by successive budding a compound longitudinal body. This view is fully confirmed by the behavior of the human embryo.

The number of the segments varies considerably, rising sometimes to as many as three hundred in some fishes and reptiles, and being generally greater in the animals below man. There are many indications, however, that in man, segments formerly possessed have disappeared. Leaving the skull for the present out of account, there are in the adult thirty-three or thirty-four vertebræ that may be held to represent these segments; the additional vertebra, when it occurs, almost invariably belonging to the coccygeal or caudal series. In the human embryo thirty-eight segments can at one time be made out. Four or five of these generally disappear, but cases are by no means wanting in which they remain until after birth and constitute a well-marked free tail. In one case, carefully examined and described by Lissner, a girl of 12 years had an appendage of this character 12.5 centimetres (very nearly 5 inches) long. Other observers, probably less careful and exact, report much greater lengths. From some observations it would appear that abnormities of this kind may be transmitted from parent to offspring.

Dr. Max Bartels recently collected from widely scattered literature reports of 116 actually observed and described cases of tailed men. In 35 instances, authors reported such abnormities to be possessed by an entire people, they themselves having observed certain individuals. These cases are scattered throughout the whole of the known globe and extend back for a thousand years. When we consider that the authenticity of many cases is beyond question, and that the number that escaped accurate observation and report must be much greater, we can see that we are not dealing with a phenomenon that is so rare as has generally been supposed.

Other regressive structures are abundant in this region. The spinal cord in its earlier state extended the entire length of the vertebral canal. In the child at birth it occupies only 85 per cent. of that length; in the adult 75 per cent. This is due mainly to the more rapid growth of the spine. There stretches however from the lower end of the cord down to the very end of the spine a small thread-like structure, the *filum terminale*, a degenerated vestige of the lower caudal part of the spinal cord. Wiedersheim suggests that the frequent occurrence of degenerative disorders in the lower end of the adult cord may be due to a pathological extension of the normal atrophy. Rauber found in this region traces of two additional pairs of spinal nerves. The vessel that runs

down in front of the sacrum and coccyx corresponding to the caudal artery of quadrupeds shows signs of a former more extensive distribution, as it ends in a curiously convoluted structure known as the coccygeal gland, containing vestiges of vascular and nervous tissues. Traces of caudal muscles still remain, notably the *ischio-coccygeus*, which in animals moves the tail sideways, and the anterior and posterior *sacro-coccygeus*, for flexing and extending it. Occasionally the *agitator caudæ* is found as a muscular slip passing from the femur to the coccyx. These muscles can not be of any value in man, as the coccyx is practically immovable. At the point where the end of the spine was primarily attached to the skin a dimple is formed by regressive growth, and here the direction of the hairs also indicates that an organ has become aborted.

Another interesting condition connected with segmentation is the varying number of the ribs. Most mammals have more ribs than man, and as we descend in the scale they continue to increase. A study of development indicates that a rib is probably to be considered as an integral portion of a vertebra. As the arch of a vertebra incloses the central nervous system, so the ribs inclose the visceral system. If this be correct they ought to be found throughout as far as the body cavity extends. This is really the case. They exist in the neck as the anterior bars of the transverse processes, in the loins as the transverse or costal processes themselves, in the sacrum welded together into what are known as the lateral masses. A great number of considerations derived from comparative anatomy, from embryology, and from variations found in the adult, combine to support these conclusions.

Nothing would seem less likely at first sight than that the capacious expanded brain-case or skull with its complicated structure should be composed of segmental pieces like the vertebræ; yet there is no doubt that the poet Goethe was on the right track when he made that important generalization. The details of the segmentation are very far from being worked out, but a vast amount of evidence indicates that the general conclusion is correct.

Since the predominant necessity in the construction of the skull is to afford a protection for the brain, we need not be surprised to find that it is very greatly modified in man. Enormous labor has been bestowed upon craniology in an attempt to separate definitely the races of men as well as to connect them with the lower forms. The success in establishing races has not been such as was anticipated. A constant intergrading of forms defies all attempts at a hard and fast classification. We also see types that intergrade between anthropoids and man, and find abundant evidence that the human skull was derived from a form similar to that of still lower mammals.

At first man's skull seems to be much simpler than the typical form. The bones are fewer and less complicated. But follow back the course of development and we find the bones separating—the frontal into two

pieces, the occipital and temporal each into four, the sphenoid into eight, repeating what we find as we descend the vertebrate scale.

Many of these peculiarities may remain throughout life. Such are the inter-parietal bone (found very frequently in ancient Peruvian and Arizonian skulls), the division of the frontal and temporal bones each into two, the persistence of the intermaxillary bones and of that division of the cheek or malar bone known as the *os japonicum.* Even cleft palate, a deformity and defect in man, merely re-produces a state natural to some of the lower mammals.

There are also present structures that are homologous with the so-called visceral arches represented in the thorax by ribs. Such are the lower jaw, the hyoid bone, and the thyroid cartilage. A study of the embryo shows us that these are portions of a series of bars primitively arranged on the plan of the branchial apparatus of the water-breathing vertebrates. Each bar has its appropriate skeleton and vascular supply and is separated from the contiguous ones by a cleft that at first passes entirely through the soft tissues and communicates with the primitive visceral cavity. These clefts may persist and cause serious deformities. The skeleton of the mandibular and hyoid bars is remarkable as containing indications of elements present in the lower vertebrates. In fishes, the lower jaw articulates with a large bone apparently not found in mammals, but on tracing carefully the development of the mammalian skull it is found that this bone is represented by the incus, one of the minute ossicles of the ear. In the fœtus the primitive lower jaw, in the shape of a bar of cartilage, actually extends into the ear cavity and the upper end of it remains as the malleus. Relics of the hyoid or second branchial arch are also found,—the styloid process of the temporal bone being one of them.

The capacity of the cranium is usually held to distinguish man remarkably, yet the lowest microcephali approach the apes in this respect and the lower races have unquestionably smaller brains than the higher. As far as can be judged, there has also been an increase in average capacity during historic times. One fact pointed out by Gratiolet is very significant. In monkeys and in the inferior races the ossification of the sutures commences at the anterior part of the head, while in Europeans these sutures are the last to close. This would indicate a greater and longer continued increase of the frontal lobes of the brain.

The same remarks may be made concerning the facial angle and prognathism. While by none of the different angles proposed have we been able to definitely separate distinct races, yet we find that the angle of the lower races and of microcephali approaches that of the anthropoid apes, and that as the capacity of the skull has increased the jaw has been thrust back under it to support the weight. This shortening of the jaw gives the characteristic expression of the civilized face. We at once recognize a brutal physiognomy by the projection and development of the great masticating apparatus, used in most animals

near man as a formidable weapon of defense. The shortening has produced some very remarkable changes. It has shoved the third molar or "wisdom tooth" so far back that it is crowded against the ascending part of the jaw, thereby occasioning disturbance and trouble in its eruption. Being no longer practically useful, it tends to disappear, and many people never cut any wisdom teeth. Among the Australasians, on the contrary, a fourth molar is not infrequently found; this rarely occurs in European skulls also. Evidences exist of a lost incisor in the upper jaw on each side. Dental follicles form for it and usually abort, but occasionally the tooth appears fully developed in the adult. The great canines or eye-teeth, used in apes and other animals for tearing and holding, are in them longer and larger than the other teeth, and room is made for them in the opposite jaw by leaving an interval, called the *diastema*, between the canine and the tooth next to it. These large projecting canines have disappeared in the normal human skull and the diastema has accordingly closed up. Yet it is by no means uncommon to see the whole arrangement re-appear, especially in low-type skulls. Projecting canines or "snag teeth" are so common in low faces as to be universally remarked, and would be oftener seen did not dentists interfere and remove them. It may be noted also that the muscle that lifts the lip from over the canines and bares the weapon, often re-appears in man and is used to produce snarling and disdainful expressions.

Many details of structure of the skull point in the same direction. Occasionally the occipital bone has a third condyle, as in some other mammals, or a large lateral projection like that of a vertebra, the paramastoid process, or indications of a separate centrum (*os basioticum* of Albrecht). It may have interiorly a hollow (*fossette vermienne*) for the vermiform process of the cerebellum, and exteriorly a large transverse ridge (*torus occipitalis*) on which are inserted the muscles of the nape. All these peculiarities are more frequent as we descend the scale, whether we regard the lower races of man, microcephalic individuals, or lower animals. Like many of these atavistic features they are also more common among the criminal classes.

I have omitted the discussion of many important structural features that mark various stadia in man's ascent. From the muscular system alone there could be adduced a very great number of instances of the survival of primitive forms and of progressive variations, particularly in the development of the muscles of the face and breast. In the osseous system also there are many such, among which may be mentioned the episternal bones, the central bones of the wrist and ankle, and the *os acetabuli*. The exact significance of these is still under discussion, as is also the question of supernumerary digits that sometimes appear on the hands and feet.

Additional instances might be drawn from the visceral system. The larynx contains small throat pouches like the great air sacs of the

anthropoid apes. The pharynx of the embryo is lined with cilia like that of the very lowest vertebrates. Traces of the primitive intestine are shown by the peculiar distribution of nerves and the folding of the peritoneum. The liver and spleen both occasionally indicate a previous simpler condition, and the intestine has sometimes diverticula of no functional use—indeed, likely to be disadvantageous—yet pointing to a previous state. These anomalies never occur at random, but can be explained consistently upon the theory of reversion.

The genito-urinary system abounds in them. The uterus may have two cavities, as in many quadrupeds, or approach that condition by being bicornuate, as in apes, and a great variety of other vestigial structures occur, all pointing back to an original neutral condition, before the sexes were differentiated.

In the nervous system there is no lack of instances. Our studies of the brain are as yet far from complete—indeed, we seem to be only at the threshold of a reasonable knowledge of the nervous centers—and the crowd of names, the inextricable maze of synonymy that now obscures that region, is only a mark of our ignorance. It is a case of "*omne ignotum pro mirifico;*" ignorant of the true value of the parts we examine, we have named even the most insignificant details of structure. Perhaps one of the most interesting results of modern research is the conclusion that the psychic life of our ancestors must have been different from our own, since they possessed organs of sensation differing in degree and probably in kind. The sense of smell as indicated by the size of the olfactory bulbs of the brain is decreasing in acuteness. The fœtal brain possesses comparatively larger bulbs, as do also the brains of lower races, and of anthropoid apes. The sense, being no longer required for the preservation of the species, is slowly becoming dulled. Jacobson's organ, a curious structure found in many mammals, combining in some unknown manner the olfactory and gustatory senses, occurs in a vestigial state in man, and the duct connecting it with the mouth yet remains as the anterior palatine canal. The pineal and pituitary bodies of the brain probably represent obliterated sense organs, the former being an eye, the latter having some connection with the pharynx. Our other senses have also been modified. The eye has a rudimentary third eyelid, such as birds and lizards possess, covered with minute hairs. The external ear shows signs of derivation from the pointed ear of quadrupeds and abounds in vestigial muscles such as they use for controlling and directing it.

From this rapid sketch it will be apparent to you that the evidence that man's path upward has led along the same route travelled by other animals is now very powerful in its cumulative weight. By no other argument can we satisfactorily explain the bewildering maze of resemblances; yet when called upon to fix the exact line by which we have reached our present estate we at once meet with serious difficulties. It is a popular misconception that there has been a regular chain-like

series, with now and then a "missing link." The various races of men and the higher simians are merely one branch of the great tree Yggdrasil, that overshadows the whole earth and reaches up into heaven. The individuals that we compare occupy the terminal twigs of that branch, being not related directly, but only as springing from a common stock. The fact that resemblances occur does not necessarily prove a lineal descent, but rather a common ancestry. The races of man arose far back in pre-historic night. Each in its own way fought the struggle for existence. Favored more by climate, the Caucasian appears to have attained an intellectual superiority; yet it should not be forgotten that the others also excel, each in its own special way. The white races endure with difficulty the climate of the tropics, and without help would starve in the Australian bush and the Arctic ice fields.

Notwithstanding all that I have said concerning reversive characters, we yet have hardly sufficient structural grounds for separating the races of man. Different varieties of the Caucasian race show marked variations. Between the lowest and most brutalized laborers and the cultivated and intelligent classes there exist anatomical differences as great as those which separate the white and the negro. The rapid change in the African races, remarkably shown in America in the three generations now before us, is a more conclusive proof of inferiority, as it indicates that they have not had time to acquire fixed characters.

Again, as to the anthropoid apes, it is evident that they have widely diverged from man and that none represent the primitive ancestor from which all were derived. The comparison of a human skull with that of an adult gorilla or chimpanzee is very striking. On the one hand we see all the structural features subordinated to the necessity of forming a capacious receptacle for the brain; on the other, a similar subordination for producing an effective fighting apparatus—jaws, teeth, and ridges for the insertion of powerful muscles. In one, intelligence predominates; in the other, force. The skulls of the young of all these species show however much greater resemblances than those of adults. This seems to indicate that there must have been a primitive common type from which all have diverged. Savages, when ill-fed and living in unfavorable conditions, may simulate the habits of anthropoid apes, and this has an effect upon their physical structure, yet not on that account should we too readily accept their close relationship.

In this summary I have purposely refrained from any discussion of the physiological phenomena that necessarily accompany anatomical structure. Yet these are most important. Anatomy and physiology are inseparable, each being dependent upon the other. The results of the erect position, of increased size of brain, of greater specialization of limbs, are almost incalculably great, so great that they affect the whole life of the animal, control his habits, direct his actions in war and in the chase, and finally mold peoples, nations, and races.

As Cuvier was able to deduce an animal's habits from the shape of his teeth, so we may speculate as to man's past and future from an examination of his anatomy. "*Ex pede Herculem*" has not ceased to be true. It would be impossible for me to adequately treat of all these results in one short hour; the subject must necessarily be deferred to another time and another place. If I have succeeded in showing you that structural features form no insignificant part of anthropology my object is attained.

ANTIQUITY OF MAN.*

By John Evans, F. R. S.

In the year 1870, I had the honor of presiding over what was then the Department of Ethnology in the Biological Section of the British Association at its meeting in Liverpool. Since that time 20 years have elapsed, during the greater portion of which period the subjects in which we are principally interested have been discussed in a department of anthropology forming part of the organization of the Biological Section, although since 1883 there has been a new section of the association, that of anthropology, which has thus been placed upon the same level as the various other sciences represented in this great parliament of knowledge. This gradual advance in its position among other branches of science proves, at all events, that whatever may have been our actual increase in knowledge, anthropology has gained and not lost in public estimation; and the interest in all that relates to the history, physical characteristics, and progress of the human race is even more lively and more universal than it was 20 years ago. During those years much study has been devoted to anthropological questions by able investigators, both in England and abroad, and there is at the present time hardly any civilized country in the world in which there has not been founded, under some form or another, an anthropological society, the publications of which are yearly adding a greater or less quota to our knowledge. The subjects embraced in these studies are too numerous and too vast for me to attempt even in a cursory manner to point out in what special departments the principal advances have been made, or to what extent views that were held as well established 20 years ago have had either to be modified in order to place them on a surer foundation, or have had to be absolutely abandoned. Nor could I undertake to enumerate all the new lines of investigation which the ingenuity of students has laid open, or the different ways in which investigations that at first sight might appear more curious than useful have eventually been found to have a direct bearing upon the ordinary affairs of human life, and their results to be susceptible of

* Presidential address before the Anthropological Section of the British Association Adv. Sci. meeting at Leeds, September, 1890. (From *Nature*, September 18, 1890, vol. XLII, pp. 507–510.)

application towards the promotion of the public welfare. I may however in the short space of time to which an opening address ought to be confined, call your attention to one or two subjects, both theoretical and practical, which are still under discussion by anthropologists, and on which as yet no general agreement has been arrived at by those who have most completely gone into the questions involved.

One of these questions is: What is the antiquity of the human race, or rather, what is the antiquity of the earliest objects hitherto found which can with safety be assigned to the handiwork of man? This question is susceptible of being entirely separated from any speculations as to the genetic descent of mankind; and even were it satisfactorily answered to-day, new facts might to-morrow come to light that would again throw the question entirely open. On any view of probabilities it is in the highest degree unlikely that we shall ever discover the exact cradle of our race, or be able to point to any object as the first product of the industry and intelligence of man. We may however I think, hope that from time to time fresh discoveries may be made of objects of human art—under such circumstances and conditions that we may infer with certainty that at some given point in the world's history mankind existed, and in sufficient numbers for the relics that attest this existence to show a correspondence among themselves, even when discovered at remote distances from each other.

Thirty-one years ago, at the meeting of this association at Aberdeen, when Sir Charles Lyell, in the Geological Section, called attention to the then recent discoveries of Palæolithic implements in the valley of the Somme, his conclusions as to their antiquity were received with distrust by not a few of the geologists present. Five years afterwards, in 1864, when Sir Charles presided over the meeting of this association at Bath, it was not without reason that he quoted the saying of the Irish orator, that "they who are born to affluence can not easily imagine how long a time it takes to get the chill of poverty out of one's bones." Nor was he wrong in saying that "we of the living generation, when called upon to make grants of thousands of years in order to explain the events of what is called the modern period, shrink naturally at first from making what seems so lavish an expenditure of past time. Throughout our early education we have been accustomed to such strict economy in all that relates to the chronology of the earth and its inhabitants in remote ages, so fettered have we been by old traditional beliefs, that even when our reason is convinced and we are persuaded that we ought to make more liberal grants of time to the geologist, we feel how hard it is to get the chill of poverty out of our bones."

And yet of late years how little have we heard of any scruples in accepting as a recognized geological fact, that both on the continent of Europe and in these islands, which were then more closely connected with that continent, man existed during what is known as the Quaternary period, and was a contemporary of the mammoth and hairy rhinoc-

eros, and of other animals, several of which are either entirely or locally extinct. It is true that there are still some differences of opinion as to the exact relation in time of the beds of river gravel containing the relics of man and the Quaternary fauna to the period of great cold which is known as the Glacial period. Some authors have regarded the gravels as pre-Glacial, some as Glacial, and some as post-Glacial; but after all, this is more a question of terms than of principle. All are agreed for instance, that in the eastern counties of England implements are found in beds posterior to the invasion of cold conditions in that particular region, though there may be doubts as to how much later these conditions may have prevailed in other parts of this country. All too are agreed that since the deposit of the gravels considerable changes have taken place in the configuration of the surface of the country, and that the time necessary for such changes must have been very great, though those in whose bones the chill of poverty still clings are inclined to call in influences by which the time required for the erosion of the river valleys in which the gravels occur may be theoretically diminished.

On the other hand, there have been not a few who, feeling that the evidence of the existence of the human race has now been satisfactorily established for Quaternary times, and that there is no proof that what has been found in the ordinary gravels belongs to anything like the first phases of the family of man, have sought to establish his existence in far earlier Tertiary times. In the view that earlier relics of man than those found in the river gravels may eventually be discovered, most of those who have devoted special attention to the subject will, I think, concur. But such an extension of time can only be granted on conclusive evidence of its necessity, and before accepting the existence of Tertiary man the grounds on which his family tree is based require to be most carefully examined.

Let me say a few words as to the principal instances on which the believer in Tertiary man relies. These may be classified under three heads:* (1) the presumed discovery of parts of the human skeleton; (2) that of animal bones said to have been cut and worked by the hand of man; and (3) that of flints thought to be artificially fashioned.

On most of these I have already commented elsewhere.† Under the first head I may mention the skull discovered by Prof. Cocchi at Olmo, near Arezzo, with which, however, distinctly Neolithic implements were associated; the skeletons found at Castelnedolo, of which I need only say that M. Sergi, who described the discovery, regarded them as the remains of a family party who had suffered shipwreck in Pliocene times; and the fossil man of Denise, in the Auvergne, mentioned by

*See A. Arcelin, "L'Homme Tertiaire," Paris, 20 rue de la Chaise, 1889.

† *Trans. Herts. Nat. Hist. Soc.*, vol. I, p. 145; "Address to the Anthrop. Inst.," 1883, *Anthrop Journ.*, vol. XII, p. 565.

Sir Charles Lyell, who may have been buried in more recent times under lava of Pliocene date. On these discoveries no superstructure can be built. The Calaveras skull seems to have better claims to a high antiquity. It is said to have been found at a depth of 153 feet in the auriferous gravels of California, containing remains of mastodon, and covered by five or six beds of lava or volcanic ashes. But here again doubts enter into the case, as well-fashioned mortars, stone hatchets, and even pottery are said to occur in the same deposits. In the same way the discoveries of M. Ameghino at the mouth of the Plata, in the Argentine Republic, require much further corroboration.

The presumably worked bones which I have placed in the second category, such as those with incisions in them, from St. Prest, near Chartres, the cut bones of Cetacea in Tuscany, the fractured bones in our own crag deposits, and numerous other specimens of a similar character, have, by most geologists, been regarded as bearing marks entirely due to natural agencies. It seems more probable that in bones deposited at the bottom of Pliocene seas cuts and marks should have been produced by the teeth of carnivorous fish than by men who could only have lived on the shores of the seas, and who have left behind them no instruments by which such cuts as those on the bones could have been produced.

As to the third category, the instruments of flint reported to have been found in Tertiary deposits, those best known are from St. Prest and Thenay, in the northwest of France, and Otta, in Portugal.

These three localities I have visited; and though at the two former, the beds in which the flints were said to have been found are certainly Pliocene, there is considerable doubt in some cases whether the flints have been fashioned at all, and in others where they appear to have been wrought, whether they belong to the beds in which they are reported to have been found, and have not come from the surface of the ground. Even the suggestion that the flints of Thenay were fashioned by the Dryopithecus, one of the precursors of man, has now been retracted. At Otta the flakes that have been found present, as a rule, only a single bulb of percussion, and having been found on the surface, their evidence is of small value. The exact geological age of the beds in which they have occurred is moreover somewhat doubtful. On the whole, therefore, it appears to me that the present verdict as to Tertiary man must be in the form "Not proven."

When we consider the vast amount of time comprised in the Tertiary period, with its three great principal subdivisions of the Eocene, Miocene, and Pliocene, and when we bear in mind that of the vertebrate land animals, of the Eocene, no one has survived to the present time, while of the Pliocene, but one—the hippopotamus—remains unmodified, the chances that man as at present constituted should also be a survivor from that period seem remote; and against the species *Homo sapiens* having existed in Miocene times, almost incalculable. The *à*

priori improbability of finding man unchanged, while all the other vertebrate animals around him have, from natural causes, undergone more or less extensive modification, will induce all careful investigators to look closely at any evidence that would carry him back beyond Quaternary times; and though it would be unsafe to deny the possibility of such an early origin for the human race, it would be unwise to regard it as established except on the clearest evidence.

Another question of more general interest than that of the existence of Tertiary man is that of the origin and home of the Aryan family. The views upon this subject have undergone important modification during the last 20 years. The opinions based upon comparative philology alone have received a rude shock and the highlands of Central Asia are no longer accepted without question as the cradle of the Aryan family, but it is suggested that their home is to be sought somewhere in northern Europe. While the Germans contend that the primitive Aryans were the blue-eyed dolichocephalic race of which the Scandinavians and North Germans are typical examples, the French are in favor of the view that the dark-haired brachycephalic race of Gauls, now well represented in the Auvergne, is that of the primitive Aryans. I am not going to enter deeply into this question, on which Canon Isaac Taylor has recently published a comprehensive treatise, and Mr. Frank Jevons a translation of Dr. Schrader's much more extensive work, "The Pre-historic Antiquities of the Aryan Peoples." Looking at the changes that all languages undergo, (even when they have the advantage of having been reduced into the written form,) and bearing in mind the rapidity with which these changes are effected; bearing in mind, also, our extreme ignorance of the actual forms of language in use among pre-historic races unacquainted with the art of writing, I, for one, can not wonder at a something like a revolt having arisen against the dogmatic assertions of those who have, in their efforts to re construct early history, confined themselves simply to the comparative study of languages and grammar. But notwithstanding any feeling of this kind, I think that all must admire the enormous industry and the varied critical faculties of those who have pursued these studies, and must acknowledge that the results to which they have attained can not lightly be set aside, and that so far as language alone is concerned, the different families, their provinces, and mutual relations, have in the main become fairly established. The study of "linguistic paleontology," as it has been termed will help no doubt in determining still more accurately the affinities of the different forms of language, and in fixing the dates at which one separated from another, as well as the position that each should occupy on the family tree, if such a tree exists. But even here there is danger of relying too much on negative evidence; and the absence—in the presumed original Aryan language—of special words for certain objects in general use, ought not to be regarded as affording absolute proof that such objects were unknown at

the time when the languages containing such words separated from the parent stock. Not only Prof. Huxley, but Broca and others have insisted that language as a test of race is as often as not, or even more often than not, entirely misleading. The manner in which one form of language flourishes at the expense of another; the various ways in which a language spreads even otherwise than by conquest; the fact that different races with totally different physical characteristics are frequently found speaking the same language or but slightly different dialects of it;--all conduce to show how imperfect a guide comparative philology may be so—far as anthropological results are concerned.

Of late, pre-historic archæology has been invoked to the aid of linguistic researches; but here again there is great danger of those who are most conversant with the one branch of knowledge being but imperfectly acquainted with the other. The different conditions prevailing in different countries, the degrees of intercourse with other more civilized nations, and local circumstances which influence the methods of life, all add difficulties to the laying down of any comprehensive scheme of archæological arrangement which shall embrace the relics, whether sepulchral or domestic, of even so limited an area as that of Europe. We are all naturally inclined to assume that the record of the past is comparatively complete. But in archæology no more than geology does this appear to be the case. The interval between the period of the river-gravels and that of the caves, such as Kent's Cavern, in England, and those of the reindeer period of the south of France, may have been but small, but our knowledge of the transition is next to none. The gap between the Palæolithic period and the Neolithic has, to my mind, still to be bridged over, and those who regard the occupation of the Belgian caves as continuous from the days of the reindeer down to late Neolithic times seem to me possessed of great powers of faith. Even the relations in time between the *kjokkenmoddings* of Denmark and the remains of the Neolithic age of that country are not as yet absolutely clear; and who can fix the exact limits of that age? Nor has the origin and course of extension of the more recent Bronze civilization been as yet satisfactorily determined; and until more is known both as to the geographical and chronological development of this stage of culture, we can hardly hope to establish any detailed succession in the history of the Neolithic civilization that went before it. In the meantime it will be for the benefit of our science that speculations as to the origin and home of the Aryan family should be rife; but it will still more effectually conduce to our eventual knowledge of this most interesting question if it be consistently borne in mind that they are but speculations.

Turning from theoretical to practical subjects, I may call attention to the vastly improved means of comparison and study that the ethnologists of to-day possess as compared with those of 20 years ago. Not only have the books and periodicals that treat of ethnology multi-

plied in all European languages, but the number of museums that have been formed with the express purpose of illustrating the manners and customs of the lower races of mankind has also largely increased. On the Continent, the museums of Berlin, Paris, Copenhagen, and other capitals have either been founded or greatly improved; while in England our ethnological collections infinitely surpass, both in the number of objects they contain and in the method of their arrangement, what was accessible in 1870. The Blackmore Museum at Salisbury was at that time already founded, but has since been considerbly augmented. In London, also, the Christy collection was already in existence, and calculated to form an admirable nucleus around which other objects and collections might cluster; and thanks in a great degree to the trustees of the Christy collection, and in a far greater degree to the assiduous attention and unbounded liberality of the keeper of the department, Mr. Franks, the ethnological galleries at the British Museum will bear comparison with any of those in the other European capitals. The collections of pre-historic antiquities, enlarged by the addition of the fine series of urns and other relics from British barrows explored by Canon Greenwell, which he has generously presented to the nation, and by other accessions, especially from the French caverns of the Reindeer period, is now of the highest importance. Moreover, for purposes of comparison the collections of antiquities of the Stone and Bronze periods found in foreign countries is of enormous value. In the ethnological department the collections have been materially increased by the numerous travellers and missionaries which this country is continually sending forth to assist in the exploration of the habitable world; and the student of the development of human civilization has now the actual weapons, implements, utensils, dress, and other appliances of most of the known savage peoples ready at hand for examination, and need no longer trust to the often imperfect representations given in books of travel. But besides the collection at Bloomsbury there is another most important museum at Oxford, which that university owes to the liberality of General Pitt-Rivers. It is arranged in a somewhat different manner from that in London, the main purpose being the exhibition of the various modifications which ornaments, weapons, and instruments in common use have undergone during the process of development. The skillful application of the doctrine of evolution to the forms and characters of these products of human art gives to this collection a peculiar charm, and brings out the value of applying scientific methods to the study of all that is connected with human culture, even though at first sight the objects brought under consideration may appear to be of the most trivial character. - - -

The subjects of an anthropological survey of the tribes and castes in our Indian possessions, and of the continued investigation of the habits, customs, and physicial characteristics of the northwestern tribes of the Dominion of Canada, were both recommended for consid-

eration to the council of this association by the general committee at the meeting at Newcastle. We have heard from the report of the council what has been done in the matter. The rapidity with which the various native tribes in different parts of the world are either modified, or in some cases exterminated, affords a strong argument for their characteristics, both physical and mental, being investigated without delay.

There are indeed now but few parts of the world the inhabitants of which have not, through the enterprise of travellers, been brought more or less completely within our knowledge. Even the center of the dark African continent promises to become as well known as the interior of South America, and to the distinguished traveller who has lately returned among us, anthropologists as well as geographers owe their warmest thanks. It is not a little remarkable to find so large a tract of country still inhabited by the same diminutive race of human beings that occupied it at the dawn of European history, and whose existence was dimly recognized by Homer and Herodotus. The story related by the latter about the young men of the Nasamones who made an expedition into the interior of Libya and were there taken captive by a race of dwarfs receives curious corroboration from modern travellers. Herodotus may indeed slightly err when he reports that the color of these pygmies was black, and when he regards the river on which their principal town was situated as the Nile. Stanley however who states that there are two varieties of these pygmies, utterly dissimilar in complexion, conformation of the head, and facial characteristics, was not the first to re-discover this ancient race. At the end of the sixteenth century, Andrew Battel, our countryman, who, having been taken captive by the Portuguese, spent many years in the Congo district, gave an account of the Matimbas, a pigmy nation of the height of boys of twelve years old; and in later times Dr. Wolff and others have recorded the existence of the same or similar races in Central Africa. Nor must we forget that for a detailed account of an Acca skeleton we are indebted to the out-going president of this association, Professor Flower. It is not however my business here to enter into any detailed account of African exploration or anthropology. I have made this incidental mention of these subjects rather from a feeling that in Africa, as well as in Asia and America, native races are in danger of losing their primitive characteristics, if not of partial or total extermination, and that there also the anthropologist and naturalist must take the earliest possible opportunities for their researches.

THE PRIMITIVE HOME OF THE ARYANS.*

By Prof. A. H. Sayce.

In my address to the Anthropological Section of the British Association in 1887, I stated that in common with many other anthopologists and comparative philologists, I had come to the conclusion that the primitive home of the Aryans was to be sought in northeastern Europe. The announcement excited a flutter in the newspapers, many of whose readers had probably never heard of the Aryans before, while others of them had the vaguest possible idea of what was meant by the name.

Unfortunately it is a name which, unless carefully defined, is likely to mislead or confuse. It was first introduced by Prof. Max Müller and applied by him in a purely linguistic sense. The "discovery" of Sanskrit and the researches of the pioneers of comparative philology had shown that a great family of speech existed, comprising Sanskrit and Persian, Greek and Latin, Teutonic and Slav, all of them sister-languages descended from a common parent, of which however no literary monuments survived. In place of the defective or cumbersome titles of Indo-German, Indo-European, and the like, which had been suggested for it, Prof. Max Müller proposed to call it Aryan—a title derived from the Sanskrit *Árya*, interpreted "noble" in later Sanskrit, but used as a national name in the hymns of the Rig-Veda.

It is much to be regretted that the name has not been generally adopted. Such is the case however, and it is to day like a soul seeking a body in which to find a habitation. But the name is an excellent one, though the philologists of Germany, who govern us in such matters, have refused to accept it in the sense proposed by its author; and we are therefore at liberty to discover for it a new abode and to give to it a new scientific meaning.

In the enthusiasm kindled by the sight of the fresh world that was opening out before them the first disciples of the science of comparative philology believed that they had found the key to all the secrets of man's origin and earlier history. The parent speech of the Indo-European languages was entitled the *Ursprache*, or "Primeval Language," and its analysis, it was imagined, would disclose the elements of articu-

* From *The Contemporary Review*, July, 1889, vol. LVI, pp. 106-119.

late speech and the process whereby they had developed into the manifold languages of the present world. But this was not enough. The students of language went even further. They claimed not only the domain of philology as their own, but the domain of ethnology as well. Language was confounded with race, and the relationship of tribe with tribe, of nation with nation, was determined by the languages they spoke. If the origin of a people was required, the question was summarily decided by tracing the origin of its language. English is on the whole a Teutonic language, and therefore the whole English people must have a Teutonic ancestry. The dark-skinned Bengali speaks languages akin to our own; therefore the blood which runs in his veins must be derived from the same source as that which runs in ours.

The dreams of universal conquest indulged in by a young science soon pass away as facts accumulate, and the limit of its powers is more and more strictly determined. The *Ursprache* has become a language of comparatively late date in the history of linguistic development, which differed from Sanskrit or Greek only in the fuller inflexional character. The light its analysis was believed to cast on the origin of speech has proved to be the light of a will-o'-the-wisp, leading astray and perverting the energies of those who might have done more profitable work. The mechanism of primitive language often lies more clearly revealed in a modern Bushman's dialect or the grammar of Esquimaux than in that much-vaunted *Ursprache* from which such great things were once expected by the philosophy of human speech.

Ethnology has avenged the invasion of its territory by linguistic science, and has in turn claimed a province which is not its own. It is no longer the comparative philologist, but the ethnologist, who now and again uses philological terms in an ethnological sense, or settles racial affinities by an appeal to language. The philologist first talked about an "Indo-European race;" such an expression could now be heard only from the lips of a youthful ethnologist.

As soon as the discovery was made that the Indo-European languages were derived from a common mother, scholars began to ask where that common mother-tongue was spoken. But it was agreed on all hands that this must have been somewhere in Asia. Theology and history alike had taught that mankind came from the East and from the East accordingly the *Ursprache* must have come too. Hitherto Hebrew had been generally regarded as the original language of humanity; now that the Indo-European *Ursprache* had deprived Hebrew of its place of honor, it was natural, if not inevitable, that like Hebrew, it should be accounted of Asiatic origin. Moreover it was the discovery of Sanskrit that had led to the discovery of the *Ursprache*. Had it not been for Sanskrit, with its copious grammar, its early literature, and the light which it threw on the forms of Greek and Latin speech, comparative philology might never have been born. Sanskrit was the magician's wand which had called the new science into existence, and without the help of Sanskrit

the philologist would not have advanced beyond the speculations and guesses of classical scholars. What wonder then if the language which had thus been a key to the mysteries of Greek and Latin, and which seemed to embody older forms of speech than they, should have been assumed to stand nearer to the *Ursprache* than the cognate languages of Europe? The assumption was aided by the extravagant age assigned to the monuments of Sanskrit literature. The poems of Homer might be old, but the hymns of the Veda, it was alleged, mounted back to a primeval antiquity, while the Institutes of Manu represented the oldest code of laws existing in the world.

There was yet another reason which contributed to the belief that Sanskrit was the first-born of the Indo-European family. The founders of comparative philology had been preceded in their analytic work by the ancient grammarians of India. It was from Pânini and his predecessors that the followers of Bopp inherited their doctrine of roots and suffixes and their analysis of Indo-European words. The language of the Veda had been analyzed 2,000 years ago as no other single language had ever been analyzed before or since. Its very sounds had been carefully probed and distinguished, and an alphabet of extraordinary completeness had been devised to represent them. It appeared as if the elements out of which the Sanskrit vocabulary and grammar had grown had been laid bare in a way that was possible in no other language, and in studying Sanskrit accordingly the scholars of Europe seemed to feel themselves near to the very beginnings of speech.

But it was soon perceived that if the primitive home of the Indo-European languages were Asia, they themselves ought to exhibit evidences of the fact. There are certain objects and certain phenomena which are peculiar to Asia, or at all events are not to be found in Europe, and words expressive of these ought to be met with in the scattered branches of the Indo-European family. If the parent language had been spoken in India, the climate in which they were born must have left its mark upon the face of its offspring.

But here a grave difficulty presented itself. Men have short memories, and the name of an object which ceases to come before the senses is either forgotten or transferred to something else. The tiger may have been known to the speakers of the parent language, but the words that denoted it would have dropped out of the vocabulary of the derived languages which were spoken in Europe. The same word which signifies an oak in Greek signifies a beech in Latin. We can not expect to to find the European languages employing words with meanings which recall objects met with only in Asia.

How then are we to force the closed lips of our Indo-European languages, and compel them to reveal the secret of their birth-place? Attempts have been made to answer this question in two different ways.

On the one hand it has been assumed that the absence in a particular language, or group of languages, of a term which seems to have been

possessed by the parent speech, is evidence that the object denoted by it was unknown to the speakers. But the assumption is contradicted by experience. Because the Latin *equus* has been replaced by *caballus* in the modern Romanic languages, we can not conclude that the horse was unknown in Western Europe after the fall of the Roman Empire. The native Basque word for a "knife," *haistoa*, has been found by Prince L.-L. Bonaparte in a single obscure village; elsewhere it has been replaced by terms borrowed from French or Spanish. Yet we can not suppose that the Basques were unacquainted with instruments for cutting until they had been furnished with them by their French and Spanish neighbors. Greek and Latin have different words for "fire;" we can not argue from this that the knowledge of fire was ever lost among any of the speakers of the Indo-European tongues. In short, we can not infer from the absence of a word in any particular language that the word never existed in it; on the contrary, when a language is known to us only in its literary form it is safe to say that it must have employed many words besides those contained in its dictionary.

A good illustration of the imposibility of arriving at any certain results as long as we confine our attention to words which appear in one but not in another of two cognate languages is afforded by the Indo-European words which denote a sheet of water. There is no word of which it can be positively said that it is found alike in the Asiatic and the European branches of the family. Lake, ocean, even river and stream, go by different names. A doubt hangs over the word for "sea;" it is possible, but only possible, that the Sanskrit *pâthas* is the same word as the Greek πόντος, the etymology of which is not yet settled. Nevertheless, we know that the speakers of the parent language must have been acquainted, if not with the sea, at all events with large rivers. *Naus*, "a ship," is the common heritage of Sanskrit and Greek, and must thus go back to the days when the speakers of the dialects which afterwards developed into Sanskrit and Greek still lived side by side. It survives, like a fossil in the rocks, to assure us that they were a water-faring people, and that the want of a common Indo-European word for lake or river is no proof that such a word may not have once existed.

The example I have just given illustrates the second way in which the attempt has been made to solve the riddle of the Indo-European birthplace. It is the only way in which the attempt can succeed. Where precisely the same word, with the same meaning, exists in both the Asiatic and the European members of the Indo-European family—always supposing, of course, that it has not been borrowed by either of them—we may conclude that it also existed in the parent speech. When we find the Sanskrit *aswas* and the Latin *equus*, the exact phonetic equivalents of one another, both alike signifying "horse," we are justified in believing that the horse was known in the country from which both languages derived their ancestry. Though the argument

from a negative proves little or nothing, the argument from agreement proves a great deal.

The comparative philologist has by means of it succeeded in sketching in outline the state of culture possessed by the speakers of the parent language, and the objects which were known to them. They inhabited a cold country. Their seasons were three in number, perhaps four, and not two, as would have been the case had they lived south of the temperate zone. They were nomad herdsmen, dwelling in hovels, similar, it may be, to the low round huts of sticks and straw built by the Kabyles on the mountain-slopes of Algeria. Such hovels could be erected in a few hours, and left again as the cattle moved into higher ground, with the approach of spring, or descended into the valleys when the winter advanced. The art of grinding corn seems to have been unknown, and crushed spelt was eaten instead of bread. A rude sort of agriculture was however already practiced; and the skins worn by the community, with which to protect themselves against the rigors of the climate, were sewn together by means of needles of bone. It is even possible that the art of spinning had already been invented, though the art of weaving does not appear to have advanced beyond that of plaiting reeds and withies. The community still lived in the stone age. Their tools and weapons were made of stone or bone, and if they made use of gold or meteoric iron, it was of the unwrought pieces picked up from the ground and employed as ornaments; of the working of metals they were entirely ignorant. As among savage tribes generally, the various degrees of relationship were minutely distinguished and named, even the wife of a husband's brother receiving a special title; but they could count at least as far as a hundred. They believed in a multitude of ghosts and goblins, making offerings to the dead, and seeing in the bright sky a potent deity. The birch, the pine, and the withy were known to them; so also were the bear and wolf, the hare, the mouse, and the snake, as well as the goose and raven, the quail and the owl. Cattle, sheep, goats, and swine were all kept; the dog had been domesticated, and in all probability also the horse. Last, but not least, boats were navigated by means of oars, the boats themselves being possibly the hollowed trunks of trees.

This account of the primitive community is necessarily imperfect. There must have been many words, like that for "river," which were once possessed by the parent speech, but afterwards lost in either the Eastern or Western branches of the family. Such words the comparative philologist has now no means of discovering. He must accordingly pass them over along with the objects or ideas which they represent. The picture he can give us of the speakers of the primeval Indo-European language can only be approximately complete. Moreover it is always open to correction. Some of the words we now believe to have been part of the original stock carried away by the derived dialects of Asia and Europe may hereafter turn out to have been borrowed by one of

these dialects from another, and not to have been a heritage common to both. It is often very difficult to decide whether we are dealing with borrowed words or not. If a word has been borrowed by a language before the phonetic changes had set in which have given the language its peculiar complexion, or while they were in the course of progress, it will undergo the same alteration as native words containing the same sounds. The phonetic changes which have marked off the High German dialects from their sister tongues do not seem to go back beyond the fall of the Roman Empire, and words borrowed from Latin before that date will accordingly have submitted to the same phonetic changes as words of native origin. Indeed, when once a word is borrowed by one language from another and has passed into common use it soon becomes naturalized and is assimilated in form and pronunciation to the words among which it has come to dwell. A curious example of this is to be found in certain Latin words which made their way into the Gaelic dialects in the fourth or fifth century. We often find a Gaelic *c* corresponding to a Welsh *p*, both being derived from a labialized guttural or *qu*, and the habit was accordingly formed of regarding a *c* as the natural and necessary representative of a foreign *p*. When therefore words like the Latin *pascha* and *purpura* were introduced by Christianity into the Gaelic branch of the Keltic family they assumed the form of *caisg* and *corcur*.

It is clear that such borrowings can only take place where the speakers of two different languages have been brought into contact with one another. Before the age of commercial intercourse between Europe and India we can not suppose that European words could have been borrowed by Sanskrit or Persian, or Sanskrit and Persian words by the European languages. But the case is quite otherwise if instead of comparing together the vocabularies of the Eastern and Western members of the Indo-European stock we wish to compare only Western with Western or Eastern with Eastern. There our difficulties begin, and we must look to history, or botany, or zoölogy for aid. From a purely philological point of view the English *hemp*, the old high German *hanf*, the old Norse *hanpr*, and the Latin *cannabis* might all be derived from a common source, and point to the fact that hemp was known to the first speakers of the Indo-European languages in northwestern Europe. But the botanists tell us that this could not have been the case. Hemp is a product of the East which did not originally grow in Germany, and consequently both the plant itself and the name by which it was called must have come from abroad. So again, the lion bears a similar name in Greek and Latin, in German, in Slavonic, and in Keltic. But the only part of Europe in which the lion existed at a time when the speakers of an Indo-European language could have become acquainted with it were the mountains of Thrace, and it must accordingly have been from Greek that its name spread to the other cognate languages of the West.

It has been needful to enter into these details before we can approach the question, What was the original home of the parent Indo-European language? They have been too often ignored or forgotten by those who have set themselves to answer the question, and to this cause must be ascribed the larger part of the misunderstandings and false conclusions to which the inquiry has given birth.

Until a few years ago, I shared the old belief that the parent speech had its home in Asia, probably on the slopes of the Hindu Kush. The fact that the languages of Europe and Asia alike possessed the same words for "winter" and "ice" and "snow," and that the only two trees whose names were preserved by both—the "birch" and the "pine"—were inhabitants of a cold region, proved that this home did not lie in the tropics. But the uplands of the Hindu Kush, or the barren steppes in the neighborhood of the Caspian Sea, or even the valleys of Siberia, would answer to the requirements presented by such words. Taken by themselves they were fully compatible with the view that the first speakers of the Indo-European tongues were an Asiatic people.

But when I came to ask myself what were the grounds for holding this view, I could find none that seemed to me satisfactory. There is much justice in Dr. Latham's remark that it is unreasonable to derive the majority of the Indo-European languages from a continent to which only two members of the group are known to belong, unless there is an imperative necessity for doing so. These languages have grown out of dialects once existing within the parent speech itself; and it certainly appears more probable that two of such dialects or languages should have made their way into a new world, across the bleak plains of Tartary, than that seven or eight should have done so. The argument, it is true, is not a strong one, but it raises at the outset a presumption in favor of Europe. Before the dialects had developed into languages their speakers could not have lived far apart; there is in fact evidence of this in the case of Sanskrit and Persian; and a more widely spread primitive community is implied by the numerous languages of Europe than by the two languages of Asia. A widley spread community however is less likely to wander far from its original seat than a community of less extent, more especially when it is a community of herdsmen and the tract to be traversed is long and barren.

Apart from the general prejudice in favor of an Asiatic origin due to old theological teaching and the effect of the discovery of Sanskrit, I can find only two arguments which have been supposed to be of sufficient weight to determine the choice of Asia rather than of Europe as the cradle of Indo-European speech. The first of these arguments is linguistic, the second is historical or rather quasi-historical. On the one hand it has been laid down by eminent philologists that the less one of the derived languages has deflected from the parent speech, the more likely it is to be geographically nearer to its earliest home. The faithfulness of the record is a test of geographical proximity. As Sanskrit

was held to be the most primitive of the Indo-European languages, to reflect most clearly the features of the parent speech, the conclusion was drawn that that parent speech had been spoken at no great distance from the country in which the hymns of the Rig-Veda were first composed. The conclusion was supported by the second argument drawn from the sacred books of Parsaism. In the Vendîdâd the migrations of the Iranians were traced back through the successive creations of Ormazd to Airyanem Vaêjô, "the Aryan Power," which Lassen localized near the sources of the Oxus and Jaxartes. But Bréal and De Harlez have shown that the legends of the Vendîdâd, in their present form, are late and untrustworthy—later, in fact, than the Christian era;* and even if we could attach any historical value to them, they would tell us only from whence the Iranians believed their own ancestors to have come, and would throw no light on the cradle of the Indo-European languages as a whole. The first argument is one which I think no student of language would any longer employ. As Professor Max Müller has said, it would suffice to prove that the Scandinavians emigrated from Iceland. But to those who would still urge it, I must repeat what I have said elsewhere. Although in many respects Sanskrit has preserved more faithfully than the European languages the forms of primitive Indo-European grammar, in many other respects the converse is the case. In the latest researches into the history of Indo-European grammar, Greek holds the place once occupied by Sanskrit. The belief that Sanskrit was the elder sister of the family led to the assumption that the three short vowels ă, ĕ, and ŏ have all originated from an earlier ă. I was, I believe, the first to protest against this assumption in 1874, and to give reasons for thinking that the single monotonous ă of Sanskrit resulted from the coalescence of three distinct vowels. The analogy of other languages goes to show that the tendency of time is to reduce the number of vocalic sounds possessed by a language, not the contrary. In place of the numerous vowels possessed by ancient Greek, modern Greek can now show only five, and cultivated English is rapidly merging its vowel sounds into the so-called "neutral" ə. Since my protest the matter has been worked out by Italian, German, and French scholars, and we now know that it is the vocalic system of the European languages rather than of Sanskrit which most faithfully represents the oldest form of Indo-European speech. The result of the discovery, for discovery it must be called, has been a complete revolution in the study of Indo-European etymology, and still more of Indo-European grammar, and whereas ten years ago it was Sanskrit which was invoked to explain Greek, it is to Greek that the "new school" now turns to explain Sanskrit. The comparative philologist necessarily cannot do without the help of both; the greater

* Bréal, "*Mélanges de Mythologie et de Linguistique*" (1878), pp. 187–215. De Harlez, "*Introduction à l'Étude de l'Avesta*" pp. cxcii, *sqq.* Compare Darmesteter's *Introduction to the Zend-Avesta,* pt. 1, in "The Sacred Books of the East."

the number of languages he has to compare the sounder will be his inductions; but the primacy which was once supposed to reside in Asia has been taken from her. It is Greek, and not Sanskrit, which has taught us what was the primitive vowel of the reduplicated syllable of the perfect and the augment of the aorist, and has thus narrowed the discussion into the origin of both.

Until quite recently however the advocates of the Asiatic home of the Indo-European languages found a support in the position of the Armenian language. Armenian stands midway, as it were, between Persia and Europe, and it was imagined to have very close relations with the old language of Persia. But we now know that its Persian affinities are illusory, and that it must really be grouped with the languages of Europe. What is more, the decipherment of the cuneiform inscriptions of Van has cast a strong light on the date of its introduction into Armenia. These inscriptions are the records of kings whose capital was at Van, and who marched their armies in all directions during the ninth, eighth, and seventh centuries before our era. The latest date that can as yet be assigned to any of them is B. C. 640. At this time there were still no speakers of an Indo-European language in Armenia. The language of the inscriptions has no connection with those of the Indo-European family, and the personal and local names occurring in the countries immediately surrounding the dominions of the Vannic kings, and so abundantly mentioned in their texts, are of the same linguistic character as the Vannic names themselves.

The evidence of classical writers fully bears out the conclusions to be derived from the decipherment of the Vannic inscriptions. Herodotus (VII. 73) tells us that the Armenians were colonists from Phrygia, the Phrygians themselves having been a Thrakian tribe which had migrated into Asia. The same testimony was borne by Eudoxos,* who further averred that the Armenian and Phrygian languages resembled one another. The tradition must have been recent in the time of Herodotus, and we shall probably not go far wrong if we assign the occupation of Armenia by the Phrygian tribes to the age of upheaval in Western Asia which was ushered in by the fall of the Assyrian Empire. Professor Fick has shown that the scanty fragments of the Phrygian language that have survived to us belong to the European branch of the Indo-European family, and thus find their place by the side of Armenian.

Instead therefore of forming a bridge between Orient and Occident, Armenian represents the furthermost flow of Indo-European speech from West to East. And this flow belongs to a relatively late period. Apart from Armenian we can discover no traces of Indo European occupation between Media and the Halys until the days when Iranian Ossetes settled in the Caucasus and the mountaineers of Kurdistan adopted Iranian dialects. I must re-iterate here what I have said many years ago: if there is one fact which the Assyrian monuments make

*According to Eustathios (*in Dion*, v. 694).

clear and indubitable, it is that up to the closing days of the Assyrian monarchy no Indo-European languages were spoken in the vast tract of civilized country which lay between Kurdistan and Western Asia Minor. South of the Caucasus they were unknown until the irruption of the Phrygians into Armenia. Among the multitudinous names of persons and localities belonging to this region which are recorded in the Assyrian inscriptions during a space of several centuries there is only one which bears upon it the Indo-European stamp. This is the name of the leader of the Kimmerians, a nomad tribe from the northeast which descended upon the frontiers of Assyria in the reign of Esor-haddon, and was driven by him into Asia Minor. The fact is made the more striking by the further fact that as soon as we clear the Kurdish ranges and enter Median territory, names of Indo-European origin meet us thick and fast. We can draw but one conclusion from these facts. Whether the Indo European languages of Europe migrated from Asia, or whether the converse were the case, the line of march must have been northward of the Caspian, through the inhospitable steppes of Tartary and over the snow-covered heights of the Ural Mountains.

An ingenious argument has lately been put forward, which at first sight seems to tell in favor of the Asiatic origin of Indo-European speech. Dr. Penka has drawn attention to the fact that several of the European languages agree in possessing the same word for "eel," and that whereas the eel abounds in the rivers and lakes of Scandinavia, it is unknown in those cold regions of Western Asia where, as we have seen, it has been proposed to place the cradle of the Indo-European family. But it is a curious fact that in Greek and Latin, and apparently also in Lithuanian, the word for "eel" is a diminutive derived from a word which denotes a snake or snake-like creature. This, it has been urged, may be interpreted to mean that the primeval habitat of the Indo-European languages was one where the snake was known, but the eel was not. The argument however cannot be pressed. We all agree that the first speakers of the Indo-European languages lived on the land, not on the water, and that they were herdsman rather than fishermen. Naturally therefore they would become acquainted with the snake before they became acquainted with the eel, however much it might abound in the rivers near them, and its resemblance to the snake would lend to it its name. In Keltic the eel is called a "water-snake," and to this day a prejudice against eating it on the ground that is a snake exists in Keltic districts. All we can infer from the diminutives *anguilla*, ἔγχελυς is that the Italians and Greeks in the first instance gave the name to the fresh-water eel, and not to the huge conger.

I can not now enter fully into the reasons which have led me gradually to give up my old belief in the Asiatic origin of the Indo-European tongues, and to subscribe to the views of those who would refer them to a northern European birth-place. The argument is a complicated one, and is necessarily of a cumulative character. The individual links

in the chain may not be strong, but collectively they afford that amount of probability which is all that we can hope to attain in historical research. Those who wish to study them may do so in Dr. Penka's work on the "*Herkunft der Arier*," published in 1886. His hypothesis that southern Scandinavia was the primitive "Aryan home" seems to me to have more in its favor than any other hypothesis on the subject which has as yet been put forward. It needs verification, it is true, but if it is sound the verification will not be long in coming. A more profound examination of Teutonic and Keltic mythology, a more exact knowledge of the words in the several Indo-European languages which are not of Indo-European orgin, and the progress of archæological discovery, will furnish the verification we need.

Meanwhile it must be allowed that the hypothesis has the countenance of history. Scandinavia, even before the sixth century, was characterized as the "manufactory of nations;"* and the voyages and settlements of the Norse Vikings offer a historical illustration of what the pre-historic migrations and settlements of the speakers of the Indo-European languages must have been. They differed from the latter only in being conducted by sea, whereas the pre-historic migrations followed the valleys of the great rivers. It was not until the age of the Roman Empire that the northern nations became acquainted with the sailing-boat; our English *sail* is the Latin *sagulum*, "the little cloak of the soldier," borrowed by the Teutons along with its name, and used to propel their boats in imitation of the sails of the Roman vessels. The introduction of the sail allowed the inhabitants of the Scandinavian "hive" to push boldly out to sea, and ushered in the era of Saxon pirates and Danish invasion.

Dr. Penka's arguments are partly anthropological, partly archæological. He shows that the Kelts and Teutons of Roman antiquity were the tall, blue-eyed, fair-haired, dolicho-cephalic race which is now being fast absorbed in Keltic lands by the older inhabitants of them. The typical Frenchman of to-day has but little in common with the typical Gaul of the age of Cæsar. The typical Gaul was, in fact, as much a conqueror in Gallia as he was in Galatia, or as modern researches have shown, as the typical Kelt was in Ireland. It seems to have been the same in Greece. Here too the golden-haired hero of art and song was a representative of the ruling class, of that military aristocracy which overthrew the early culture of the Peloponnese, and of whom tradition averred that it had come from the bleak North. Little trace of it now remains; it is rarely that the traveler can discover any longer the modern kinsfolk of the golden-haired Apollo or the blue-eyed Athênê.

If we would still find the ancient blonde race of Northern Europe in its purity we must go to Scandinavia. Here the prevailing type of the

* "Quasi officina gentium aut certe velut vagina nationum:" Jordanes, *De Getarum sive Gothorum origine*, ed. Closs, c. 4.

population is still that of the broad shouldered, long-headed blondes who served as models for the Dying Gladiator. And it is in southern Scandinavia alone that the pre-historic tumuli and burying-grounds yield hardly any other skeletons than those of the same tall dolichocephalic race which still inhabits the country. Elsewhere such skeletons are either wanting or else mixed with the remains of other races. It is therefore reasonable to conclude that it was from southern Scandinavia that those bands of hardy warriors originally emerged, who made their way southward and westward and even eastward, the Kelts of Galatia penetrating like the Phrygians before them into the heart of Asia Minor. The Norse migrations in later times were even more extensive, and what the Norse Vikings were able to achieve could have been achieved by their ancestors centuries before.

Now the Kelts and Teutons of the Roman age spoke Indo-European languages. It is more probable that the subject populations should have been compelled to learn the language of their conquerors than that the conquerors should have taken the trouble to learn the language of their serfs. We know at any rate that it was so in Ireland. Here the old "Iverniau" population adopted the language of the small band of Keltic invaders that settled in its midst. It is only where the conquered possess a higher civilization than the conquerors, above all, where they have a literature and an organized form of religion, that Franks will adapt their tongues to Latin speech, or Manchus learn to speak Chinese. Moreover in southern Scandinavia where we have archæological evidence that the tall blonde race was scarcely at any time in close contact with other races, it is hardly possible for it to have borrowed its language from some other people. The Indo-European languages still spoken in the country must, it would seem, be descended from languages spoken there from the earliest period to which the evidence of human occupation reaches back. The conclusion is obvious: Southern Scandinavia and the adjacent districts must be the first home and starting-point of the Western branch of the Indo-European family.

If we turn to the Eastern branch, we find that the farther east we go the fainter become the traces of the tall blonde race and the greater is the resemblance between the speakers of Indo-European languages and the native tribes. In the highlands of Persia, tall, long-headed blondes with blue eyes can still be met with, but as we approach the hot plains of India the type grows rarer and rarer until it ceases altogether. An Indo-European dialect must be spoken in India by a dark-skinned people before it can endure to the third and fourth generation. As we leave the frontiers of Europe behind us we lose sight of the race with which Dr. Penka's arguments would tend to connect the parent speech of the Indo-European family.

I can not now follow him in the interesting comparison he draws between the social condition of the southern Scandinavians as disclosed by the contents of the prehistoric "kitchen maidens," and the social

condition of the speakers of the Indo-European parent speech according to the sobered estimate of recent linguistic research. The resemblance is certainly very striking, though, on the other hand, it can not be denied that archæological science is still in its infancy, and that Dr. Penka too often assumes that a word common to the European languages belonged to the parent speech, an assumption which will not, of course, be admitted by his opponents.

What more nearly concerns us here however is the name we should give to the race or people who spoke the parent language. We can not call them Indo-Europeans; that would lead to endless ambiguities, while the term itself has already been appropriated in a linguistic sense. Dr. Penka has called them Aryans, and I can see no better title with which to endow them. The name is short; it has already been used in an ethnological as well as in a linguistic sense, and since our German friends have rejected it in its linguistic application it is open to every one to confine it to a purely ethnological meaning. I know that the author has protested against such an application of the term; but it is not the first time that a father has been robbed of his offspring, and he can not object to the robbery when it is committed in the cause of science. For some time past the name of Aryan has been without a definition, while the first speakers of the Indo-European parent speech have been vainly demanding a name; and the priests of anthropology cannot do better than to lead them to the font of science and there baptize them with the name of Aryan.

THE PRE-HISTORIC RACES OF ITALY.*

By Canon ISAAC TAYLOR.

Nowhere in the world is there such a mixture of races—such a *colluvies gentium*—as in Italy.

At the beginning of the historic period we find Siculi and Sicani in the south, Etruscans in the north, and in the center Umbrians, Latins, Sabines, and Samnites, all speaking Aryan languages. At a very early time the Carthaginians made good their footing in the west of Sicily, and the Greeks established colonies in the east. Southern Italy became Magna Græcia—so that the greater Greece lay beyond the Adriatic, just as the greater Britain now lies beyond the Atlantic. The Greeks pushed their trading posts as far as Cumæ in the Bay of Naples, and the Phœnicians established theirs at Cære, 20 miles from Rome.

In the fourth century B. C. the Gauls poured over the Alps into the plain of the Po, establishing a Gallia Cisalpina in the north answering to the Magna Græcia in the south.

And then, when the Roman legions had conquered Italy and the Eastern World, Rome herself was overrun by the peoples she had subdued. Rome became an oriental city. The Orontes, as a Roman writer complained, had emptied itself into the Tiber. A flood of Syrians, Jews, Greeks, Egyptians, Africans, Spaniards, Gauls, and Dacians—slaves, freedmen, or adventurers—poured into the Eternal City, making it a *cloaca maxima*—the universal sewer of the world. Then came the inroads of the northern hordes—Heruls, Goths, Vandals, Huns, and Lombards—who rushed in to appropriate the treasures which during four centuries had been plundered from Africa and Asia. Next came the inroads of Normans, Moors, Spaniards, French, and Germans, and lastly, the peaceable invasion of winter residents.

These are the races which, in historic times, have been added to the pre-historic peoples of the land.

At the beginning of the historic period we find the Etruscans established north of the Tiber, the Latins and other tribes speaking Aryan languages further to the south, and an earlier aboriginal population in the Apennines and Calabria.

In books written only 30 years ago the oldest civilization of Italy is attributed to a mysterious people, who are called the Pelasgi. We

* From *The Contemporary Review*, August, 1890, vol. LVIII, pp. 261–270.

hear of these Pelasgi in Greece as well as in Italy. Those megalithic structures which still excite our wonder—the walls of Mycenæ and Tiryns, as well as those of Cortona and Russellæ—are called Pelasgic. Cære and Cortona are said to have been Pelasgic cities prior to the Etruscan conquest. We must therefore begin by asking who were these Pelasgi. The modern doctrine, it is hardly needful to say, is that the word has no ethnological significance, the name Pelasgic being merely equivalent to "ancient" or "aboriginal." The term was a term of ignorance, like the word "natives" now applied to Polynesians, Patagonians, Red Indians, or Maoris. We may therefore leave the Pelasgians out of account; or rather, try and find out what races were grouped together by ancient writers under this convenient but delusive appellation.

What we may call "the ethnological horizon" has wonderfully widened of late years. For vast periods, for many millenniums, we are able to trace the history of man in Europe. He is now proved to have been the contemporary of the great extinct carnivora and pachyderms, and to have followed northward the retreating ice sheet of the last glacial epoch. The history of these primeval races has been traced by the tools and weapons which they have left, and by the shape and character of their skulls.

Archæologists have distinguished the successive ages of stone, bronze, and iron. The bronze age in Italy is believed to have commenced some 4,000 years ago. The stone age, which preceded it, is divided into two epochs, the Palæolithic age, or age of chipped flints, and the Neolithic age, when the flint implements were ground or polished. The Palæolithic people were utter savages, clad in skins, living in caves or rock shelters, making use of no fixed sepulchers, subsisting on shell fish or the products of the chase, ignorant of pottery, without bows and arrows, and armed merely with spears, tipped with flint, horn, or bone.

Skulls which are believed to be of Palæolithic age have been found in various parts of Italy—at Olmo, at Isola del Liri, at Mentone, and in some Sicilian caves. They are all dolichocephalic, or long skulls. Owing to the presence in their refuse heaps of human bones which seem to have been broken in order to extract the marrow, it is believed that these people occasionally practised cannibalism. But their chief food seems to have consisted of wild horses of a small breed, which then roamed over Europe in immense herds. Enormous refuse heaps, consisting mainly of the bones of horses, have been found outside the caves which were inhabited by this race. In the caves at the foot of Monte Pellegrino, near Palermo, the floor is formed by a magma of the bones of wild horses, which were either stalked with spears, driven by the hunters into pit-falls, or chased over the cliffs. Similar deposits have been found at the cave of Thäyngen, in Switzerland, and in front of the rock shelter at Solutré, near Macon, where there is a vast de-

posit, the relics of the feasts of these savages, nearly 10 feet in thickness and more than 300 feet in length, composed entirely of the bones of horses, and comprising the remains of from 20,000 to 40,000 individuals.

The Palæolithic period must have lasted for unnumbered millenniums. Archæologists conjecture that it came to an end some 20,000 years ago, when it was succeeded by the Neolithic period, which may have lasted for some 16,000 years. At the beginning of the Neolithic age, when regular sepulchers were first used, we find savages, who may probably be the descendants of the Palæolithic people, spread over western Europe. They were clad in skins, stitched together with bone needles. They wore bracelets of shells, and painted or tattooed their bodies with red oxide of iron. Broca considers that this early race is allied to the North African tribes, their language probably belonging to the Hamitic class, without inflexions and almost without grammar.

To us the chief interest of these people lies in the fact that their descendants may probably be traced in the present inhabitants of Sardinia and of southern Italy, as well as in some parts of the British Islands and of Spain. They are usually called the Iberian race. In the early Neolithic period we find skulls of the Iberian type all over western Europe, in Caithness, Yorkshire, Wales, and Somerset, in the south of France, in Spain and Italy. This race was swarthy, with olive complexion and black curly hair; it was orthognathous, leptorhinic, and highly dolichocephalic, with a low orbital index, and short stature, averaging about 5 feet 4 inches. Their present descendants are found in Donegal, Galway, and Kerry, in some of the Hebrides, in Denbighshire, and in the counties bordering on Wales. They are also to be recognized among the Spanish Basques, the Berbers, the Kabyles, the Guanches of Teneriffe, the Corsicans, the Sardinians, the Sicilians, and the people of southern Italy. Pausanius informs us that the Sardinians were Libyans, or what we should now call Berbers. Seneca says that Corsica was peopled by Iberians and Ligurians. Thucydides and Ephoros also inform us that the oldest inhabitants of Sicily were Iberians.

There are several pre-historic skulls of this race in the Kincherian Museum at Rome, and the Falerian skull in the Villa Papa Giulio belongs to the same type. These skulls are orthognathous and dolichocephalic, resembling the modern Sardinian skull and ancient Iberian skulls found in caves at Gibraltar and in Sicily.

This ancient type is still predominant in southern Italy, Sicily, Sardinia, and Corsica. Professor Calori, of Modena, has measured more than 2,400 skulls in different provinces of Italy. In southern Italy only 36 per cent. are round-headed, with a cephalic index* above 80; whereas

* The cephalic index gives the proportion of the breadth of the head to the length, and is obtained by dividing the breadth by the length from front to back, and then multiplying by 100.

in northern Italy the proportion is 87 per cent. In northern Italy less than 1 per cent. are of the extreme Sardinian type, with the index below 74; while in southern Italy 17 per cent. belong to this type. The difference of race, as shown by the difference in the shape of the skull, may account to some extent for the difference in the existing civilization in the north and south of the peninsula.

Early in the Neolithic age, before the reindeer had withdrawn from Belgium, another race makes its appearance in Europe. They were a round-headed people of short stature, with a mean cephalic index of about 84. We first find their remains in the sepulchral caves of Belgium and central France, whence they extended to Savoy and to the Rhætian and Maritime Alps. They manufactured rude pottery; their weapons were axes of flint, carefully chipped and roughly polished, and spears tipped with bone or horn. The skull is of the same shape as that of the Lapps, whom they resembled in their short stature. Their original speech is probably represented by the Basque, and a few of their words may be preserved in mountain names of the Alpine region, such as *Cima*, "a hill," which is seen in the name of Cimiez near Nice, of the Cima de Jazi, and of the Cevennes. They are designated as the Auvergnat, Rhætian, or Ligurian race.

In the early Neolithic period we find in Italy only these two races, the dolichocephalic, or long-headed, Iberian race, who are physically allied to the North African tribes, and the brachycephalic, or round-headed, Ligurian race, allied to the Lapps and Finns. These two races inhabited the same caves, together or in succession. Thus in a Neolithic cave at Monte Tignoso, near Livorno, two skulls were found, one of the Iberian type, with an index less than 71, and another of the Ligurian type, with an index of 92. In another Neolithic cave, called the Caverna della Matta, an Iberian skull was found with an index of 68, and a Ligurian skull with an index of 84. No anthropologist would admit that these skulls could have belonged to men of the same race.

We now come to the third Italian race, which may be called the Umbrian or Latin race. They spoke an Aryan language, and must be regarded as the ancestors of the Romans. They made their appearance in Europe at a much later time, probably not more than 6,000 or 7,000 years ago. They were taller and more powerful than either of the earlier races, and were orthocephalic, with an index of from 79 to 81. When we first meet with them, they are no longer mere savages, living solely by the chase, but are a pastoral people, who had domesticated the dog, the ox, and the sheep, and who had invented the canoe, and even the ox-wagon, in which they followed their herds over central Europe. They no longer, like the two earlier races, sheltered themselves in caves, but lived in huts made of boughs plastered with clay, and in winter in pit dwellings roofed with poles and twigs.

We can trace this race all over Central Europe. We find their remains in the round barrows of Britain, but more especially in the pile

dwellings which they erected in the lakes of Germany, Switzerland, and northern Italy.

From southern Germany they spread to western Switzerland, where we find the remains of their settlements in the lakes of Constance, Neufchâtel, Bienne, and Geneva. These Swiss settlements began in the stone age, but were in many cases continuously inhabited from the age of stone through the age of bronze, coming down, in a few cases, to the age of iron. We can trace these people advancing gradually in civilization, at first subsisting mainly on the chase of the stag and the wild boar, afterwards, as these beasts became scarce, depending more and more on their domesticated animals, the ox and the sheep, and gradually taming the goat, the pig, and the horse. At first we find them without cereals, and evidently ignorant of the rudest agriculture, laying up in earthen pipkins stores of acorns, hazel-nuts, and water-chestnuts; and then, after a time, growing barley, wheat, and flax, learning to spin and weave, to tan leather, and even to make boots. They are identified with the Helvetii, a Celtic people.

This race gradually extended itself to Italy, crossing the Alpine barrier either through Carniola or by one of the western passes, and occupying by degrees Venetia, Lombardy, and the Emilia, and finally, the whole valley of the Po.

When they first appear in Italy they were still in the stone age, and had domesticated the ox, but were ignorant of agriculture. Now the bronze age is believed to have begun in Italy not later than 1900 B. C., and therefore this Umbro Latin Aryan race must have entered Italy considerably more than two thousand years before the commencement of our era.

On arriving in Italy they built pile dwellings in the North Italian lakes, similar to the pile dwellings of Switzerland and southern Germany, and disclosing much the same stage of civilization. We cannot doubt that they belonged to the same race, and this is confirmed by the close connection between Celtic and Italic speech.

In Italy, as well as in Switzerland, the pile dwellings began in the age of stone and lasted down into the age of bronze. Many of the small lakes have been converted into peat-bogs, and in digging out the peat the remains of these settlements have been disclosed.

One of the settlements has been discovered in a peat moor at Mercurago, near Arona. This moor was formerly a shallow lake, in which a pile dwelling was built by some of the earliest settlers of the Umbro-Latin race. They had no knowledge of agriculture, but fed on hazel-nuts and wild cherries. They had rude pottery, and polished flint implements. A dug-out canoe, a disk of walnut wood, which had evidently formed the wheel of an ox-cart, and one bronze pin were found, showing that the settlement was not finally abandoned till the age of bronze had commenced.

Farther north, in the Lake of Varese, there are seven villages built

on piles, two of them large, with numerous huts, which might almost be called towns. One of these towns belongs entirely to the stone age, exhibiting no trace of metal, but with remains of the stag, ox, goat, and pig. The other was founded in the stone age, but survived into the age of bronze, a pin, a fish-hook, and two spear-heads, all of bronze, having been found.

Another large pile dwelling in the Lago de Garda, opposite Peschiera, was founded in the stone age, and was in continuous occupation through the age of copper to the age of bronze.

Perhaps the most instructive of these lake settlements is the pile dwelling in the Lake of Fimon, near Vicenza. It must have been founded very soon after the Umbrians first reached Italy, and was destroyed before they had passed from the pastoral to the agricultural stage of civilization. There are two successive relic-beds, separated by an interval, which shows that the earlier town was burned, and then, after a time, re-built. In the oldest bed there is no trace of agriculture, even of the rudest kind. The inhabitants lived chiefly by the chase, but had domesticated the ox and the sheep. The bones of the stag and the wild boar are extremely numerous, and these animals evidently formed the chief food of the people, the bones of the ox and the sheep being rare. Thère is no grain, and no cereals of any kind, but great stores of hazel-nuts have been found, together with water-chestnuts (*Trapa natans*), wild cherries, and stores of acorns. The acorns were roasted for food, as is proved by fragments adhering to earthen pipkins. Flint tools and rude pottery are found, but no trace of metal. The settlement was burned, and after a time re-built. The newer relic-bed contains numerous flint chips, and one bronze ax, showing that the age of metal had commenced. But the notable fact is, that at the time of this new settlement the people had passed from the hunting to the pastoral stage. Wild animals had now become scarce, bones of the stag are absent, and those of the wild boar are rare, but those of the ox and the sheep have become common. The agricultural stage had not however been reached when this second settlement was destroyed, the only farinaceous food being hazel-nuts, cornel, cherries, and acorns. The dwellings were round huts, built of wattle, and plastered with clay. The remains of a canoe have been found.

We learn therefore that when the Umbro-Latin people reached Italy they were ignorant of metals and of agriculture, living mainly by the chase, and on wild fruits, nuts, and acorns.

After the lakes at the foot of the Alps had been occupied, the population increased, and gradually extended itself southward, building pile dwellings in the marshes in the neighborhood of Mantua. The race next crossed the Po, erecting on dry land in the plain of the Emilia similar villages of pile dwellings, the remains of which are very numerous, and go by the name of *terre mare*. These *terre mare*, or "marl beds," are small knolls or elevations, rising a few feet above the plain,

and are most numerous in the provinces of Parma, Reggio, and Modena. They consist of beds of brownish or dark-colored earth, rich in phosphates and nitrates, and which are now used by the peasants for manuring their fields. They are plainly the refuse heaps or middens of ancient villages, which were pile dwellings erected on dry land. They vary from an acre to 3 or 4 acres in extent, and usually rise some 10 feet above the plain, resembling the Arab villages in Egypt, each standing on its *tell*, raised above the inundation. These knolls are composed solely of the refuse of habitation, of the bones of animals, and of broken pottery thrown out from the huts, which were built on platforms resting on piles. The lower strata of rubbish belong to the age of stone, while in many cases the upper strata belong to the age of bronze. They must have been occupied for many centuries, to allow of such vast accumulations of refuse. They were protected by a square earthen mound or rampart, surmounted by palisades, like a New Zealand *pah*.

These *terre mare*, of which nearly a hundred are known, disclose clearly the civilization of the first Aryan settlers in Italy, the ancestors of the Latin race. They made mats from the bark of the clematis; they knew how to prepare and to weave flax; they even obtained amber beads from the Baltic, but they possessed no swords, fibulæ, or rings. They had neither iron, gold, silver, nor glass. Bronze was cast, but not forged. We find strainers for preparing honey, and hand-mills or querns for grinding grain, but there is no sign of bread having been baked. The vine was cultivated, but the art of making wine had not been discovered. No idols of any kind have been found. Certain earthenware crescents, supposed at one time to have been symbols used for lunar worship, prove to be neck-rests, used for sleeping on the ground, so as to avoid disturbing the elaborate coiffure. The dwellings were merely huts of wattle and dab, no stone or mortar having been used in their construction. The people hunted the stag, the roe, and the wild boar, and kept dogs, oxen, sheep, goats, and pigs. They had no fowls. The ass was unknown, and it is doubtful whether they had tamed the horse. They had dishes perforated with holes, which were probably used for making cheese, but no fish-bones or fish-hooks have been found. They grew wheat, beans, and flax, and gathered wild apples, sloes, and cherries. Acorns were carefully preserved in jars for winter use.

These peaceful people must have inhabited the plain of the Po for at least a thousand years, probably for a much longer time, two or even three thousand years. They had advanced to the bronze age, and must be regarded as the ancestors of the Latins and the other Aryan tribes of Italy.

At some period in the bronze age they were suddenly overwhelmed by the invasion of the Etruscans, a fierce and savage race which broke in on them from the north. All their settlements were destroyed--not

one survived to the iron age, which probably commenced in Italy in the ninth or tenth century B. C. On other grounds it is believed that the Etruscan invasion was not later than the eleventh century B. C. We learn from Varro that the Etruscan era began 291 years before the Roman. The Roman era began in 753 B. C., and therefore the Etruscan era dates from 1044 B. C. But it is not likely that the Etruscan era began before the conquerors had settled down into an organized state—*duodecim populi Etruriæ*, or confederation of the twelve Etruscan tribes. We may therefore, with some probability, place the Etruscan invasion of Italy in the twelfth century B. C. It may not improbably be connected with the great movement of races about this period, which began with the conquest of Syria by the Hittites, and of Egypt by the Hyksos, and ended with the Thessalian and Dorian invasions of Greece, and that consequent emigration of the older Greek tribes to Asia Minor which lies at the base of the Homeric Epos. It is possible that the Etruscans may themselves have been an Asiatic people, akin to the Kheta and the Hyksos. This supposition derives support from the similarity in the appearance of the Hittites and the Etruscans as portrayed on their respective monuments, from the old tradition which connects the Etruscans with Asia Minor, and also from the recent discovery in Lemnos of inscriptions believed to be in a language of the Etruscan type.

After overwhelming the Umbrian settlements in the valley of the Po, the Etruscans extended their dominion across the Apennines to the Arno and the Tiber. It seems probable that the foundation of Rome was due to the Umbro-Latin fugitives, who placed the Tiber as a barrier between themselves and the invaders, establishing themselves on the Palatine, as their Etruscan foes did at Veii, 11 miles north of Rome. Just as the foundation of Venice is attributed to the fugitives from the invasion of Attila and the Huns, so the foundation of Rome may be due to fugitives from the invasion of the Etruscans. This is supported by the fact that the *terra mare* and the *palafitte*, which are believed to constitute the primitive settlements of the Umbro-Latin Aryan race, are not found south of the Apennines beyond the Emilia and the valley of the Po. The Etruscan dominion and civilization endured for some 700 years. At length it fell before the invasion of the Gauls in 400 B. C., just as the Umbrian civilization had fallen before the inroad of the Etruscan hordes. And thus Etruria Circumpadana, the former Umbrian land, became cisalpine Gaul, its possession reverting to a people who in race and language were nearly akin to its former inhabitants.

The settlements of the Gauls are recognized by the torques and the long iron swords which are found in their graves. At Bologna, in the cemeteries of the Certosa and Marzabotto, we have the tombs of the three successive races, Umbrians, Etruscans, and Gauls, all different in character, and easily to be distinguished.

Thus it appears that the fertile plain of the Po was occupied by many successive races, whose descendants may, with greater or less certainty,

be recognized in the present population of Italy. We have first the Palæolithic Iberian savages, mere hunters and probably cannibals, living in caves, ignorant of pottery, whose descendants may be traced in Sardinia and Southern Italy. They were followed, in the early Neolithic period, by the Ligurians, possessed of pottery, but without domestic animals. Their descendants now occupy the Rhætian and Maritime Alps. They were succeeded towards the close of the Neolithic age by the Umbro-Latin race, who lived in huts and pile dwellings instead of caves, who possessed oxen and sheep, canoes and wagons, and who gradually acquired a knowledge of bronze. In the bronze age, sometime before the middle of the eleventh century B. C., they were overwhelmed by the Etruscan inroad, their villages were destroyed, and they fled southward from the invaders. Then, at the close of the fifth century B. C., the Etruscan dominion was destroyed by the Boii and other Gaulish tribes, who were in the iron stage of civilization. Finally came the conquest of the Romans, and afterwards those of the Heruls, Goths, Huns, and Lombards.

The people who lived in the pile dwellings in the valley of the Po, and who are usually called Umbrians, were clearly of the same race as the ancient Romans. The skull is of the same shape, the type of civilization was the same, and Latin and Umbrian were merely dialects of the same language.

Owing to the practice of cremation genuine Roman skulls are rare, and of skulls ostensibly Roman many turn out to be those of freedmen or provincials. But, judging from the few we possess, the shape of the head was almost identical with that of the Umbrians, of the Swiss lacustrine people, and of the Celtic round barrow race of Britain. The great breadth of the Roman skull is well seen in the portrait busts of Tiberius, Nero, Vespasian, Titus, and Marcus Aurelius.

That the Romans were originally in the same pastoral stage of civilization as the Umbrians is shown by the fact that the words for money and property, *pecunia* and *peculium*, are derived from *pecus*, cattle; while the ox, which appears on some early Roman coins, may indicate the fact that the ox was the standard of pecuniary value. The hut urns found in the ancient cemetery of Alba Longa show that the Latins at first lived in huts like those of the Umbrians. The *ædes Vestæ* in the Forum, the most venerable relic of early Rome, was originally a hut of wickerwork and straw, and so was the *càsa Romuli* on the Palatine.

The population of Italy has now become so mixed that in many provinces it is difficult to detect and separate the original elements. But the Sardinians and the peasants of Southern Italy still display the primitive Iberian type, and the Greek type survives on the sites of some of the old Greek colonies. For instance, at Naxos and Syracuse about 24 per cent. of the people have blue eyes, while at Palermo, which was never a Greek city, the proportion is less than 1 per cent. In some

parts of Lombardy Teutonic village names are numerous, and Teutonic names, of Gothic or Lombard origin, are common among the nobility. Filiberto, Humberto, and Garibaldi are genuine Teutonic names; so also is that of the Italian seaman, Amerigo Vespucci, who bore the Gothic and Lombardic name of Amaric, which he has given to the New World.

It is curious that America, the continent which has become the patrimony shared nearly equally by the Teutonic and Latin races, should itself bear a Teutonic name, whose Latinized form bears indisputable witness to the Teutonic conquest of the oldest seat of the Latin race in Italy.

THE AGE OF BRONZE IN EGYPT.*

BY OSCAR MONTÉLIUS.

It is generally admitted that bronze was known and made use of in Egypt from the earliest times of the ancient empire, about 6,000 years ago:† but authors do not agree so well in respect to iron. The majority affirm that this metal was also known in the valley of the Nile at an epoch not less remote. Very strong reasons however appear to me to demonstrate that it was in the second millenium before the Christian era, that the use of iron possessed in Egypt an importance that authorizes us to speak of an age of iron in that country. The greater part of the time, then, that embraces Egyptian civilization should be considered as an age of bronze.

Assuredly to the majority of persons this conclusion will appear unexpected, if not absurd. Egyptian civilization, in fact, during the period mentioned was eminent to a degree that can scarcely be believed possible without an acquaintance with iron. But we must not forget that the Aztecs in Mexico, with their civilization largely developed, were still living in a pure age of bronze on the first arrival of Europeans. The most important and almost note-worthy reason that has been cited to establish the age of iron in Egypt several thousand years before the Christian era is that at this remote epoch massive edifices were already erected there in wrought stone, and that this stone is so hard that it can be cut only with implements of iron, or rather of steel. The celebrated German Egyptologist, Mr. Lepsius, affirms: "Great masses of carved granite, certain specimens of which are met with from the fourth dynasty of Manéthon, do not permit us to question that iron was known at that era."‡ On the other hand it has been established by

* Translated from *L'Anthropologie*, January, 1890, vol. I, p. 25.

† Perrott and Chipiez: *Histoire de l'Art l'antiquité*, vol. I, Égypte, p. 829. Arcelin: *Influence Egyptienne pendant l'âge du bronze*, in the *Materiaux pour l'histoire de l'homme*, 1869, p. 377. In making deep drills in Egypt "a copper knife" was discovered at a depth of 24 feet (Mook, *Ægyptens vormetallische zeit*, p. 5). The British Museum possessed a few axes of bronze with descriptions from the time of the sixth dynasty, or about the middle of the third millenium before the Christian era *A Guide to the Egyptian Rooms* (in the British museum), p. 48.

‡ Lepsius, *Les Metaux dans les Inscriptions Egyptiennes*, translated from the German by W. Berend, Paris, 1887, p. 57.

special experiments that metal instruments are not required in order to carve a stone as hard as that of Egyptian edifices. Stone implements may be employed, although in this manner the work progresses very slowly, and requires a great deal of patience.*

But the Egyptians had had occasion to exercise patience, and every work can be accelerated by a multiplication of the forces put in operation. The Egyptian kings in their enterprises of construction were not accustomed to spare their laborers. Moreover it must be noted that Egyptian granite is so hard that our best steel instruments are soon ruined when one attempts to work with them. The fine figures which are found on Egyptian monuments, and especially the hieroglyphics, may rather be designated as engraved than carved. "It is in no wise improbable," says the English antiquarian, Mr. Wilkinson, who interested himself very much in the ancient Egyptains, "that they were familiar with the use of emory at the time when that substance, which is met with in the islands of Archipelago, was accessible to them; and if this be admitted we can explain the perfection and admirable delicacy of the hieroglyphics upon the monuments of granite and basalt. We then also comprehend why implements of bronze will be preferred to those of steel, which are harder and denser; for it is evident that emery powder will be incrusted upon the former and that its action on stone becomes greater in proportion to the quantity fixed on the sharp edge of the chisel; in our times, with the same view, we prefer soft iron tools to those of hard steel."

It is probable that sand—or emery, if they really possessed it—was used in the sawing of stone. We can thus more easily explain why verdigris has been sometimes observed in the quarries upon places where fragments of the rock have been detached by much sawing.†

The proof that bronze implements were employed by Egyptians for stone work is given by a Grecian author, Agatharchides, who lived about a hundred years before the birth of Jesus Christ. He relates that in his time bronze tools had been found in the gold mines in Egypt which had formerly been used by the mining laborers. He explains the utilization of bronze very correctly in stating that iron was entirely unknown at the time when the first operations in mining were begun.‡

Upon the monuments in the time of the ancient empire we sometimes see representations of men who are carving stone by the instrumentality of chisels, whose yellow or reddish-brown color shows that they were of bronze.§

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* Soldi, *Les Arts méconnus*, Paris, 1881, p. 492. Perrot Chipiez, *ouv. cit.* I, p. 755.

† Wilkinson: "Manners and Customs of the Ancient Egyptians," 1st edition, vol. III, pp. 250, 251.

‡ Evans: "Ancient Stone Implements of Great Britain," p. 6.

§ Rosellini: *Monumenti dell' Egittoc della Nubia* (*Monumenti civili* PL. XLVIII.) One of these chisels is not bluish, as has been indicated (Rhind: *Thebes, its Tombs and their Tenants*, p. 222), but reddish brown.

At Thebes, in the midst of the waste from the carving of stones, Wilkinson found a large bronze chisel which evidently had been forgotten by the citizens thousands of years ago.* This chisel, 22 centimeters in length, presents at the upper extremity very clear marks of blows from a hammer, but the edge is so intact that it appears new. It would soon have been destroyed if workmen little accustomed to such implements had endeavored to cut with it a stone similar to that which it shaped in other days.

That the Egyptians, by means of their bronze implements, could have been able to produce what they have made, undoubtedly does not depend, as has been supposed, upon the fact that they were in possession of the secret talent, hid for so long a time, of tempering bronze, but only that they had the skill acquired by long practice of using their utensils, a skill that we no longer possess, being accustomed to other instruments. It can not be denied that the manner in which the stones of Egyptian monuments are cut, presents a great analogy with the fabrication of pre historic tools of stone and the sockets of their handles. Even lately it was supposed that these tools and their sockets could not have been fabricated without the aid of steel instruments. This view was sustained until experiments had placed beyond all dispute the fact that by using stone, bone, or wood solely, such tools could be made and perforated, provided that the necessary skill and time were bestowed on this work.

Imposing edifices of hard stone, richly adorned with reliefs, may be constructed without iron. Proof of this is furnished by Mexico and Central America, which are rich in monuments of this kind anterior to Columbus and to the introduction of iron into those countries by Europeans. One cannot therefore rely upon the fact that the construction and embellishment of the stately edifices of the ancient empire are impossible without steel, to maintain that the age of iron commenced in Egypt at that distant period.

We must then fix the epoch of the introduction of the age of iron into Egypt by the same method which has so well succeeded in other countries. This problem attracted too late the attention of the Egyptologists. The greater portion of the discoveries that are pertinent to this question were not therefore investigated as they should have been. It will be seen presently however that the documents are numerous and clear, and that the paintings especially instruct us with very great exactitude.

It is necessary to examine the facts by grouping them under four heads:

(1) What are the objects in iron discovered in Egypt which date from the most remote era, and of what character are they?

*Wilkinson: "Manners and Customs of the Ancient Egyptians, vol. III," pp. 249, 252, and 253.

(2) What are the most ancient inscriptions in which iron is mentioned? Can we be fully enlightened through them in respect to the signification of the hieroglyphics which are supposed to designate iron?

(3) What are the most antique monuments representing arms and instruments of iron?

(4) Up to what epoch did they continue in Egypt to employ arms and instruments of bronze? Can we perceive upon these objects marks left as a consequence of long usage, whether in the reparation of the sharp edges, or otherwise, proving that they were used, and that they were not fabricated solely for the tomb?

To the first interrogatory it is easy to respond: Fragments of iron instruments have been found in a few pyramids; and if they date from the time of these mausoleums they fully establish the great antiquity of iron in Egypt. The best known of these fragments is the one discovered by an Englishman, Mr. Hill, in 1837, in the great pyramid at Gizeh, built about 3,000 years before Christ. It is supposed that it was a fragment of an instrument with which the surface of hewn stone was polished, but it is also believed that it is not of steel, but of iron. It may have been discovered near the orifice of one of the narrow atmospheric canals which traverse the body of the pyramid as far as the mortuary cavern, and in articulation of the stones, but not until after the two layers of exterior blocks forming the cap of the pyramid has been removed. No fissure was observed or aperture of any description, through which this iron after the construction of the pyramid might have been introduced at the point where it was found. For this reason several persons, having explored this locality immediately after the discovery, have publicly attested their conviction that the fragment of iron had been left between the stones during the construction of the pyramid, and that it could not have been inserted there after this period.*

Similar discoveries have been made more recently. Thus M. Maspero, in 1882, collected several parts of iron hoes in the black pyramid at Aboukir, probably built during the sixth dynasty; that is to say, in the third millenium before the Christian era. He discovered, moreover, a few fragments of iron instruments in the mortar between two stones, in a pyramid in the vicinity of Esneh.† This pyramid is not anterior to the seventeenth dynasty, and its construction consequently immediately preceded the inauguration of the New Empire. Mr. Maspero, as I believe, has given no information more precise in regard to the situation and the bearing of these fragments of iron.

Reasons that we are about to assign authorize us to doubt the conclusions which have been drawn from these discoveries. The presence of these iron fragments is certain; but are we equally assured that they date from the erection of the monuments that contained them? The

* Vyse, "Pyramids of Giseh," I, pp. 275, 276. Transactions of the second session of the International Congress of Orientalists, held in London, 1874, pp. 396–399.

† Maspero, *Guide du Visiteur au Musée de Boulaq* (Paris, 1884), p. 295.

points at which they have been met with—have they not been accessible to man from time to time during the thousands of years that have followed the construction?*

Can it be affirmed that the layers of stone under which they were lying were intact, and that the blocks had never been displaced and afterwards restored to their place? In our days these blocks have been removed without any harm being done to the solidity of the edifice. The circumstances of the bearings do not extinguish all possibility of their introduction at an epoch more or less late, and it does not seem that we are justified in drawing from these discoveries the conclusion that iron was already known and employed by the Egyptians 3,000 years B. C.† One has so much the less the right to an opposite conclusion, as we are about to see, from all that is known elsewhere concerning the epoch when iron was used, not only in Egypt, but even in other countries.

Lepsius, who supposes that iron was in use in Egypt from the fourth century,‡ is obliged to avow that in Egyptian tombs, until now, few objects in iron have been found, and that these objects are some of an uncertain era, others recent. This declaration of one of the most eminent scholars in Egyptian antiquity was made, it is true, 12 years ago, but, more recently, in the special circle of Egyptologists doubts have been manifested concerning the antiquity of iron in Egypt.§

Incontestable proofs of the existence of iron before the epoch of the new empire, that is to say, before the middle of the second millenium, B. C., have not been produced. Having maintained as evident that the Egyptians were in possession of iron at an epoch far more remote, they have meanwhile tried to explain the absence of this metal in the most ancient Necropolises and Mausoleums by invoking a religious prejudice.

Iron was regarded as the bone of Typhon, the enemy of Osiris, and for this reason considered impure; one could not make use of it even for the most ordinary requirements of life without polluting his soul in a way that would cause him harm both on earth and in the other world. Meanwhile, Mr. Maspero, one of the most eminent Egyptologists, has demonstrated that this explanation is not satisfactory, for

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* Vyse says ("Pyramids of Gizeh," I, p. 4) that the mouth of the atmospheric canal in question is found partly enlarged before he began his labors there.

† The scruples I have expressed in respect to the discovery of iron in the great pyramid are not now presented for the first time; compare the work of Rhind, published in 1862, Thebes, its Tombs and their Tenants, p. 227.

‡ Lepsius, *Les Metaux*, p. 54.

§ In the official guide, printed in 1879, for the use of the visitors to the British Museum (*A Guide to the Egyptian Rooms*, p. 40) we read: "It is doubtful if the use of iron was known at a very remote period." In the same way people expressed themselves later, in 1884. See Journal of the Anthropological Institute of Great Britain and Ireland, session of March 25, 1884. Compare, also, Perrot and Chipiez, *Histoire de l'Art*, etc., vol. I (printed 1882), pp. 753, 754, and 830.

he says (p. 295), "The religious impurity of an object has never sufficed to prevent the use of such object. To cite but a single example, pork also was dedicated to Typhon and considered impure; they were bred however in droves, and the number of these animals was considerable enough, at least in certain cantons, to allow the good HerodItus to relate that they were let loose in the fields after the harvests in order to press down the earth and bury the grain. Besides, in Egypt each individual object was not exclusively pure or impure, but sometimes one, sometimes another, according to circumstances. It is thus that the boar and the sow, despite their Typhonian character, were the animals of Isis, and consequently share the Osirien purity. Iron, which certain traditions call the bone of Typhon, is commonly called "bonipit," the substance of heaven; it is hence pure in certain aspects, and impure in certain others."

Religious scruples had not placed any obstacle to the employment of iron in Egypt at the epoch when this useful metal was really known, for divers iron instruments have been found in contemporaneous tombs. Many of these objects have been deposited in the museum of the Louvre; they are all probably posterior to the fifteenth century B. C. and very near to that date. The most ancient—if we do not consider the fragments already mentioned which come from the pyramids—that are known in Egypt, and the age of which can be established, is a curved blade resembling a reaping hook, which Belzoni one day put under one of the Sphinxes at Karnak, but the age of this blade does not go beyond the seventh century B. C.* Maspero, who supposes the use of iron in Egypt to be very ancient, has endeavored in two instances to find an explanation of the rarity of this metal. He thinks, in the first place, that iron utensils which could not be employed have been remelted. But this does not explain to us the reason why in Egyptian museums objects in iron are more rare than objects in bronze. The recasting of bronzes out of use was at least practiced as much.

In the second place, Mr. Maspero, and with him many others, have desired to explain the absence of iron by arguing its destruction by rust. But it is necessary to recall that Egyptian tombs are so dry that here, less than anywhere else, could iron have been corroded by oxidation. Besides, rust has not the consuming activity which is sometimes believed. A great deal of time is required to accomplish its work of destruction; and in point of fact this rust could not entirely disappear.

* Day: *Prehistoric Use of Iron and Steel*, p. 14. Compare *Journal of the Anthropological Institute*,, March 25, 1883. The authenticity of this discovery is questioned by Rhind (*Thebes*, p. 228). An iron chisel was found under an obelisk at Karnak, which should date from the eighteenth dynasty (Arcelin, in the *Materiaux*, 1869, p. 377), but the determination of the age of the chisel is perhaps questionable. If it were even correct, the discovery nevertheless is posterior to the beginning of the New Empire.

In the tombs the rusted object, or the trace of the rust upon neighboring objects, would have been discovered.*

Now never in my knowledge has a vestige of iron been found more or less rusted, never even has the stain of rust been found in the tombs prior to the fifteenth century B. C.

This is all the more important as among the discoveries of the first millenium B. C. both in Egypt and elsewhere, numerous iron objects, among which are several well preserved, have been met with (Rhind, *Thebes*, p. 218). Now, if an object made of iron can be preserved almost intact during two or three thousand years, there is no reason why this object should have disappeared without leaving any trace if it had remained a little longer in the earth and under identical conditions.

Inasmuch as to-day almost all the hieroglyphic inscriptions can be read without difficulty, it might be supposed that it was easy to respond to the second inquiry above made and specify the group of characters that constitute the name of iron. Now, erudite men differ in this particular; with one it is such a term, with a second another.

It does not appertain to me, who am not an Egyptologist, to examine if the various and contradictory opinions do not proceed from the fact that some have imagined they discovered the word "iron" in inscriptions of an epoch when this metal was as yet unknown.

I do not know, further, if the group of hieroglyphics which is reputed to signify the term iron on a recent occasion referred to has the same signification in the inscriptions of the New Empire. It does not suffice that the word exists; it is necessary to prove also that this term at a remote period did not signify anything else; and for example, that there was instead of the meaning iron the meaning bronze or some other metal in general.

A striking example of such a change of signification is the following: In India in the early era of the Vedas *ayas*, designated bronze, and then, after the introduction of iron it was applied to the new metal. The Latin has preserved the primitive sense of the word *æs*. The problem is very essentially cleared up if recourse be had to another catagory of instruction: this is our third standpoint.

Among the mural paintings, so numerous and so often admirably preserved, there are a great many arms and modelled instruments, the greater part red or yellow, the rest blue. Surely one will not deny that the red and the yellow represents copper and bronze and the blue iron or steel.

In a tomb comparatively modern, that of Rameses III, some arms are red, others blue. Rhind (*Thebes*, p. 221) has erroneously drawn from

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*Lepsius: *Les métaux*, pp. 52 and following. He says (p. 63) that the iron has not yet been found represented under his name. Perrot and Chipiez: *Histoire de l'art*, vol. I, p. 753; Chabas: *Sur le nom de fer chez les anciens Égyptiens* in the *Comptes rendus* de l'Académie des Inscriptions (January 23, 1874). Brugsch: *Hieroglyphisch-Demotisches Wörterbuch*, vol. V, p. 413, and following.

this the conclusion that colors are without signification. This fact demonstrates only that at that epoch some arms of bronze and others of iron were employed, although the latter metal had then been long known.

When we examine attentively the paintings of ancient times we observe that arms and tools are painted there red or yellow, never blue. Lepsius, who believes in the antiquity of iron in Egypt, is nevertheless very much surprised at the fact (p. 57) that red or light brown is employed in the re-production of axes, arrow barbs, pruning hooks, saws, chisels razors, and butcher knives.

It is in the paintings of the new empire alone that metallic objects are painted blue. This can be naturally explained. Bronze, until towards the fifteenth century B. C., was employed only for the fabrication of arms and instruments; iron was not as yet in use. Let us examine, finally, the last aspect of the question. The absence of objects made of iron in the mortuary furniture of the ancient era could have no signification or importance if those of bronze were equally wanting. But it is not thus. Bronzes are met with abundantly in the tombs.

We can now, thanks to the latter, approach the fourth of our queries. Among the most remarkable discoveries of bronzes anterior to the new empire, or contemporaneous with the early centuries of that era, we may cite that of Drah-aboul-Neggah, to the north of Thebes. In 1860, some Arabs exhumed from the sand a coffin, that of Queen A'hhotep. This queen had been married to Kamos, a king of the seventeenth dynasty, and perhaps the mother of King Ahinos I, or of his consort Nofirtari. King Ahinos was the first of the eighteenth dynasty. A'hhotep, consequently was living more than 1,500 years B. C. Her coffin contained a large number of precious objects and arms, with which the museum of Boula is enriched and which we are about to describe.* There was gold, silver, bronze, but no trace of iron.

Arms and bronze instruments were in use at a later period and concurrently with those of iron. This is proven by numerous bronzes in the Boulaq Museum and the European museums which bear the name of Thoutmos III. This king of the eighteenth dynasty lived during the first half of the seventeenth century B. C. If one has carefully read the group of hieroglyphics which it is assumed constitutes the term iron, they were acquainted with that metal at that epoch. There are also bronzes which bear the name of Queen Hatschopsitu, the sister and coregent of Thoutmos III. The inscriptions that bear these names are engraved or written with ink on the bronze itself or on the wooden handles of the tools (Figs. 29 and 39). It is proper to note that on sev-

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*The discovery is described by Mariette in *Notice des principaux monuments du musée d'antiquités égyptiennes à Boulaq.* (Second edition, Alexandria, 1868, pp. 257–267), and by Maspero in his *Guide du visiteur au Musée de Boulaq*, Paris, 1884, pp. 77–83 and 320. Compare Perrot and Chipiez, *Histoire de l'Art* vol. I, p. 297, and Erman, *Ægyptien*, p. 612. The discovery is represented in the *Revue de l'Architecture*, 1860.

eral of these objects the same inscription is seen, with insignificant changes "The gracious God Ra-men-Kheper (prænomen of Thoutmos III), the beloved of Ammon, when the cord was stretched at Amen-saraku." It is supposed that it relates to the foundation of a "pylon" which Thoutmos caused to be built at Karnak.*

The fact that upon so many objects we meet with the name of this king would indicate that this name is of frequent occurrence in Egyptian inscriptions. This however may be the effect of accident which on a single day exposed an exceptional number of these objects; thus in a tomb at Thebes were discovered several baskets filled with instruments of this character.†

It has been supposed that the arms and tools exhumed from the tombs had been specially fabricated of bronze to be employed at ceremonies either solemn—for example, the foundation of a "pylone," as we have just seen—or funereal.‡

The bronze from the considerations of religious orders would have continued to be utilized long after industry might have manufactured arms of iron and tools for ordinary use. But this supposition is contradicted. Since a great number of these bronzes bear evident marks of long use, they were not fabricated, consequently in order simply to be deposited in the tombs.§ At the close of the second millenium B. C. arms of bronze were not yet entirely replaced by iron arms. The mural paintings in the tomb of King Ramses III at Thebes, which date from the twelfth century, prove this; here a great quantity of arms may be seen, the major part blue, the remainder red. Lance-barbs and swords with two edges are sometimes red, at others blue.‖

The arms represented in this tomb were those which were used in war at the time of Ramses III. It cannot therefore be assumed that the red arms were of bronze, because they were especially fabricated for the tomb. They are painted red because bronze arms were then in

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* *Sitzungsberichte der königl. Preussichen Academie der Wissenchaften zu Berlin*, 1888, vol. XXXIV, p. 770.

† Bronzes with the name of Thoutmos III, or that of Hatschopsitu are deposited in the Boulaq museum (an ax, a chisel, two blades of a saw, etc.); Maspero, *Guide*, etc., pp. 297–299); the British Museum (three axes and a couple of saws); *a Guide to the Egyptian rooms*, p. 42; the museum at Leyden (two axes, one saw, etc., Leemans *Mon. Egypt du Musée de Leide*, pl. 80, fig. 3 and pl. 90, figs. 157, 159, Chabas, *Etudes sur l'antiquité historique*, pp. 76–79; the collection of the Duke of Northumberland at Alnwick Castle (ax, two chisels, a drill, a saw-blade) Birch, *Catalogue of the Collection of Egyptian Antiquities at Alnwick Castle*, p. 200, pl. B.

‡ Birch, *Catalogue*, p. 200. The same inscriptions are duplicated also upon the bronzes that belong to the collection at Alnwick Castle.

§ The celebrated Swedish Egyptologist, M. Piehl, whose attention I called to the importance of this question before one of his visits to Egypt, had the kindness to write me that a considerable number of arms and bronze instruments, preserved in the Boulaq museum, had evidently been long in use, as is demonstrated by the fact that they were used and re-sharpened again and again.

‖ Champollion *Monuments de l'Egypt*, pl. 263–264. Rosellini, *Monumenti Civili*, pl. 121; Lepsius *Les Meteux*, p. 117, and pl. 11, fig. 2–7.

general use. It is very important to establish that bronze arms were common in the second century, and even that they were in a majority at this moment, which is already an iron age. Such would not have been the case if (as many authors suppose) iron had been known and employed in this country for thousands of years. This is an observation of which the value will be apprehended. If iron had been in the service of industry from the early dynasties it would not be found so rare still towards the second century. Everywhere else it is agreed that when iron appears, bronze is not long in yielding place to it for the fabrication of swords, axes, knives, etc. The experience acquired everywhere on this subject does not permit us to doubt that it would not have been otherwise on Egyptian soil, or that bronze, in despite of the presence of iron, would have remained so long alone or almost exclusively utilized.

It seems to result from the discoveries of Schliemann at Mycene and Tiryns, and which belong to the close of the second millenium B. C., that iron was not known in Egypt as early as has been asserted. Amongst so many objects of every variety which have been collected in the tombs of Mycene, there is no trace of iron, whilst hundreds of swords and other arms are of bronze. In the royal palace at Tiryns there is no iron or trace of iron.*

Now, the antiquities of these two cities testify to a powerful influence from Egypt, undoubtedly exercised through the agency of the Phœnicians, and it would then be scarcely possible that iron should have been completely unknown in Greece, if for two thousand years it had already been known in Egypt.

From what we have just said it follows with great probability that the Egyptians, during the whole time of the ancient empire, and probably until almost fifteen hundred years B. C., were not acquainted with the use of iron, and employed only bronze for their arms and instruments; that the age of bronze consequently continued in Egypt until the epoch mentioned, and that iron, as yet, towards the close of the second millenium B. C., *had not altogether replaced bronze for the construction of arms and edged instruments.*

The most remarkable discovery in Egypt of bronze arms is, as we have already said, that which was made in the coffin of Queen A'hhotep. Among the great quantity of precious things which this tomb contained we first mentioned were the following objects, which constituted a part of the toilet of that princess: Several gold bracelets, ornamented with precious stones and plates of glass, rings for the legs of gold, a golden chain, a diadem, a large collar and a decoration for the breast of gold

* The lyrics of Homer speak sometimes of iron, but these songs were probably not composed until long after the epoch of the Trojan war, and certainly they were not written in the condition in which we now have them. They cannot then be in testimony of the knowledge of iron in Greece at the time of Agamemnon or of Ulysses.

and precious stones, the handle of a fan of wood laminated with gold, an ebony mirror of gold and gilded bronze, etc. But along with these were found in the tomb of the queen various arms and a small boat of massive gold mounted upon a wooden chariot with wheels of bronze and a similar boat of silver. In the golden boat twelve oarsmen are seen, also of gold, who are rowing under the orders of the helmsman and pilot in the prow. In the center of the boat a diminutive personage holds an ax and a baton of authority; a cartouche engraved behind the helmsman teaches us the death for which he was originally predestined. This boat is King Kamos. The vessel itself is a symbol of the craft on which the deceased must embark according to the creed of the Egyptians, and be borne to Abydos in order to enter the other world.

Upon a few other objects found in this tomb may be read the names of the kings, Kamos, Ahmos, and the prenomen of the latter, Nibpehtiri.

The arms found in the tomb are of great importance to our subject. They are three poniards with blades of bronze and gold; two axes, one of gilded bronze, the other of silver; nine small hatchets, three of gold and six of silver; and a baton of authority, made of black wood and gold.

The figures that we are about to refer to are grouped upon the plates apart from the text here subjoined. One of the poniards was originally sheathed in a scabbard of gold.* The handle is of wood, and ornamented with small triangles in cornelian, lapis-lazuli, feldspar and gold forming the reverse. For the pommel, four female heads in pricked gold; an inverted bull's head conceals the soldering of the blade to the handle. The body of the blade is of dark bronze, inlaid with massive gold and damascened. Upon the upper face above the prenomen Nibpehtiri a lion is pursuing a bull, in advance of which two locusts are quietly proceeding. The lower facet bears the name of Ahmos I and fifteen flowers in full bloom which issue one from another and disappear toward the point of the blade. Another poniard (Fig. 18) has a gold handle, the blade being of bronze. The third poniard (Fig. 11) is formed with a very heavy blade, and a disk of silver serving as a handle. Figure 12 exhibits the poniard from a side point of view.†

One of the large axes is represented in Fig. 26. The handle is of cedar wood, ornamented with a golden leaf. The name of the king, Ahmos, is here traced in incrustations of lapis-lazuli, cornelian, turquoise, and green feldspar. The blade is provided with a simple handle

* The figures which here represent the objects preserved in the museum at Boulaq are executed after photographs which, through the kind instrumentality of M. Piehl and M. Brugsch Bey, were executed for me. The description of the sumptuous arms in the tomb of A'hhotep are taken from the *Guide du Visiteur au Musée de Boulaq*, by Maspero, pp. 79–83. Compare Erman, *Ægypten*, p. 612.

† Fig. 12 is designed from Fig. 564 in the first volume of Perrot and Chipiez, *ouvr. cit.*, where it is exactly indicated as representing a pin.

of wood and maintained in its socket by a coil of gold thread. It is of black bronze and has been gilded.

One of its facets bears lotuses on a ground of gold; the other represents Ahmos threatening with his axe a barbarian, half overthrown, whom he is holding by the hair of his head. Above this scene is represented the god of war, Montou Thebain, under the form of a griffin with the head of an eagle.*

The other axe is of the same form, the handle being of horn garnished with gold, the blade being of silver. Among the bronze axes found in Egypt with which I am acquainted none is perforated in the same way as the axes used in our days. All are of the same form as the wedges of bronze so common during the age of bronze in Europe, and are fastened to the handles by thongs or other bands.

All the Egyptian axes that I have had an opportunity to see at the Louvre and in other collections have been flat wedges without any traces of elevation along the borders, "straight borders" † without shoulders near the middle portion, to prevent the blade from entering into the handle when one struck with it.

The blades are either nearly of the same form as the axes of stone (Fig. 36) or else somewhat enlarged at the edge. The upper portion of several have a form characteristic of Egyptian axes (Fig. 28). It is rectilinear and prolonged into a point toward the two extremities.

There were however other forms of bronze axes, besides. Among the re-productions which date from the early era of the ancient empire, axes with a half circular blade are to be seen, as in Fig. 31. This blade is massive; but later on, towards the close of the ancient empire, the blade has very often the form that is shown in Fig. 32, with two round holes near the handle. ‡ The arm represented by Fig. 33 has a similar blade with two holes, only more elongated; the surface of the handle is of silver. §

Sometimes the axe blades are pierced through and through like that

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* According to Erman (*Ægypten*, p. 612) the middle of this facet is covered with blue enamel of the very deepest shade.

† In *Materiaux pour l'Hist de l'Homme*, 1869, pl. 19, Fig. 3, an Egytian axe is represented which, according to p. 378, should have straight borders, but the designs of the plate referred to are not sufficiently exact to draw any conclusions. I have written to the museum at Boulaq, where the figured axe should have been deposited, to inquire about it, but have received no reply.

‡ Intermediate forms between figures 32 and 33 are reproduced from the monuments at Thebes, in the *Manners and Customs of the Ancient Egyptians*, by Wilkinson (first edition), vol. I, p. 325. Comp. Lepsius, *Denkmaler aus Ægypten und Æthiopien*. Vol. II, Pl. 132.

§ Axe blades of precisely the same form as in Fig. 33 (without a handle), are deposited one at the Louvre and the other in the collection of Mr. Greenwell at Durham. In these two, as in the original of Fig. 33, the borders around the two semi-circular apertures are in slight relief. Similar axes to those of Fig. 33 may be seen among the reproductions of the twelfth dynasty. Lepsius, *les Métaux*, vol. II, Pl. 132 and Wilkinson, *Manners and Customs*, vol. I, p. 325, Figs. 5 and 6.

of Fig. 34, and present divers images. Celts, with sockets similar to those which are so often found in Europe, are unknown in Egypt, but celts with pinions are met with there which approach in appearance those with sockets. The pinions, which are folded around the handle, are found only on one side; Fig. 40 represents such a celt. The latter is made of iron, but bronze celts of the same form are likewise discovered in Egypt.*

In Egyptian tombs, poniards with double edges have been found of bronze. The hilt is frequently formed from a bronze plate, the two sides covered with wood, horn, bone, or ivory.

The hilts of the poniards represented by Figs. 1 and 3–5, which are of this description, have around them a border in bronze. A like border may also be seen in the larger part of the hilt of figure 2, but the pommel is entirely of bone, or rather of ivory fastened by a rivet, parallel to the blade.

Upon the handles last mentioned, the hilt properly speaking is, as is generally the case, much large than the pommel. Such however is not always the case. A poniard of bronze discovered at Thebes, of the same type as in Fig. 9, has a pommel almost as large as the hilt.†

In the poniard represented in Fig. 9, the pommel is a little larger than the hilts.‡ The latter has two semi-circular holes and is of ivory, while the rest is of horn or hard wood, fastened with bronze rivets. In Fig. 10, the pommel is very much larger than the handle, in which are not to be included the long and narrow lobes of the handle, which comprise the upper extremity of the blade. § Still larger is the pommel of the poniard which is represented by Fig. 11, and which we have already described; the two semi-circular holes which are seen in the pommel of Fig. 9 are likewise found in Fig. 11, as also in Fig. 10. Each of these four poniards have pommels almost circular.

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*A similar celt in bronze, the pinions of which do not extend as far as those in Fig. 40, is deposited in the Leyden museum. Leeman's *Monuments Égyptiens du Musée de Leide*, Pl. 80, Fig. 5. Chabas, *Étude sur l'antiquite historique*, p. 76.

† The hilt is half horn, half ivory (Prisse d'Avennes *Monuments Égypt*, Pl. 46, Chabas, *Études sur l'antiquite historique*, p. 92). The work quoted by Prisse d'Avennes as well as many other books of importance for this essay, is not at Stockholm.

‡ The original of Fig. 9 is deposited in the British Museum. The handle is prolonged into a narrow tongue which crosses the hilt. (Kembler *Horæ ferales* Pl. 8, Fig. 3, p. 156.) There another poniard is mentioned deposited in the British Museum, "a bronze or silver hilt which unites the pommel of ivory to the blade." On the occasion of a session of the Institute of Archæological Correspondences at Rome, on the 28th of February, 1879, I saw a magnificent poniard of the same type as Fig. 9. It belonged to Mr. Alex. Castellani, who had received it from Marietta. The poniard, with the two usual holes, was of cedar wood, entirely covered with gold. The lower part of the hilt was of silver, ornamented with gold rivets, symmetrically placed. Along the middle of the blade was a simple line in relief, the greater part of which had sharp edges.

§ The original of Fig. 10 constitutes a portion of the collection of Mr. Greenwell at Durham. I owe the design of the latter and of other Egyptian bronzes deposited in the same collection to my friend Mr. Sven Soderberg, of Lund.

Fig. 8 shows us a poniard of bronze deposited in the museum at Berlin, the pommel of which is very much larger than the hilt. The pommel, not round, but elongated, is of ivory; the rest of a dark substance (horn or rhinocerous hide), fastened with large rivets incrusted with gold.

Sometimes the whole hilt is of metal, as in the case of one of the poniards found in the tomb of Queen A'hhotep (Fig. 18), the blade being of bronze the hilt of gold. Yet more precious is the hilt of the other poniard discovered in the same tomb (Fig. 15). The mural paintings of the tomb of King Rameses III at Thebes represent a number of arms, among others long poniards with double blades, as in Fig. 20. The blades of some are painted red, others blue or green.*

The hilts of these arms are yellow; probably they were made of gold or were gilded. An arm of similar form (Fig. 19), which must have been of bronze, since it is painted red, is seen in another mural picture.

Besides these poniards with double edges, a kind of long knife or short sword with one edge was employed in Egypt. On the Theban bas-relief, King Rameses II wears an arm of this form, and the god Ammon is quite often represented with a like arm in his hand. A bas-relief in a temple at Ibsambul, in Nubia, shows us Ammon and King Rameses III, the latter raising his hand to strike a multitude of vanquished enemies. In the hand of the god the arm reproduced in Fig. 13 is seen. It is painted red, and must consequently have been of bronze. Among the arms of mural paintings already mentioned in the tomb of Rameses III are several of this character, a few even carved (Fig. 6), but they are all blue, and consequently were of iron.

The museum of the Louvre possesses an arm in bronze of this type (Fig. 14). The blade and the hilt are fused in one piece; the hilt, which ends on the reverse side in a little eye, is ornamented with a dog very well modeled; on the blade is seen a legend in hieroglyphics.

Egyptian monuments very often represent poniards rather long (Fig. 20), but veritable swords are not seen during the period we have under consideration. Neither, as I am aware, has the discovery of a real sword in bronze been made in Egypt. It is true that in the magnificent collection of Mr. John Evans, at Nash Mill, is deposited a bronze sword which was discovered at Kawtara during the construction of the Suez canal, and consequently near the frontier. It is very uncertain therefore, whether it can be called Egyptian, at least considering that it is the sole one of its kind. The blade, 43 centimeters long, ends above in a tongue slight and curved forward in the form of a hook; at the base of the blade are two rivet holes.†

The Berlin museum likewise possesses a bronze sword which is reputed to have been discovered in Lower Egypt.‡ But this indication

* Rosellini, Monumenti Civili, Pl. 121.

† Evans. *The ancient bronze implements of Great Britain*, p. 298.

‡ Bastian and Voss. *Des Bronzeschwerter les K. Museums zu Berlin*, Pl. XVI, Fig. 32.

is unreliable, and so much the less probable, inasmuch as the blade in nowise recalls Egyptian poniards, but, on the contrary, resembles many European swords of bronze.

The Egyptians, like other nations, made use of lances. On Egyptian monuments these arms are sometimes seen provided with very short handles.*

Bronze barbs are also found, but not in large numbers, in the collection of Egyptian antiquities. One of these is exhibited in Fig. 41; the long socket is formed by a fold so that a lengthy fissure is seen.†

Bronze lances, the sockets of which are formed in this primitive manner, have not only been discovered in Egypt, but also in Cyprus and Greece. Some of the lance points of Egyptian bronze have a very narrow barb, others are of greater width.‡

As innumerable representations demonstrate, the bow played a prominent rôle among the Egyptians, both in war and in the chase. Consequently, a large quantity of arrow points of bronze have been found. A goodly number of them have a stalk, by means of which they are attached to the staff (Fig. 23). They are often also ornamented with two long projections from the barb (Fig. 24). Others are provided with a socket (Fig. 22). Sometimes the sockets of the arrows (Fig. 21) are formed by folding back the edges of the lower portion; that is, in the same manner as in the cases of the sockets for the lance barbs.

A large proportion of Egyptian arrow points are made with three sharp edges. Such barbs are frequent in western Asia and Greece, where they belong to epochs comparatively recent.

Sometimes upon Egyptian monuments the arrow points have a transversal edge (Fig. 25), the red color of which makes us apprehend they were of bronze.

Arrow points of silex with a transversal edge have been found in Egypt and in some European countries, such as France and southern Sweden.

Amongst the bronze implements it is necessary to remark, besides the axes already mentioned, chisels (Fig. 39), knives (Fig. 42), saws (Fig. 44), drills, awls (Fig. 46), small pincers, hooks (Fig. 45), etc. A large number of them have still retained their handles of wood or horn. Just

* Perrot and Chipiez. *Ouvr. cit.*, vol. I, Fig. 173. Comp. Wilkinson, *Manners and Customs*, p. 291. The points are often painted red, and consequently were of bronze. (Lepsius, *Les Métaux*), Pl. II, Figs. 4 and 12.

† The original of Fig. 41 belongs to the museum at Boulaq. The rent is not only to be seen upon the socket part, which is below the commencement of the blade, but also above it. A similar lance point of Theban bronze forms a part of the collection of Mr. Greenwell at Durham. Compare, *Mémoires de la Société royale des Antiquaires du Nord.* 1873–'74, p. 128, Fig. 3.

‡ The Louvre possesses an Egyptian lance point of bronze, the blade of which is not so narrow as that in Fig. 41, nor of an equal width. Still wider is a lance point which belongs to the Berlin Museum (Wilkinson, *Manners and Customs*, vol. I, p. 312, Fig. 34*a*). A lance point with a blade of unusual length, wide at bottom, but narrow at the top, is represented in the work last cited, vol. I, p. 406.

as upon the axes and poniards, are often seen upon these implements—either on the handle or the bronze itself—a legend in hieroglyphics.

The majority of the implements which we have just cited are also represented on Egyptian monuments, and are there usually painted red* (Figs. 38 and 44). Sickles and needles were also made of bronze; likewise mirrors, strings for musical instruments resembling harps, not to cite other examples.† The mirrors, which are round slabs or plates, with handles, resemble those with which we are acquainted from Estruscan tombs.

We possess as yet very few Egyptian bronzes of a well determined age, and these date almost all from ages immediately bordering on the epoch when they had begun to use iron. Now we can not respond as completely as we would wish to this important question, What forms are characteristic to each period of the Egyptian age of bronze?

It is only very seldom—as, for instance, when hilts of poniards (Figs. 9–11), or handles of axes (Figs. 30–33) are referred to—that we can follow the typologic development. Meanwhile that which we know already is very interesting. The discovery of the tomb of Queen A'hhotep proves that poniards of the type of Fig. 11 are a little anterior to the year 1500 B. C.

As a consequence the types (Figs. 9 and 10) belong to a more remote era.‡ This is confirmed by the fact that the original of Fig. 9 was discovered in the same tomb as the ax represented by Fig. 33.

This tomb ought to date from the year 2000 B. C. or thereabouts, since the axes similar to Fig. 15, as we have seen, are represented upon the monuments of the twelfth dynasty, reigning at that period. Too few Egyptian bronzes of the epoch we are examining have been until now chemically investigated. We can, however, discover that the bronze then employed in Egypt, as that used in Europe during the age of bronze, was an alloy of copper and tin, probably without the intentiona addition of lead, zinc, or other metal. §

An Egyptian poniard analyzed by Vauquelin, containing 85 parts to 100 of copper, 14 parts to 100 of tin, and 1 part to 100 of iron, or of other metals. ||

Other arms of Egyptian bronze are composed of 94 parts to 100 of copper, 5.9 parts to 100 of tin, and 0.1 part to 100 of iron.¶

According to Wilkinson ** the proportion of tin in almost all Egyptian bronzes analyzed up to the present time is about 12 parts to 100.

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*Lepsius. *Les Méteaux*, Pl. II. Fig. 19, of the same plate proves that bronze knives were also used for shaving off the hair.

†Lepsius. *Les Méteaux*, Pl. II, Fig. 13 (sickle), Fig. 20 (mirror), and Fig. 22 (harp).

‡D'Athanasi. *Account of researches*, p. 183.

§ In more recent Egyptian bronzes we often meet with lead, and perhaps zinc. Bibra. *Die Bronzen der alten und altesten Volker*, , p. 94.

|| Bibra. *Die Bronzen*, p. 94.

¶ Birtish Museum. *A guide to the Egyptian rooms* (London 1879), p. 40.

** *Manners and Customs*, vol. III. The special analyses upon which this datum is based are not, however, quoted.

The Egyptians were forced to import the tin necessary for their industries, and this was certainly an enormous quantity. They probably had recourse to Asia, for this precious metal, even more indispensable in antiquity than in our own days.* Copper, on the other hand, was common, if not in their own country, at least in the immediate vicinity. The peninsula of Sinai possesses considerable mines, mining operations in which began at a period very remote.

* Erman. *Ægypten*, p. 613.

DESCRIPTIONS OF THE PLATES.

PLATE I.

Fig. 1. Bronze poniard; hilt of wood and bronze ($\frac{1}{4}$).* Museum of the Louvre. (Lindenschmit; *Altherthmeümer unserer Heidenschen Vorzeit*, 2, XI, Pl. 3, Fig. 1.)

Fig. 2. Bronze poniard; hilt of bronze, wood, and ivory ($\frac{1}{3}$). Museum of the Louvre. (Lindenschmit; *Alterthümer* 2, XI, Pl. 3, Fig. 2.)

Fig. 3. Bronze poniard; hilt of wood and bronze ($\frac{1}{3}$). Museum of the Louvre. (After a photograph.)

Fig. 4. Bronze poniard; hilt of ivory and bronze ($\frac{1}{4}$). British Museum. (Kemble, *Horæ ferales*, Pl. 8, Fig. 2.)

Fig. 5. Bronze poniard; hilt of bronze and wood ($\frac{1}{3}$). Turin Museum. (After a photograph.)

Fig. 6. Saber, painted blue; mural painting on the tomb of Rameses III, at Thebes. Reseillni *Moumenti civili*, Pl. 121; Lepsius *les Métaux dans les inscriptious égyptiennes* Pl. 2, Fig. 2.)

Fig. 7. Bronze knife. Collection of Mr. Greenwell at Durham, England. (After a design executed by Mr. Soderberg.)

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* The figures indicate the relation between the objects and their natural size. It is sometimes very difficult to distinguish whether the handles are of wood or bone.

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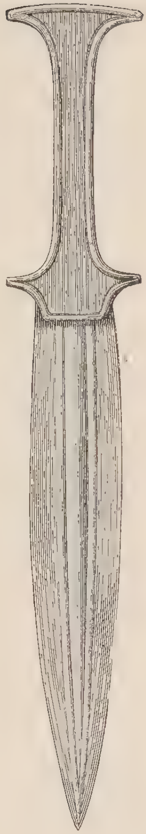

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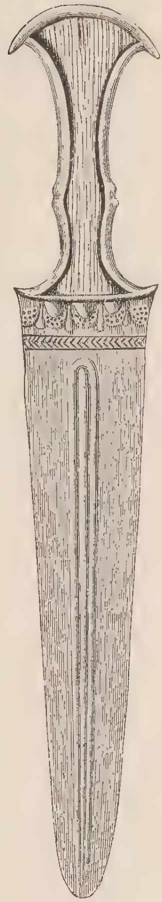

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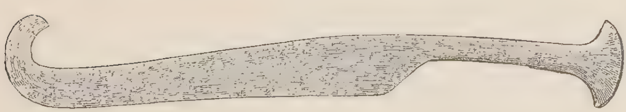

7.

THE AGE OF BRONZE IN EGYPT.

Plate II.

Fig. 8. Poniard (bronze); hilt of bronze, horn (or rhinoceros hide), and ivory (¼). Berlin Museum. (Bastian and Voss. *Die Bronzeschwerter des Königlichen Museums zu Berlin*, Pl. 16, Fig. 31*a*. Compare 31*b* of the same; plate sheath of leather.)

Fig. 9. Bronze poniard; hilt of bronze, ivory, and horn (¼). British Museum. (Kemble, *Horæ ferales*, Pl. 7, Fig. 3.)

Fig 10. Bronze poniard; hilt of bronze and bone (¼). Collection of Mr. Greenwell at Durham. (After design executed by Soderberg.)

Fig. 11. Bronze poniard; hilt of bronze and silver (½). Museum at Boulaq. (From a photograph.)

Fig. 12. The same poniard, side view. Perrott and Chipiez, *Histoire de l'Art dans l'Antiquité* vol. I, p. 830, Fig. 564.

Fig. 13. Arm painted red, handle yellow. Temple of Ibsambul in Nubia, in the time of Rameses III. Champollion, monuments egyptiens, vol. I, Pl. 11. Lepsius, *Les Métaux*, Pl. 2, Fig. 8.

Fig. 14. Large knife of bronze (c. ½). Louvre Museum. (From a photograph.)

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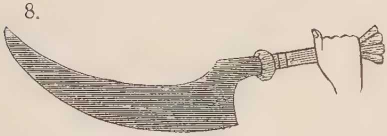


13.

14.

THE AGE OF BRONZE IN EGYPT.

Plate III.

Fig. 15. Bronze poniard, wood and precious stones ($\frac{1}{2}$), see the description, p. 39, found in the tomb of Queen A'hhotep. Boulaq Museum. (From a photograph.)

Fig. 16. Bronze poniard; ($\frac{1}{2}$). British Museum. (From photograph.)

Fig. 17. Bronze poniard; ($\frac{1}{2}$). Lower portion of the hilt of hollow bronze, upper portion wanting. Boulaq Museum. (From a photograph.)

Fig. 18. Bronze poniard, hilt of gold ($\frac{1}{3}$), found in the coffin of Queen A'hhotep. Boulaq Museum. (From a photograph.)

Fig. 19. Poniard painted red, the hilt yellow. Mural painting. Lepsius, *Les Métaux*, Pl. 2, Fig. 9.

Fig. 20. Long poniard painted red, hilt yellow. Mural painting on the tomb of Rameses III at Thebes (Rosellini, *Monumentii cirili*), Pl. 121. Lepsius, *Les Métaux* Pl. 2, Fig. 1.

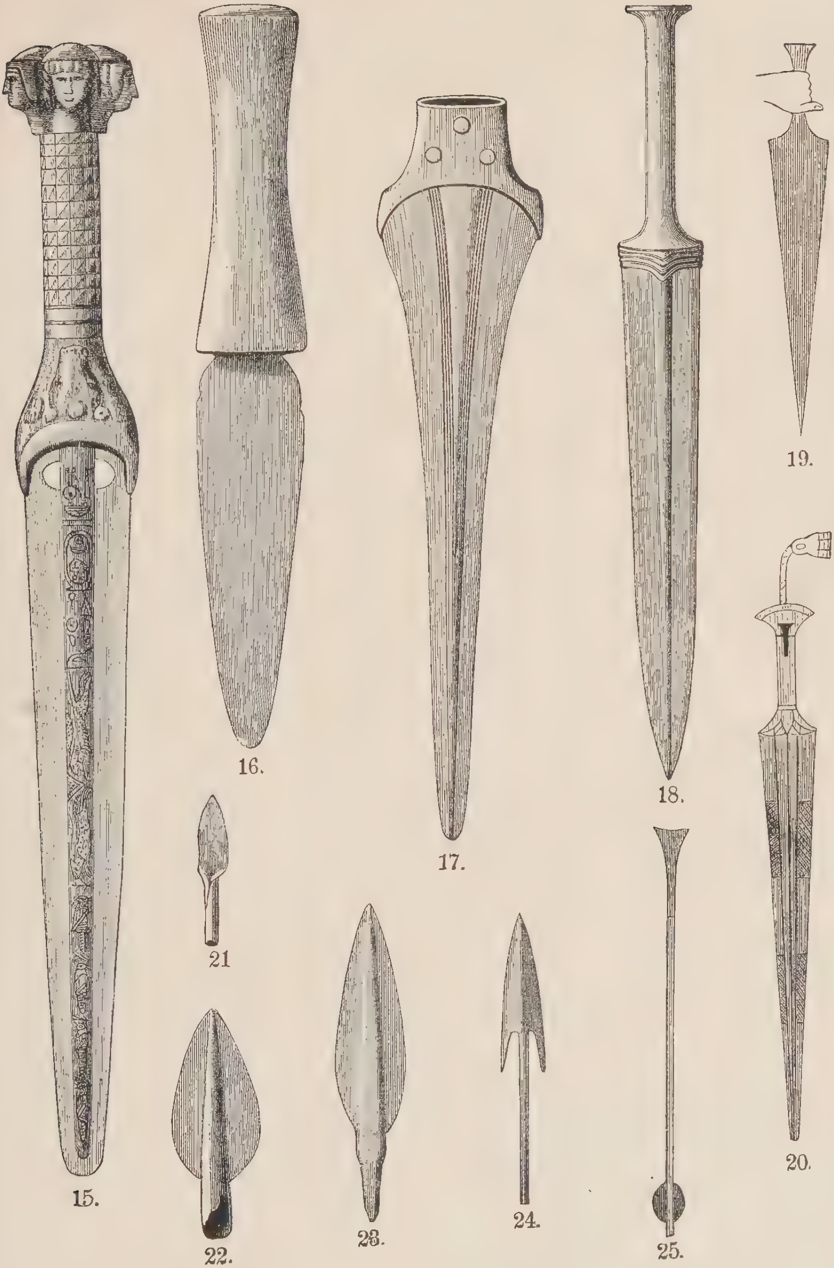
Fig. 21. Arrow point of copper (pure) ($\frac{1}{4}$). British Museum. Kemble, *Horæ ferales*, Pl. 6, Fig. 1.

Fig. 22. Arrow barb of bronze ($\frac{2}{3}$). Boulaq Museum. (From a photograph.)

Fig. 23. Arrow barb of bronze ($\frac{2}{3}$). Boulaq Museum. (From a photograph.)

Fig. 24. Arrow barb of bronze ($\frac{2}{3}$). Boulaq Museum. (From a photograph.)

Fig. 25. Arrow; barb with a transversal sharp edge, painted red. Mural painting. Lepsius, *Les Métaux* Pl. 2, Fig. 12.



THE AGE OF BRONZE IN EGYPT.

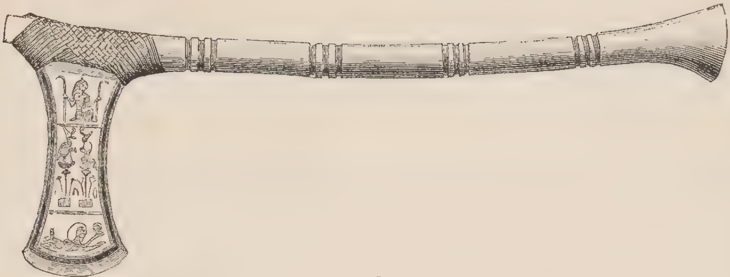
Plate IV.

Fig. 26. Ax of gilt bronze, hilt, wood and precious stones ($\frac{1}{5}$). Coffin of Queen A'hhotep, Boulaq Museum. (From a photograph.)

Fig. 27. Reverse of the same ax. (From a photograph.)

Fig. 28. Bronze ax ($\frac{1}{2}$). Boulaq Museum. (From a photograph).

Fig. 29. Bronze ax bearing the name of Thoutmos III, the handle of wood ($\frac{1}{4}$). Boulaq Museum. (From a photograph.)



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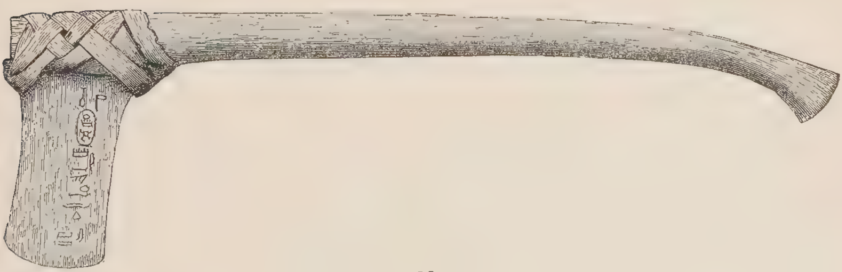


29.

THE AGE OF BRONZE IN EGYPT.

Plate V.

Figs. 30 and 31. Axes. Mural paintings of the sixth dynasty. Lepsius, *Denkmäler aus Ægypten und Æthiopen*, vol. II., Pls. 121 and 108.

Fig. 32. Ax, blade painted yellow (or red). Mural painting of the twelfth dynasty. (Lepsius, *Denkmäler* vol. II, Pl. 151. Lepsius, *Les Métaux* Pl. 2, Fig. 2.)

Fig. 33. Bronze ax; the surface of the handle of silver ($\frac{1}{5}$). British Museum. (From a photograph.)

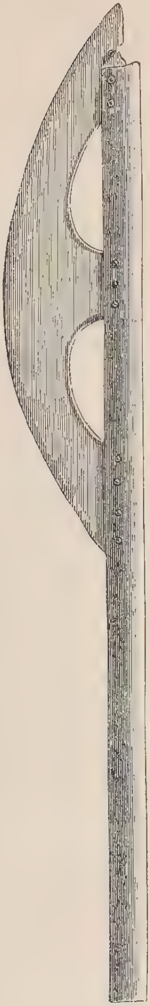
Fig. 34. Bronze ax; pierced through, handle of wood ($\frac{1}{4}$). British Museum. (From a photograph.)

Fig. 35. Bronze ax; ($\frac{1}{2}$). British Museum. (From a photograph.)

Fig. 36. Bronze ax; (c. $\frac{1}{2}$). Museum of the Louvre. (From a photograph.)

Fig. 37. Bronze ax; bearing the name of Thoutmos III; handle of wood. *Mémoires de la Socitété royal des Antiquarries du Nord*, 1873–'74, p. 128, Figs. 5*a* and 5*b*; front view of the blade.

Fig. 38. Ax; the handle painted red. Mural painting. Lepsius, *Les Métaux;* Pl., Fig. 15.



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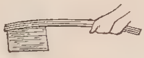

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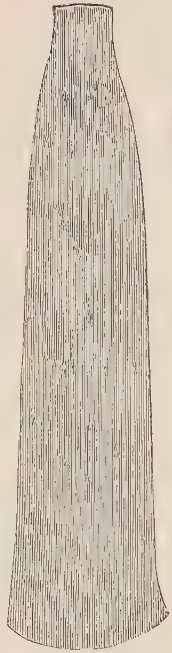

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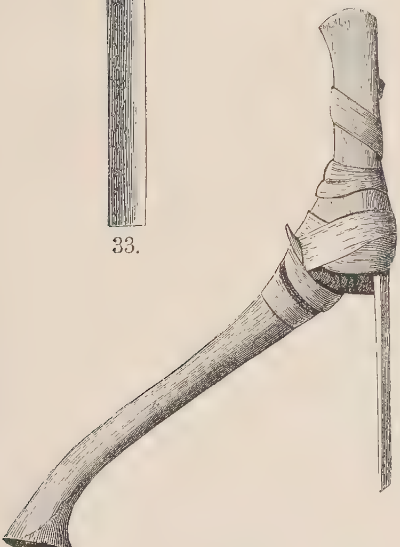

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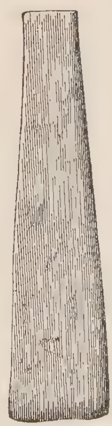

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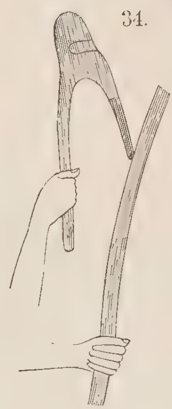

38.

THE AGE OF BRONZE IN EGYPT.

Plate VI.

Fig. 39. Bronze chisels ($\frac{1}{3}$), hilt bearing the name of Thoutmos III, of wood. Boulaq Museum. (From a photograph.)

Fig. 40. Celt (c. $\frac{1}{2}$). Museum of the Louvre. (From a photograph.) The Leyden Museum possesses a celt of bronze of the same form.

Fig. 41. Lance barb of bronze ($\frac{1}{2}$). Boulaq Museum. (From a photograph.)

Fig. 42. Bronze knife (c. $\frac{1}{4}$). Boulaq Museum. (From a photograph.)

Fig. 43. Bronze saw ($\frac{1}{8}$), wooden handle. British Museum. (From a photograph.)

Fig. 44. Saw painted red. Mural painting. (Lepsius, *Les Métaux*, Pl. 2, Fig. 14.)

Fig. 45. Bronze fishhook ($\frac{1}{2}$). Boulaq Museum. (From a photograph.)

Fig. 46. Bronze awl ($\frac{1}{4}$), wooden handle. British Museum. (From a photograph.)

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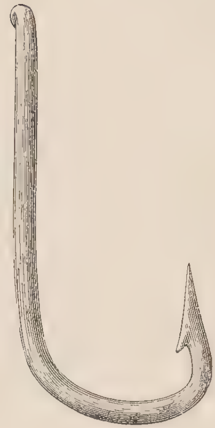

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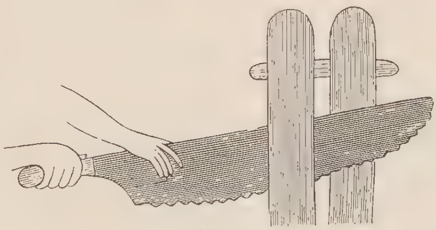

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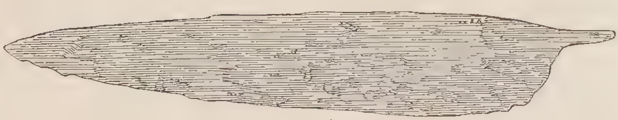

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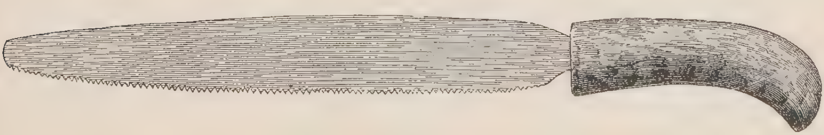


43.

THE AGE OF BRONZE IN EGYPT.

PROGRESS OF ANTHROPOLOGY IN 1890.

By Prof. OTIS T. MASON.

INTRODUCTION.

In the present summary of anthropology it is designed to show the progress of the science in a somewhat elementary manner, in order to reach a larger number of readers. The phrase, "natural history of man" is here taken to mean the employment of the apparatus, processes, and principles of natural history to the study of mankind. This definition will be subject to constant changes. Just as soon as any set of phenomena or facts concerning our species may be arranged, classified, and studied after the manner of the naturalist, only then should they be admitted into the laboratory of anthropology.

Once admitted, their difficulties will not cease. In order to keep pace with other natural knowledge, these series of phenomena or facts must ever be subjected to new forms of scrutiny. Botanists and zoölogists are constantly inventing better apparatus and refining their methods, and furthermore, each department of these sciences requires special machinery and appliances to perfect the delicacy of the senses and to enable the investigator to hold large masses of facts before his mind at once.

Anthropology therefore is required to be a most vigorous science, keeping pace with every improvement in other sciences, both general and special, and refining its own apparatus and methods perpetually.

The summary which at the close of each year faithfully chronicles the topics discussed, the organized means of research, the improvement in apparatus and the results attained, serves as a historical monument by means of which future students may trace their way backward in the development of the science.

A complete syllabus of anthropology would include—first, what man is, and second, what man does. What man is may be denominated *structural anthropology;* what man does, *functional anthropology.*

Science always deals with phenomena, and the name of each science is derived from the things observed and studied. For instance, we

may arrange the various parts of the subject under consideration in the order of phenomena.

PHENOMENA.	SCIENCE.
All mankind as natural objects	Anthropology.
What man is—Structural anthropology.	
The embryo of mankind and life of the individual	Ontogeny.
The body of man (specific and comparative)	Anatomy.
The functions of the body	Physiology.
Form and color, weight and number	Anthropometry.
The nervous system in relation to thought	Psycho-physics.
Natural divisions of mankind	Ethnology.
What man does—Functional anthropology.	
To expresss his thoughts	Glossology.
To supply his wants	Technology.
To gratify his desires	Aesthetics.
To account for phenomena	Science and philosophy.
To co-operate in the activities and ends of life	Sociology.
In presence of a spirit world	The science of religion.
The past of human life and actions is studied—Science.	
(1) In things decayed or dug from the earth	Archæology.
(2) In the decipherment of inscriptions	Palæography.
(3) In the acts and sayings of the unlettered	Folk-lore.
(4) In written records	History.
Sciences helpful to anthropology.	
To determine the material of art-products	Mineralogy.
To fix the age of relics	Geology.
In studying the mutual effects of man and the earth on each other	Geography.
To determine man's place in nature and his acquaintance therewith	Botany and zoölogy.

It will readily be seen that one man may not be profoundly versed in anthropology, but everyone who reads the foregoing syllabus carefully will at a glance discover that there is some particular branch of the subject for which he is fitted by his daily occupations.

The resources already in existence for the student, both general and special, will be noted in the proper order. They may be classified as follows:

(1) Those relating to the subject as a whole.

(2) The resources of biological studies.

(3) Psycho-physical investigations, that is, the study of psychology experimentally.

(4) The races of men.

(5) Language.

(6) Arts and archæology.

(7) Sociology.

(8) Philosophy, folk-lore, and mythology.

(9) The relation of nature to man.

I.—GENERAL ANTHROPOLOGY.

It must be remembered in this connection that we have not now to lay the foundation for a new science, but to bring together the results of an exceedingly vigorous one. The resources at our command are:

(1) General treatises, like Tylor's "Anthropology," courses of lectures, encyclopædias, and classifications.

(2) Societies with their published proceedings and transactions and periodicals devoted entirely to the study of man.

(3) Assemblies and congresses, national and international, with their *Comptes-rendus.*

(4) Museums and collections, public and private, with catalogues and books of instructions. Expositions.

(5) Special libraries containing both literature and albums.

(6) Laboratories, as in other sciences, for investigation both in structural and functional anthropology.

The most noteworthy event in our science for Americans, was the Congrès International des Américanistes, at Paris. At this meeting the *compte-rendu* of the seventh session held in Berlin (1888) was presented. The list of papers there printed is as follows:

On the name America, Guido Cora. Basques, Bretons, and Normans on the coast of North America in the beginning of the sixteenth century, M. Gaffarel. Publication of writings and documents relative to Columbus and his times, on the occasion of the celebration of the fourth centenary of the discovery of America, Guido Cora. Ensayo histórico de la legislacion primitiva de los estados españoles de América, M. Fabié. Bemerkungen zur modernen Litteratur über die Entdeckung Amerikas, M. Gelcich. On the Nahuatl version of Sahagun's Historia de la Nueva España, Daniel G. Brinton. Archæology of Mexico and South America, Dr. Heger. Colliers de pierre de Porto Rico, Jimenez de la Espada. Antiquities of the State of Vera Cruz, Hermann Strebel. Archæological result of a voyage to Mexico, Edward Seler. Origin, working hypothesis, and primary researches of the Hemenway Southwestern Archæological Exposition, F. H. Cushing. Antiquities of Nicaragua, Charles Boralius. Antiquités céramiques de l'île de Marajo; sur la néphrite et la jadeite, Ladislau Netto. Sur la provenance de la néphrite et la jadeite, R. Virchow. Die Verbreitung der Eskimo Stämmer, H. Rink. The Aztecs and their probable relations to the Pueblo Indians of New Mexico, S. B. Evans. De l'emploi de la coca dans les pays septentrionaux de l'Amérique du Sud, A. Ernst. Die Bekleidung eines reichen Guajiro Indianers, C. M. Pleyte. Sur la craniologie américaine, R. Virchow. An anatomical characteristic of the hyoid bone of the pre-Columbian Pueblo Indians, Arizona, Drs. Wortman and Ten Kate. Die Frage nach der Einheit oder Vielheit der amerikanischen Eingeborenenrasse geprüft an der Untersuchung ihres Haarwuchses, Gustav Fritsch. Die Chronologie des diluvialen Menschen in Nordamerika, Émil

Schmidt. Vestiges laissés par les populations pré-Colombiennes de Nicaragua, Désiré Pector. Über alt-peruanische Hausthiere, Dr. Nehring. Die Nutzpflanzen der alten Peruaner, L. Wittmack. Diritto e morale nel Messico antico, Vincenzo Grossi. La cremazione in America prima e dopo Cristoforo Colombo, Grossi. Anthropologie des peuples d'Anahuac au temps de Cortez, R. Hartmann. Was America peopled from Polynesia? Horatio Hale. Étude sur la langue Mam, le Comte de Charencey. Textes, analyses et vocabulaire de la langue Timucua, Raoul de la Grasserie. De la famille linguistique Pano, *id.* The historical archives of the Hemenway Southwestern Archæological Expedition, Adolf Bandelier. Sur le débris de cuisine (Sambaquis) du Brésil, H. Müller. Das Verhältniss zwischen dem Ketschua und Aimaráid. Sur une ancienne carte de l'Amérique, M. Gaffarel. Verwandtschaften und Wanderungen des Tschebtscha, Max Uhle. Trois familles linguistiques des bassins de l'Amazone et de l'Orenoque, Lucien Adam. Bibliographie des récentes conquêtes de la linguistique sud-américaine, Lucien Adam. Das Tonalamatl der Aubin'schen Sammlung und die Verwandten Kalenderbücher, Edward Seler. Die Entzifferung der Maya Handschriften, E. Förstemann. Classification chronologique des monuments architectoniques de l'ancien Pérou, Ferdinand Borsari. Contribution à l'américanisme du Cauca (Colombie), Léon Douay. Linguistique des peuples qui habitent le centre de l'Amérique du Sud, von den Steinem. Figures péruviennes en argent, Lüders.

The Section of Anthropology in the American Association for the Advancement of Science had for its presiding officer Dr. Frank Baker, the director of the National Zoölogical Park. His address will be noticed in the chapter on Biology. The following are the titles of important papers read: Indian origin of maple sugar, H. W. Henshaw; Fort Ancient, W. K. Moorehead; Aboriginal stone implements of the Potomac Valley, W. H. Holmes; Earthwork near Fosters, Little Miami Valley, Ohio, F. W. Putnam; Brains and medisected head of man and chimpanzee, Burt G. Wilder; Gold beads of Indian manufacture from Florida and New Jersey, C. C. Abott; A study in mental statistics, J. Jastrow; Arts of modern savages for interpreting archæology, O. T. Mason; Relation of mind to its physical basis, E. D. Cope; Ancient hearth in the Little Miami Valley, F. W. Putnam; Evolution of a sect, Anita N. McGee.

The sixtieth meeting of the British Association for the Advancement of Science was held in Leeds, September 3–13. The vice presidential address of Mr. John Evans was devoted mainly to this question: What is the antiquity of the human race, or, rather, what is the antiquity of the earliest objects hitherto found which can with safety be assigned to the handiwork of man? As regards Tertiary man there are three classes of evidence, to wit: (1) the presumed discovery of parts of the human skeleton; (2) that of animal bones said to have been cut and worked by the hand of man; and (3) that of flints thought to be arti-

ficially fashioned (J. Anthrop. Inst., XII, 565; Tr. Hertsford Nat. Hist. Soc., I, 545). In summing up the evidence, Dr. Evans says that the present verdict as to Tertiary man must be in the form of "not proven." The latter part of the address is devoted to the question of the Aryan language and the Aryan race and to the improved resources of anthropological study. Papers were read upon the following topics: Hereditism, F. O. Morris; Religion of the Australian aborigines, J. W. Fawcett; The present aspect of the jade question, F. W. Rudler; Is there a break in mental evolution? Lady Welby; Unidentified peoples in Britain in pre-Roman times, Dr. Phéné; Yourouks of Asia Minor, T. Bent; Aryan cradle land, J. Stuart Glennie; Reversions, Nina Layard; Physical development, G. W. Hambleton; Archæological remains bearing on the origin of the Anglo-Saxons in England, Dr. Munro; Duggleby "Howe," E. Maure Cole; Romano-British graveyard in Wetwang-with-Fimber, J. R. Mortimer; Minute neolithic implements, H. C. March; Retrogression in prehistoric civilization in Thames Valley, H. Stopes; Boring of stone hammers, W. Horne; Stethographic tracings of male and female respiratory movements, Wilberforce Smith; Human remains at Woodyates, Wittshire, J. G. Garson; Old and modern excavations of the Wandsdyke at Woodyates, Gen. Pitt Rivers.

The British Association committees form an active part of the general meetings. Upon anthropological subjects were the Report upon the new edition of the little handbook for collectors entitled Notes and Queries; Report of the committee on anthropometric laboratory; On prehistoric inhabitants of Britain; On nomad tribes of Asia Minor; On northwestern tribes of Canada; On India. The British Association for the Advancement of Science, coöperating with the Anthropological Institute of London, organized a lecture course on anthropology, differing from the Paris course not only in being less technical, but also in the repetition of the lectures before institutions and before the public in various cities throughout the United Kingdom. The series was as follows:

(1) Physical anthropology. By Dr. Garson.

(2) The geological history of man. By F. W. Rudler.

(3) Prehistoric dwellings, tombs, and monuments. By A. L. Lewis.

(4) Development of the arts of life. By Henry Balfour.

(5) Social institutions. By E. W. Brabrook.

(6) Anthropometry. By G. W. Bloxam.

During the current year the beneficent results of the Paris Exposition began to appear, especially in the form of reports on the various congresses. Of the tenth session of Congrès international d'Anthropologie et d'Archéologie préhistoriques, M. Hamy, Membre de l'Institut, and general secretary of the congress, prepared the Compte Rendu, a pamphlet of 48 pages. The French Association for the Advancement

of Science met during the current year at Limoges, August 7–15. In this association is a section devoted exclusively to anthropological subjects.

The twenty-first meeting of the German Anthropological Association was held at Munster, Westphalia, August 11–15. At each one of these annual meetings it is customary to explore thoroughly the anthropological resources of the region. Professor Hosius this year read a paper on the geognostic structure of Westphalia, the prehistoric stations and the remains of quaternary animals found there, and Professor Nordhoff followed up this communication with one upon the urns and the weapons found in this state.

The German Association of Naturalists and physicians (Versammlung deutscher Naturforscher und Aertzte) must not be confounded with the General Anthropological Society of the empire and Austria. The first named held its sixty-third meeting in Bremen, 15–20th September.

The Russian Association of Naturalists and physicians held its eighth meeting in St. Petersburg, January 8–19. In the 70 sessions 2,200 took part and over 400 communications were made. One of the ten sections was devoted to geography, ethnography, and anthropology. The subjects discussed were, migrations, history of primitive culture, anthropometry, local archæology, and the ethnography of Russia. Upon this last point the opportunities of study are unparalleled and the Russian ethnographers have not failed to make use of them.

There is no better illustration of the rapidity with which the science of anthropology has asserted itself than the museo de la Plata, a sketch of which is here given (Plate I). The capital of the province of Buenos Ayres, the city of La Plata, was founded in 1882, to replace as a seat of provincial authority the city of Buenos Ayres declared in 1880 to be the capital of the republic. In the brief space of time intervening, under the energetic management of Signor Francisco P. Moreno, a fully equipped museum is completed. The anthropological portion owes its existence almost entirely to the director. It is especially rich in material illustrating the aboriginal life of the republic. (Plate II.—Ground-plan of Museum.)

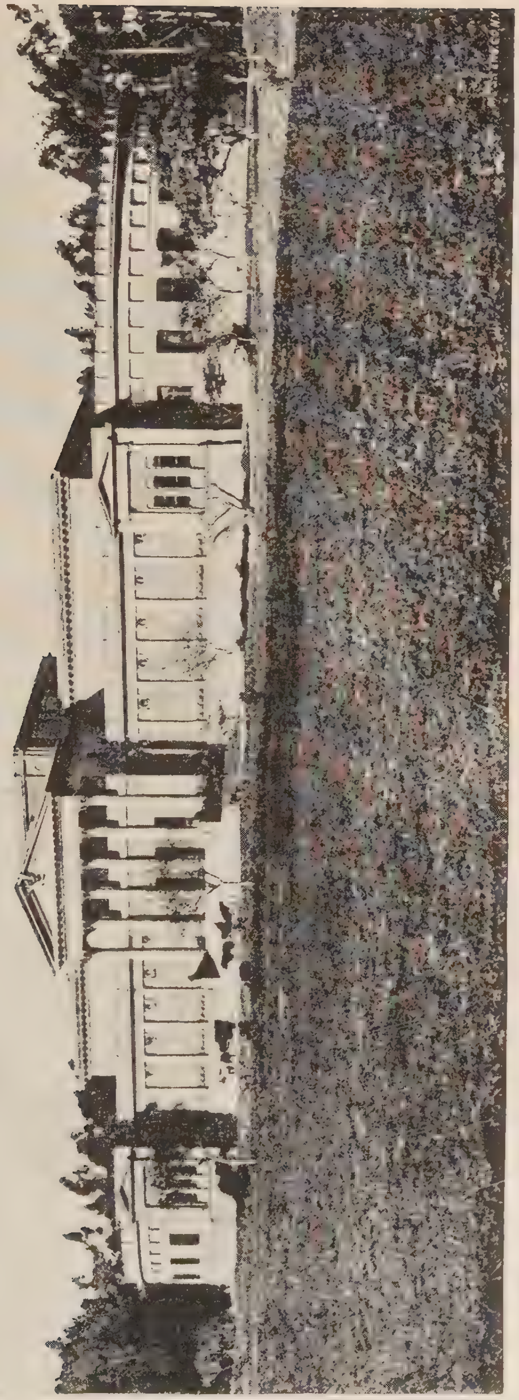
In the summary of last year a brief account was given of the manner in which the science of man is covered in the institutions of Paris. Dr. Sophus Müller contributes the following list for Copenhagen:

(1) Royal Museum of Northern Antiquities. Devoted to early Denmark, including the stone, the bronze, the iron, and the historic period, until 1660.

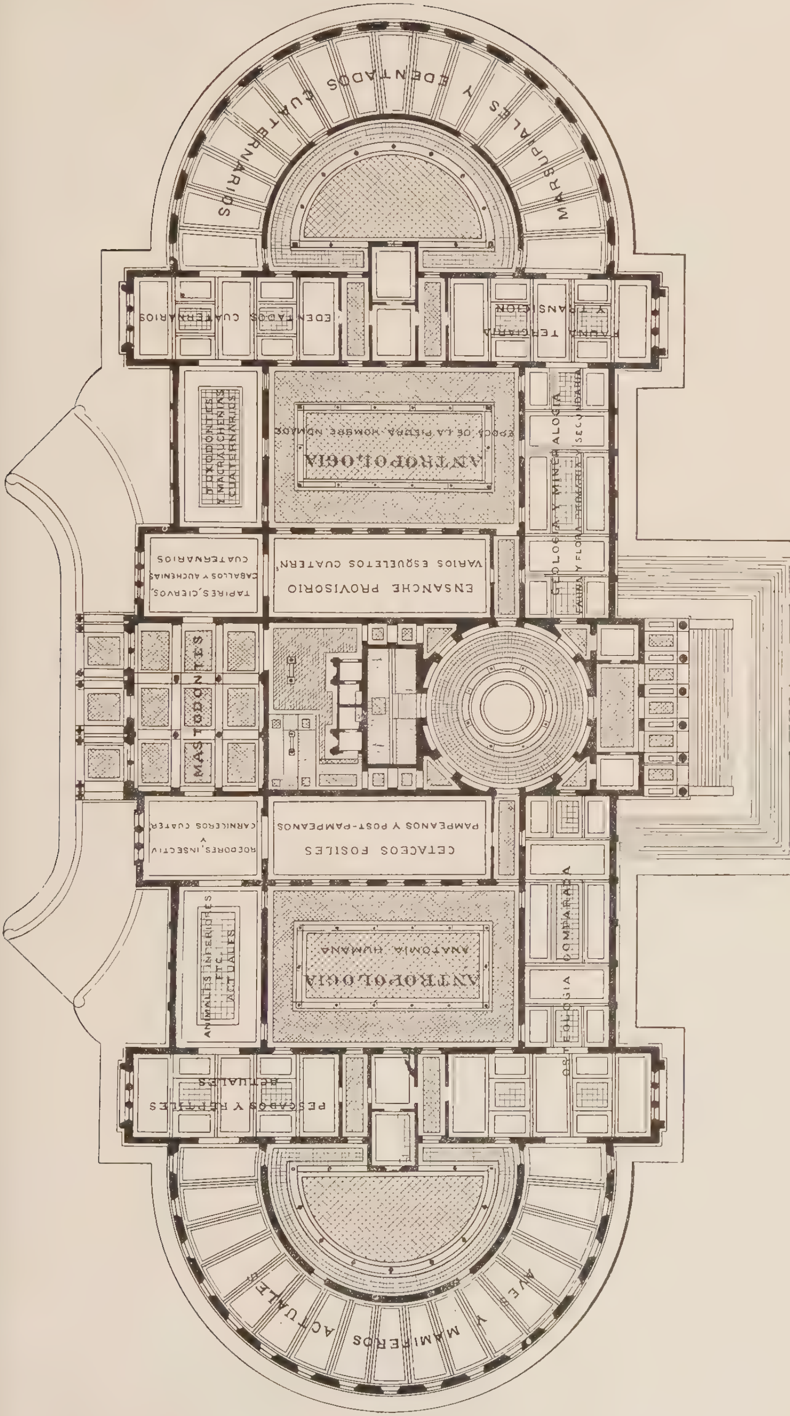
(2) The Folk Museum, general historic museum, from 1660 to 1800. Will be united with the Museum of Northern Antiquities under one direction in a new building.

(3) Rosenburg Castle, the collections to illustrate the life and history of the present dynasty.

(4) The Fredericksburg Castle Collection, general Danish history from 1000–1800.



MUSEO DE LA PLATA.



PLAN OF THE MUSEUM OF LA PLATA—PRINCIPAL FLOOR.

(5) A new museum for mediæval and modern times in other countries of Europe.

(6) Ethnographic Museum, arranged to show the civilizations of the world by tribes. This was probably the first collection in Europe to be laid out upon a strictly ethnographic basis.

(7) Royal Museum of classic antiquities in Prinzens Palais.

(8) Royal collection of coins in Prinzens Palais.

No mention is made here of the royal galleries of art nor of the collection of crania and skeletons in the Zoölogical Museum. The visitor to Copenhagen never fails to spend a day in the Thorwalsden Museum, into which the affectionate esteem of his fellow citizens has gathered the works of the great sculptor and his personal effects and displayed them most attractively.

A work of primary importance, which the director of every other anthropological museum should imitate with great promptness and care, is Dr. Hamy's volume entitled Origines du Musée d'Ethnographie du Trocadero, Paris. The first exotic presents known to have come to France were the gifts of Haroun al Raschid to Charlemagne, 801 and 807, A. D. From that moment to the present all sorts of treasures, gotten in many ways, have been in the charge of public keepers. The modern museum is shown by this volume to have been the growth of ages, the beginning or germ being the curiosity of the king or some of the nobility. It would be well if every important museum could have a volume of history like Dr. Hamy's "Origines."

In addition to a thorough history of each public museum, prepared by its own authorities, the exigencies of intercommunication have led to the founding of a journal for museum workers, entitled, *Internationales Archiv für Ethnographie* (Leyden), and in February, appeared the first number of the Bulletin des Musées, Paris. It is edited by Mr. Edward Garnier and Léonce Benedite, and resembles the Berlin "Year Book of the Royal Prussian Art Collections," under the heading of "Mouvement des Musées it gives notes on other national galleries and collections, and a bibliography.

The standard list of journals remains the same. No anthropologist can afford to neglect the following list:

The American Anthropologist, Washington; *Archiv für Anthropologie*, Braunschweig; *Archivio per l'Antropologia*, Firenze; *Bulletins de la Société d'Anthropologie de Paris; Internationales Archiv für Ethnographie*, Leyden; *Journal of the Anthropological Institute of Great Britain and Ireland*, London; *L'Anthropologie*, Paris; *Mittheilungen der Anthropologischen Gesellschaft in Wien; Verhandlungen der Berliner Gesellschaft für Anthropologie, etc.*, Berlin; *Zeitschrift für Ethnologie*, by the same society.

Journals of a popular character which can not be neglected are: *Academy*, London; *The American Naturalist*, New York; *Athenæum*, London; *Ausland*, Stuttgard; *Nature*, London; *Popular Science Monthly*, New York; *Révue Scientifique*, Paris; *Science*, New York.

II.—BIOLOGICAL ANTHROPOLOGY.

This enormous subject, covering practically the whole of the structural part of anthropology, is amply represented in a few publications. For titles alone the Index Medicus and the Index Catalogue of the Surgeon-General's library are the best guides accessible to Americans.

In England this part of the subject is most elaborately worked out in the biological and zoölogical journals. The Paris *Bulletins*, the German *Archiv* and *Zeitschrift*, the Italian *Archivio*, and the Austrian *Mittheilungen*, though covering the entire science, are specially rich and full in biological matters. With the original papers, accounts of meetings, reviews of publications and bibliography there is little more to be desired either for the beginner or for the advanced student.

Dr. Frank Baker devoted his vice-presidential address before Section H of the American Association to the organs of the human body that point to a past condition much lower than the present;—indications of the pathway by which humanity has climbed from darkness to light, from bestiality to civilization. These organs are of two kinds, those that added or improved and those that are taken away or atrophied. Those specially mentioned are connected with the modifications of the limbs, with the erect posture, and with the segmentation of the body.

In the hand the special flexor muscle of the thumb is a new element, while the palmaris longus is in the category of disappearing muscles. The torsion of the humerus and the incurvation of its trochlear surface and the scapular index all show a progressive development both in the individual and in the race.

The palmar fascia, the epitrochles-anconeus, a process resembling the supra-condyloid foramen of marsupials, the perforation of the olecranon fossa remind of primitive conditions. While the region of the hand and fore arm indicates increase of specialization, the upper part of the limb generally testifies to a regression. This principle is illustrated by examples. The hind limbs of apes as compared with the human legs and the acquisition of the erect posture are closely examined. Upon the latter point Dr. Baker summarizes the evidences that the adaptation of man to the erect posture is yet far from complete.

These resemblances with anthropoid apes are held to indicate not lineal descent, but common ancestry, and the differences in the races of mankind do not justify our separating them on structural grounds.

In his work on races and peoples Dr. Daniel G. Brinton summarizes the physical characteristics used in classification of mankind:

SCHEME OF PRINCIPAL PHYSICAL ELEMENTS.

Skull	Dolichocephalic	long skulls.
	Mesocephalic	medium skulls.
	Brachycephalic	broad skulls.
Nose	Leptorhine	narrow noses.
	Mesorhine	medium noses.
	Platyrhine	flat or broad noses.

Eyes	Megaseme	round eyes.
	Mesoseme	medium eyes.
	Microseme	narrow eyes.
Jaws	Orthognathic	straight or vertical jaws.
	Mesognathic	medium jaws.
	Prognathic	projecting jaws.
Face	Chamæprosopic	low or broad face.
	Mesoprosopic	medium face.
	Letoprosopic	narrow or high face.
Pelvis	Platypellic	broad pelvis.
	Mesopellic	medium pelvis.
	Leptopellic	narrow pelvis.

On the 13th of March Mr. J. Venn gives in *Nature* the results of a series of measurements made upon the students of Cambridge University, in England. The following queries are put, according to Dr. Galton's system: (1) The distance of the clearest vision, (2) traction upon the dynamometer, (3) force of pressure by the hand, (4) volume of the head, (5) capacity of pulmonary inspiration, (6) stature, (7) weight of the body. The most interesting result relates to the head, which is found to be larger in volume in the better students, and in all classes to increase up to the age of 25. Into comparison with this study may be brought that of Pauline Tarnowsky upon 150 prostitutes, 100 female thieves, 100 peasants, and 50 women of culture:

	Prostitutes.	Thieves.	Peasants.	Cultured.
Antero-posterior diameter and transverse maximum divided by 2	160.3	161.6	163.2	164.2
Horizontal circumference	531.6	535.5	537.0	538.0
Frontal diameter	137.5	138.6	139.2	145.9
Cephalic index	80.0	80.2	79.9	79 1
Stature	153.5	155.6	156.4	154.1

The vexed question at this moment in the science called criminology is whether there is an ensemble of characteristics which consign their possessor to a life of crime, or which may be used to distinguish different sorts of criminals. In some form the Italian school are committed to this doctrine, and are more or less opposed by the French school.

In 1889 Dr. N. Anoutchine, of Moscow, published an elaborate work on stature of men in Russia compared with that of other nations. An excellent summary of this monograph is given in L'Anthropologie (I, 62–74), with chart and map. Every work of importance on human biology is noted in the Index Medicus, published by Dr. J. S. Billings and Dr. Robert Fletcher, of the Surgeon-General's Office, in Washington. The permanent record of this literature is to be found in the Index-Catalogue of the Surgeon-General's Office. Further important works are the following: Anthropometric Identification of Criminals, Bertillon; Anthropometry, Galton, Hurd; Ascent of Man, Baker; Cerebral Convolutions, Turner; Chest Development in Young Persons, Berry; Color of Skin in Oriental Races, Beddoe; Corsets, Robin; Cross-

Infertility, Gulick; Evolution and Disease, Sutton; Evolution of Sex, Geddes, Ryder; Giants, Laloy; Heredity, Hutchinson, La Pouge, Turner, Weismann, Thompson, Stoller; Human Selection, Wallace; Hypertrichosis, Jaws and Teeth, Talbot; Longevity and Climate, Remondino, Humphrey; Olecranon Perforation, Lamb; One-sided Occupation, Müller; Orbitomaxillary Suture, Thoms; Paternal Impressions, Bullard; Physical Proportions, Greenleaf, Bellary; Physiological Selection, Romanes; Physique of Women, Bowditch; Pigment in the Negro, Morison; Right handedness, Baldwin; Rumination, Einhorn; Sex, Wallian; Skull of Charlotte Corday, Topinard, Benedikt; Tailed Men, Schaeffer; Teeth of Prehistoric Skeletons, Ward; Weight of the Human Body, Ranke.

III.—PSYCHOLOGY.

In the science of anthropology, psychology is the application of measures to the activities of the mind through its material agency, the brain and the nervous system. The two sets of phenomena, those of the normal mind and healthy brain and those of the abnormal mind, are included. The former find their able organ in the *American Journal of Psychology*, Worcester, Massachusetts, and the latter phenomena are treated in the journals of neurology.

Abroad the greatest activity prevails in this department of research. Wundt's *Studien*, Dubois-Reymond's *Archiv*, Pflüger's *Archiv*, most of the physiological journals, *Mind*, *Brain*, and even the periodicals devoted to criminology, must be consulted.

The *American Journal of Psychology* furnishes (III, 275–286) a report on the amount of psychophysical instruction in the following American institutions of higher learning: University of Wisconsin, University of Nebraska, New York College for the Training of Teachers, Columbia College, Harvard University, Yale University, Army Medical Museum, University of Pennsylvania, Indiana University, Clark University, and University of Toronto. In each case the instructors' names are given and a syllabus of the instruction. It would be well to repeat here, did space permit, these curricula, to mark the present position of this branch of anthropology. It will suffice to append Dr. J. McK. Cattell's account of work done in the psychological laboratory of the University of Pennsylvania.

"Special courses in psychology were given at the University of Pennsylvania by Professor Fullerton and Prof. James McKeen Cattell. Professor Fullerton delivered two courses—one for undergraduates, the other for graduate students. In these courses special stress is laid on psychological analysis and those regions of psychology which border on the theory of knowledge. Professor Cattell gave three courses extending through the year—an introductory course in experimental psychology, a course beginning with the special study of some psychological problem and taking up in the second half year comparative, social, and

abnormal psychology, and an advanced course in physiological and experimental psychology. These courses include either practical work or research on the part of the student. A lecturer on philosophy and an assistant in psychology are about to be appointed, and additional courses will be given next year.

"In addition to these special courses, physiological, abnormal, and comparative psychology may be studied in the medical and biological departments of the university. These are probably without rival in America, and offer complete courses of lectures, practical work, and clinics. Psychology borrows from and lends to all the sciences. Every one of the large number of advanced courses offered by the university bears some relation to psychology, and may prove useful to the student. The asylums and hospitals will be found of special advantage to the student of psychology.

The new library building of the university is nearly completed. There is a special endowment for the purchase of philosophical and psychological books, and any books needed by students for special work will be obtained. The university press is about to begin the issue of a series of monographs representing work done in the fields of philosophy and psychology. The first number, now in press, is a psychological study on "Sameness and Identity," by Professor Fullerton. Following this number will be a series of researches from the laboratory of psychology and an edition of Descartes' "Meditations," with Latin and English texts and philosophical commentary."

Professor Cattell makes the following report of work done in the psychological laboratory. "The chief work before experimental psychology is the measurement of mental processes. As experimental physics is devoted to the measurement of time, space, and mass in the material world, so experimental psychology may measure time, complexity, and intensity in consciousness. In so far as cases are investigated in which one mental magnitude is the function of another, a mental mechanics is developed.

"The laboratory possesses apparatus, which measures mental times conveniently and accurately. This apparatus has been described in *Mind* (No. 42), but since then it has been improved. The chronoscope has been altered and a new regulator made, so that the mean variation of the apparatus is now under one-thousandth of a second. New pieces have been built for the production of sound, light, and electric stimuli. Apparatus for measuring the rate of movement and for other purposes have been added. The observer is placed in a compartment separated from the experimenter and measuring apparatus. With this apparatus researches are being carried out in several directions. Professor Dolley is measuring the rate at which the nervous impulse travels, using two different methods. In one series of experiments an electrical stimulus is applied to different parts of the body, and a reaction is made either with the hand or foot. The rate of transmission in

the motor and sensory tracts of the spinal cord has thus been determined. In a second series of experiments two stimuli are given at different parts of the body, and the interval between them adjusted until the observer seems to perceive them simultaneously. It is thought that these experiments will throw more light on human physiology than cases in which the nerve (motor only) of a partly dead frog is artificially stimulated. The times are also of interest to psychology, as they are needed in order to determine purely mental times. Mr. Witmer is measuring the personal difference in reaction-times, and the work will be extended to different mental processes. These times seem to vary with age, sex, nationality, education and occupation, and their study may have practical value as well as theoretic interest. Length of life should be measured by rate of thought. Experiments are also being made on the variation in the reaction-time from hour to hour and day to day. With the co-operation of Dr. Weir Mitchell and other eminent neurologists the alteration in the time of physiological processes in diseases of the nervous system is being studied. It is believed that such tests may be of use in diagnosis. The nervous impulse may be sent through the system in different directions until a relative delay discovers the diseased part. Recovery and progression may be studied by noting the alteration in time.

"Owing to the introduction of cerebral surgery and the advances recently made in the treatment of diseases of the nervous system, any method which may make diagnosis more exact deserves careful study. In addition to the time of physiological processes in disease, other tests of loss of sensation, power and intelligence, are made in the laboratory. The following ten tests are recommended; the methods, etc., are described in an article now in press for *Mind:* (1) Dynamometer pressure; (2) rate of movement; (3) sensation-areas; (4) pressure causing pain; (5) least noticeable difference in weight; (6) reaction-time for sound; (7) time for naming colors; (8) bisection of 50 centimeters line; (9) judgment of 10 seconds time; (10) number of letters remembered on hearing once. These determinations are made not only on those who are suffering from disease, but also on every one who wishes to be tested. It is hoped that the same tests will be made elsewhere, so that the results of a large number of observations may be compared and combined. The undergraduate students in experimental psycology undertakes a course of laboratory work in which about two hundred tests and measurements are made. It is hoped that when a sufficient mass of data has been secured, it will have some scientific value. In the cases of two of the tests given above, the rate of movement and the pressure causing pain, researches are being carried out in the laboratory. By altering the distance and nature of the movement, and the point of the body to which the pressure causing pain is applied, new quantitative results are obtained."

Professor Fullerton is carrying on a research to determine the rate

at which a simple sensation fades from memory. A stimulus is allowed to work on the sense-organ for one second, and after an interval of one second a stimulus slightly different in intensity is given for one second, and the least noticeable difference in intensity is determined by the method of right and wrong cases. The interval between the stimuli is then altered, and it is determined how much greater the difference between the stimuli must be in order that it may be noticeable. The rate of forgetting is thus measured in terms of the stimulus. Intervals varying from one second to three minutes have been used. For these experiments new apparatus was constructed, and it was discovered that when sensations of light are excessive and last for one second, the least noticeable difference in intensity is not about one one-hundredth, as is supposed, but much the same as for the other senses under like conditions. Other observations, such as the importance of keeping the time of stimulation constant, the stronger stimulus coming before or after the weaker, the degree of confidence, the personal and daily variation, etc., have made a new investigation of the least noticeable difference in sensation necessary. This is at present in progress, while further work on memory must wait for its completion. Mr. De Bow is in the meanwhile making experiments determining the time of stimulation giving the greatest accuracy of discrimination.

The rate, extent, and force of movement is the subject of a somewhat extended investigation, which will not be completed for some time. The maximum rate of movement has been noticed above. Experiments on the maximum pressure have been published, as also on extent of right and left handed movements. But the least noticeable difference in the rate, extent, and force of movement has never been studied in the same way as the least noticeable difference in passive sensation. Yet it would seem to need such study even more, owing to the importance and obscurity of the "sense of effort."

The laboratory possesses apparatus for studying the time, intensity, and area of stimulation needed to produce the just noticeable sensation and a given amount of sensation. These mental magnitudes are correlated so that one may be treated as the function of the other. The results of studying the relation of time to intensity have been published in *Brain* (pt. 31), it being found that the time colored light must work on the retina in order that it may be seen, increases in arithmetical progression as the intensity of the light decreases in geometrical progression. The relation of area to intensity and time is now being studied. Other experiments on the relation of intensity, time, and area of stimulation, as determined by the length of the reaction-time and accuracy of discrimination, have been begun.

The laboratory has a valuable collection of Koenig's apparatus for the study of hearing and the elements of music, and a spectrophotometer, a perimeter, and other pieces for the study of vision. Work on hearing and vision has been begun in several directions, but is at pres-

ent delayed for lack of workers. Some progress is, however, being made in studying the fusion of sensations of light, the laboratory possessing special apparatus by which colored surfaces of given areas may in any succession work on the retina for given times. Mr. Newbold, who has been helping with the experiments on memory, is about to begin a research on attention, and it is hoped that next year there will be others ready to undertake original work. Among the subjects for which apparatus has been secured and preliminary study has been made are: The building of complex perceptions, exertion, and fatigue, the measurement of contrast, the association of ideas, and subconscious mental processes.

Dr. Joseph Jastrow has prepared for the series of Fact and Theory Papers a small volume on the time-relations of mental phenomena. "The study of the time-relations of mental phenomena is important from several points of view. It serves as an index of mental complexity, giving the sanction of objective demonstration to the results of subjective observation; it indicates a mode of analysis of the simpler mental acts, as well as the relation of these laboratory products to the processes of daily life; it demonstrates the close inter-relation of psychological with physiological facts, an analysis of the former being indispensable to the right comprehension of the latter; it suggests means of lightening and shortening mental operations, and thus offers a mode of improving educational methods; and it promises in various directions to deepen and widen our knowledge of those processes by the complication and elaboration of which our mental life is so wonderfully built up. An excellent bibliography of well selected authorities relating to general psycho-physics, time-reactions, adaptive reactions, and association times will be found at the end of the volume. The *American Journal of Psychology*, edited by President Stanley Hall, and published at Clark University, Worcester, Massachusetts, is the standard authority on the physical side of psychology.

Metaphysical psychology, represented in the English publication *Mind*, may be said to have fairly entered the arena of anthropology since the revelations of consciousness are now subjected to experimental examination. The following topics show the range of study on both sides: Animal Intelligence, Alix, Foveau; Double Consciousness, Binet; Effect of Fatigue on Muscular Contraction, Lombard; Effect of Music on Animals, Stearns, Weissman; Experimental Psychology, Jastrow; History of Reflex Action, Hodge's Hypnotism, Felkin, Innes, Lays, Moll, St. Clair, Bonjean, and many others; Inhibition in the Phenomena of Conscience, Beriet; Intelligence of Animals, Corsetti; Mental Evolution, Varigny; Mental Tests, Cattell; Origin of Mind, Carus; Origin of Human Faculty, Romanes; Perception of Length and Number Among Little Children, Binet; Physiognomy and Expression, Mantegazza; Principles of Psychology, James; Psychic Life of Micro-Organisms, Beriet; Psychic Time Measures, Fricke; Psychology of

Attention, Ribot; Relation of Mind to Its Physical Basis, Cope, Salter; Sense of Direction in Animals, Lubbock; Space Consciousness, Spencer.

IV.—ETHNOLOGY.

Since the dividing lines between races have come to be drawn upon color rather than upon osteology, much ingenuity has been expended in devising a scheme of colors. Broca's standards, published in the first edition of the British Association "Anthropological Notes and Queries," are well known. They appear also in the French "Queries." Dr. Beddoe, president of the London Anthropological Institute, has further studied these Broca standards and makes the following subdivisions:

(1) Red (including pink) passing through reddish brown towards black.

(2) Orange, or reddish yellow, passing through brown towards black.

(3) Yellow, passing through yellow brown and olive brown toward black.

(4) Gray or cendre, darkening to black.

Dr. Beddoe presents an ingenious table, in which the proportions of these colors are given for people that he has specially examined.

Elements of color—Decads.

	Gray.	Yellow.	Orange.	Red.
Chinese	2.7	3.9	0.2	3.0
N. Guinea	0.3	0.5	2.5	6.7
N. Hebrides	....	....	4.0	6.0
Maoris, adult	....	0.7	5.0	4.2
Maoris, children	....	....	4.1	5.8
Australians	....	....	5.4	4.4
Melaneseans	....	....	4.1	5.8
Cingalese	....	....	6.2	3.7
Gujeratis, } After exposure	....	....	6.2	3.8
Goanese, } After exposure	....	1.0	5.0	4.0

This is followed by a more extended table, in which the proportions of Broca's numbers entering into each skin color are given. The notable differences of form existing between the parts of the skeleton and the other profound portions of the body in different groups of mankind seem to have been produced antecedent to these migrations and separations which have brought about race distinctions at present, such as color of skin and eyes and texture of the hair.

Dr. Daniel G. Brinton has published a volume on Races and Peoples, in which he combines the results of a course of lectures before the Academy of Natural Sciences of Philadelphia. This volume supplies a vacancy previously existing, since there was no good summary of ethnology published in English giving the results of modern research. The peculiar doctrine of the author is the location of man's origin in southwestern Europe and the parts of Africa opposite, both on zoölogical and archæological grounds. His classifications of mankind, though agreeing essentially with those of other recent systematists, possess

sufficient interest to be repeated, since they grow somewhat out of Dr. Brinton's theory concerning man's cradle land. They are reproduced here in order to enable the reader to compare them with those of Welcker, Topinard, Hæckel, Müller, Flower, Quatrefages, and others in preceding summaries of the Smithsonian Report:

General ethnographic scheme.

Race.	Traits.	Branches.	Stocks.	Groups or peoples.
Eurafrican	Color, white, Hair, wavy and Nose, narrow.	I. South Mediterranean.	1. Hamitic	1. Libyan.
				2. Egyptian.
				3. East African.
			2. Semitic	1. Arabian.
				2. Abyssinian.
				3. Chaldean.
		II. North Mediterranean.	1. Euskaric	Euskarian.
			2. Aryac	Indo-Germanic or Celtindic peoples.
			3. Caucasic	Peoples of the Caucasus.
Austafrican	Color, black or dark, Hair, frizzly and Nose, broad.	I. Negrillo	1. Central African	Dwarfs of the Congo.
			2. South African	Bushmen, Hottentots.
		II. Negro	1. Nilotic	Nubian.
			2. Soudanese.	
			3. Senegambian.	
			4. Guinean.	
		III. Negroid	1. Bantu	Caffre and Congo tribes.
Asian	Color, yellow or olive; hair, straight and Nose, medium.	I. Sinitic	1. Chinese	Chinese.
			2. Thibetan	Natives of Thibet.
			3. Indo-Chinese	Burmese, Siamese.
		II. Sibiric	1. Tungusic	Manchus, Tungus.
			2. Mongolic	Mongols, Kalmucks.
			3. Tartaric	Turks, Cossacks.
			4. Finnic	Finns, Magyars.
			5. Arctic	Chukchis, Ainos.
			6. Japanese	Japanese, Koreans.
American	Color, coppery, Hair, straight or wavy, and Nose, medium.	I. Northern	1. Arctic	Eskimos.
			2. Atlantic	Tinneh, Algonkins, Iroquois.
			3. Pacific	Chinooks, Kolosh, etc.
		II. Central	1. Mexican	Nahuas, Tarascos.
			2. Isthmian	Mayas, Chapanecs.
		III. Southern	1. Atlantic	Caribs, Arawaks, Tupis.
			2. Pacific	Chibchas, Quichuas.
Insular and littoral peoples.	Color, dark Hair, wavy or frizzly, and Nose, medium or narrow.	I. Nigritic	1. Negrito	Mincopies, Aetas.
			2. Papuan	New Guineans.
			3. Melanesian	Feejeeans, etc.
		II. Malayic	1. Malayan	Malays, Tagalas.
			2. Polynesian	Pacific Islanders.
		III. Australic	1. Australian	Australians.
			2. Dravidian	Dravidas, Mundas.

—Brinton, D. G., Races and Peoples, New York, 1890, p. 99.

Scheme of the Eurafrican race—North Mediterranean branch.

[Tribes in *italics* are extinct.]

Stock	Group	Tribes
I. Euscaric stock	1. Euscaric group	Euscaldonac, Basques, *Sards*, *Siculi*, *Aquitanians*, *Picts*, (?) *Ligurians*, (?) *Cantabrians*.
II Aryac stock	1. Celtic group	*Gauls*, Highland Scotch, Irish, Welsh, Manx, Bretons, *Celtiberians*, *Cymri*.
	2. Italic group	*Latins*, *Umbrians*, *Oscans*, *Sabines*, Italians, French, Spanish, Portuguese, Roumanians, Wallachians.
	3. Illyric group	*Illyrians*, Albanians, *Thracians*, *Japyges* (?).
	4. Hellenic group	*Pelasgi*, *Phrygians*, *Lydians*, *Macedonians*, Greeks.
	5. Lettic group	Letts, Lithuanians, *Old Prussians*.
	6. Teutonic group	*Goths*, *Vandals*, *Franks*, *Angles*, *Saxons*, *Suevi*, Scandinavians, Germans, Danes, Dutch, English, Anglo-Americans.
	7. Slavonic group	Russians, Poles, Czechs, Servians, Croatians, Wends, Bulgarians, Montenegrins.
	8. Indo-Eranic group	Armenians, Persians, *Bactrians*, Hindoos, Kafirs, Dards, Beluchis, Hunzas, Gypsies.
III. Caucasic stock	1. Lesghic group	Avars, Laks, Udes, Kurins.
	2. Circassic group	Circassians, Abkhasians.
	3. Kistic group	Tush, Karaboulaks.
	4. Georgic group	Georgians, Mingrelians, Lazs.

—Brinton, Races and Peoples, New York, 1890, p. 140.

Scheme of Aryac migration.

[Extinct peoples in *italics*.]

		European.	Asian.
Primitive Aryans (Western Europe).	Northern Peoples (Blondes)	Letto-Lithuanians.	
		Teutons.	*Phrygians*.
		Slavonians.	*Cappadocians*.
		Thracians.	Armenians.
	Southern Peoples (Brunettes)	*Dacians*.	*Medes*.
		Hellenes.	Iranians.
		Illyrians.	Indians.
		Italians.	
		Celts.	

—Brinton, D. G., Races and Peoples, New York, 1890, p. 153.

Scheme of the European race—South Mediterranean Branch.

[Extinct peoples in *italics*.]

Stock	Group	Peoples
I. Hamitic stock.	1. Libyan group	*Numidians*, *Getulians*, *Libyans*, *Mauritanians*, *Guanches*, Berbers, Rifians, Zouaves, Kabyles, Tuareks, Tibbus, Ghadumes, Mzabites, *Ghanatas*, *Etruscans*, *Amorites*, *Assyrians*, *Hittites* (?).
	2. Egyptian group	Copts, Fellaheen.
	3. East African group	Gallas, Somalis, Danakils, Bedjas, Bilins, Afars, Khamirs.
II. Semitic stock.	1. Arabian group	*Himayarites*, *Sabeans*, *Nabotheans*, Arabs, Bedawin, Ehkilis.
	2. Abyssinian group	Ambarnis, Tigris, Tigrians, Gheez, Ethiopians, Harraras.
	3. Chaldean group	Israelites, Arameans, Samaritans.

Brinton, D. G., Races and Peoples, New York, 1890, p. 104.

Scheme of the Austafrican Race.

I. Negrillo branch..	1. Equatorial group......	Akkas, Tikkitikkis, Obongas, Dokos, Vonatoans, *Kimos* of Madagascar.
	2. South African group..	Bushmen, Hottentots, Namaquas, Quaquas.
II. Negro branch...	1. Nilotic group	Shilluks, Dinkas, Bongos, Kiks, Baris, Nuers.
	2. Sudanese group	Haussas, Battas, Bornus, Kanoris, Ngurus, Akras.
	3. Senegambian group...	Serreres, Banyums, Wolofs, Foys.
	4. Guinean group.........	Ashantis, Dahomis, Fantis, Yorubas, Mandingoes, Veis, Krus.
III. Negroid branch.	1. Nubian group.........	Nubas, Barabras, Dongolowis, Pouls, Tumalis, Nyam Nyams, Monbuttus.
	2. Bantu group..........	Caffres, Zulus, Bechuanas, Sakalavas, Damas, Herreros, Suahelis, Ovambos, Bassutos, Barolongs, Bengas, Duallas, Wagandas.

—Brinton, D. G., Races and Peoples. New York, 1890, p. 174.

Scheme of the Asian Race.

I. Sinitic Branch..	1. Chinese Group.........	Chinese.
	2. Thibetan	Thibetans, Ladakis, Nepalese, Bhotanese.
	3. Indo-Chinese Group....	Birmese, Siamese, Annamese, Cambodians, Cochin-Chinese, Tonkinese.
II. Sibiric Branch..	1. Tungusic Group.......	Tungus, Manchus.
	2. Mongolic Group	Mongols, Kalmucks.
	3. Tartaric Group	Turcomans, Yakouts, Turks (Osmanli), Usbeck, Kirghis, Cosacks, Huns.
	4. Finnic Group	Finns, Lapps, Esthonians, Ugrians, Magyars, Mordvins, Samoyeds, Ostyaks, Voguls, Livonians Karelians.
	5. Arctic Group	Chukches, Koraks, Kamschatkans, Namollos, Ghiliaks, Ainos.
	6. Japanese Group	Japanese, Koreans.

—Brinton, D. G., Races and Peoples. New York, 1890, p. 194.

Scheme of Insular and Litoral Peoples.

I. Negritic stock .	1. Nigrito Group	Mincopies, Aetas, Schobaengs, Mantras, Semangs, Sakaies.
	2. Papuan Group	Papuas, New Guineans.
	3. Melanesian Group	Natives of Feejee Islands, New Caledonia, Loyalty Islands, New Hebrides, etc.
II. Malayic stock..	1. Malayan Group.	Malays, Sumatrese, Javanese, Battaks, Dayaks, Macassars, Tagalas, Hovas (of Madagascar).
	2. Polynesian Group......	Polynesians, Micronesians, Maoris.
III. Australic stock.	1. Australian Group......	Tasmanians, Australians.
	2. Dravidian Group.......	Dravidas, Tamuls, Telugus, Canarese, Malayalas, Todas, Khonds, Mundas, Santals, Kohls, Bhillas.

Brinton, D. G. Races and Peoples, New York, 1890, p. 220.

It is not necessary to more than mention the essay at classification made by Dr. Lombard the preceding year and published in the Bulletins de la Société d'Anthropologie, Paris (XII, 129; 185). The author starts out with the hypothesis that the human species first appeared

in the circumpolar region during the Miocene epoch and that it expanded slowly and progressively over all the continents. As soon as the parts of this original group separated, races were formed which set up a movement from north to south, the more recent and better perfected driving before them the older and more degraded. Three primary races are demanded by this theory, and their modern representatives are to be seen in Tierra del Fuego, Cape Colony, and Tasmania or Australia.

The best journals on ethnography and ethnology are the organs of the great societies in England, France, and Germany. The geographic magazines and publications of all the societies devoted to geography can not be overlooked. While their ruling motive is the conquest of the world for civilization they do not fail to mention and describe the aborigines. The *Internationales Archiv für Ethnographie*, Leyden, edited by J. D. E. Schmeltz, is designed exclusively for museum directors who have in charge ethnographic material.

The difficulty still remains of confounding language with blood, in this area of anthropology, to such an extent that lists of tribes contain tongues, and *vice versa*. Trained ethnologists, however, make the proper distinction, and gradually the error will eliminate itself.

General works on Ethnology.—The beech tree in Ethnology, Taylor; Ethnography, Races and Peoples, Brinton; Ethnology in relation to races and peoples, Achelis; Geographic names, Hirrle, also Bulletin I, United States Board of Geographic names; Numeration in the light of ethnography, Günther, Reinach; Pygmies, Werner; Race and disease, Hoffmeister, Stokris; Race susceptibilities, Grieve: Teeth of different races, Belty.

America.—Age of puberty among Indians, Holder; Americanists, Brinton; Beothuks, Gatschet; Cherokees, Moony; Cherokees and Mound-builders, Thomas; Eskimo, Murdoch, Rink; Illustrated Americana, Hunnewell; Indians of Puget Sound, Eells; The Mexicans, Gooch, Seler; Northwest Coast tribes, Jacobsen; Omaha and Ponca Indians, Dorsey; Peopling of America, Quatrefages; South American Culture, Stübel; Tribes of Canada, Boas; Ethnography of Venezuela, Marcano; Western Denes, Morice.

Europe.—Aryan cradle-land, Glennie, Huxley, Taylor; Basques, Stoll; Ethnography of Europe, Lombard; Ethnography of Turkey, Garnett; Ethnology of British Isles, Rhys; Etruscans, Brinton, Bugge; Finland, Reuter; Germans and Slavs, Virchow; Lapps, Amich, Deniker, Khabouzine, Rabot; Origin of the English, Freeman; Prehistoric races of Italy, Taylor; Russia, Stuart; The Slavs, Hellwald; Stature in Russia, Anoutchine; Tartars in the Crimea, Deniker.

Asia.—Annametes, Deniker; Anthropology in India, Ibbetson; Armenia, Lanin; Asia Minor, Bent; Cambodia, Combette; Caucasus, De Morgan; China, Gordon, Tcheng; Cochin China, Combette, Faure; Ethnography of Western Asia, Lombard; History of Israel, Renan;

India, Tavernier; Indo-China, Rosset; Japanese studies, Remy; Kirghiz, Khabouzine, Kurds and Yesides, Kovalewsky; Thibet, Delbard, Rockhill, Sandberg.

Africa.—Angolese, Topinard; Bantu stock, Haarhoff; Congo tribes, Stanley (the Stanley literature in geographic journals and scientific periodicals), Ward; Dahomy, Delbard; Gaboon, Delbard; Madigascar, Oliver; South African Ethnology, Macdonald.

Oceanica.—Australia, Porter, Howitt, Reclus; Borneo, Woodford; Indian Archipelago, Baron Hoevell; Flores and Celebes, Weber; New Caledonia, Combette; New Hebrides, Imhaus; Polynesian race, Fornander; Solomon Islanders, Woodford; Tasmania, Roth; Torres Straits, Haddon.

Prof. A. H. Keane, of London, prepared for Chambers' Encyclopædia, new edition, articles on ethnographic titles.

V.—GLOSSOLOGY.

The resources of linguistic studies in the United States are, on the classical side, represented by the *American Journal of Philology*, and on the ethnic side by the studies and publications of the American Oriental Society, by Dr. Daniel Brinton's American series, and by the collections of the Bureau of Ethnology in Washington.

Abroad, the list of philological journals is too long to reproduce; furthermore, in most of them language is studied quite apart from man who uses it. Trübners catalogues, not forgetting the Journal of the Royal Asiatic Society; Revue de Linguistique; Zeitschrift der Morgenländischen Gesellschaft, Lazarus and Steinthal's Zeitschrift and Friedlander's Catalogues must be consulted for works in special lines. The following papers may be consulted: Asiatic affinities of Malay languages, Wake; Blackfeet language, Tims; Category of Moods, Grasserie; Chinook jargon, Hale; Comparative Grammar, Grasserie; Eskimo Vocabularies, Wells; Ethnographic basis of Language, Leitner; Evolution of Language, Murphy; Gothic languages, Balg; Indo-European linguistics, Regnaud; Language of the Missisaguas, Chamberlain; Manual of Comparative Philology, Schrader; New Linguistic Family, Henshaw; Phonograph in the Study of Songs, Fewkes; Poul language, Tautain; Science of Langaage, Sayce; Semitic languages, Wright; Textes Manchu, Bang; Timucua text, Gatschet; Tupi language, Dom Pedro; Zulu Dictionary, Manner.

VI.—TECHNOLOGY.

Klemm's plan of tracing out the lineage and migrations of human inventions, perfected later by General Pitt-Rivers, is really the most productive of scientific results among ethnologic methods. The study of an art in its historic elaboration may be called technography and the tracing of an art through the tribes that practice it ethnotechnics. At any rate, every year some one among the host of anthropologists gathers the specimens and the evidence to show how one of our well known

implements, processes, or art products has come to be what it is. The following is a good example of this: A symposium was held by the Anthropological Society of Washington to study the arrow-maker's art. Six members made communications and their results are published in the *Anthropologist.* Each reader was an expert in his field, so that, practically, there is little more to be said on that subject. Illustrations of some of the methods are to be found in the Reports of the Smithsonian Institution, but the perfecting of the point is shown only in the *American Anthropologist* and is reproduced here to give the subject a wider circulation. The steps are as follows: (1)

FIG. 1.—Free hand or direct percussion.

Free hand or direct percussion; (2) direct percussion, manner of

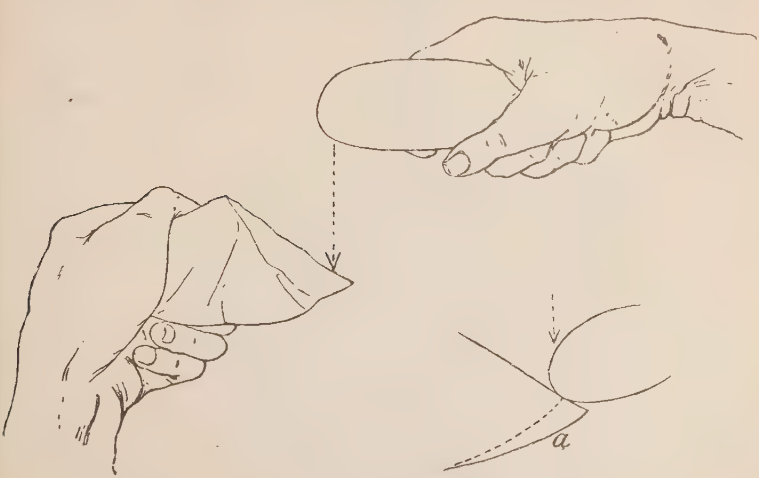


FIG. 2.—Direct percussion.

striking when the edge is sharp; (3) indirect percussion, practiced by

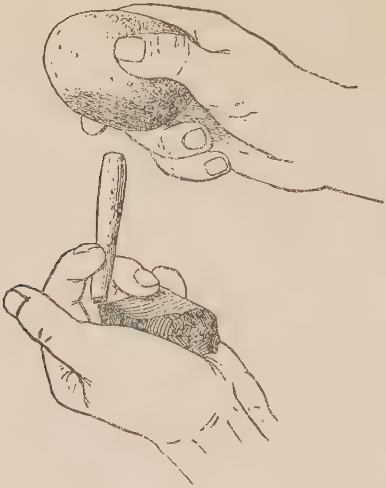


FIG. 3.—Indirect percussion.

the Wintuns and described by B. B. Redding; (4) indirect percussion

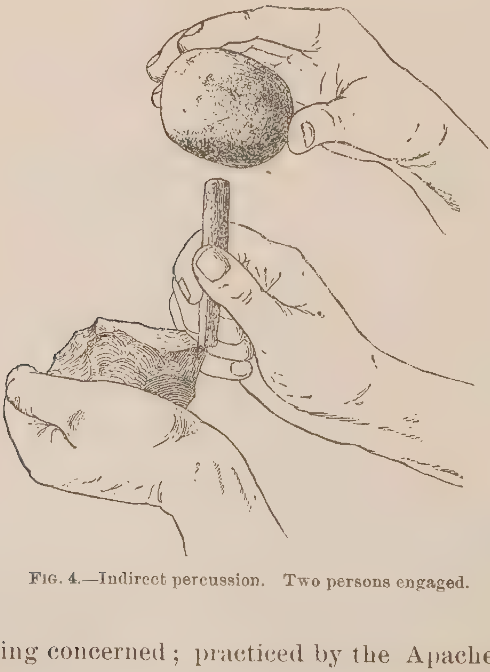


FIG. 4.—Indirect percussion. Two persons engaged.

two persons being concerned; practiced by the Apaches, according to

Catlin; (5) flaking by pressure, a bone implement being used, *a*, bone

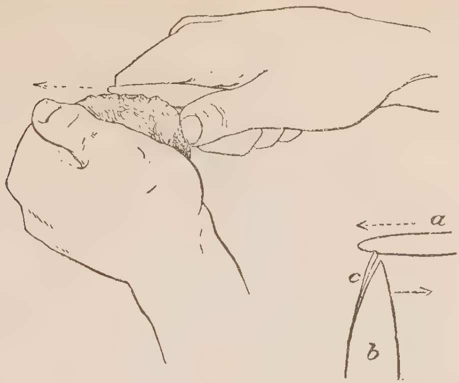


FIG. 5.—Flaking by pressure; a bone implement being used.

tool, *b*, the stone, *c*, the flake; (6) flaking by pressure; manner of

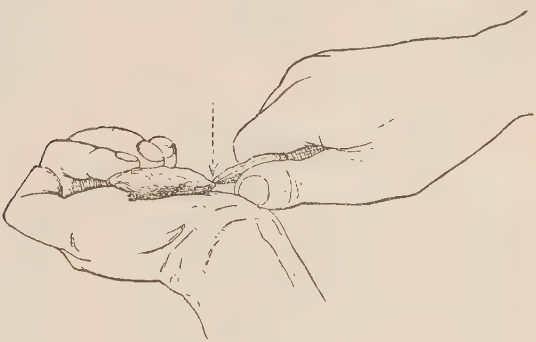


FIG. 6.—Flaking by pressure.

holding as observed among many tribes by J. W. Powell and others;

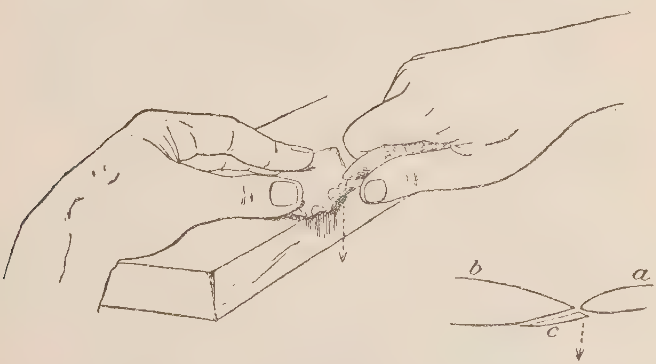


FIG. 7.—Flaking by pressure.

(7) flaking by pressure, a bone point being used, the implement to be

used resting on a support; (8) flaking by pressure, bone pincers being used.

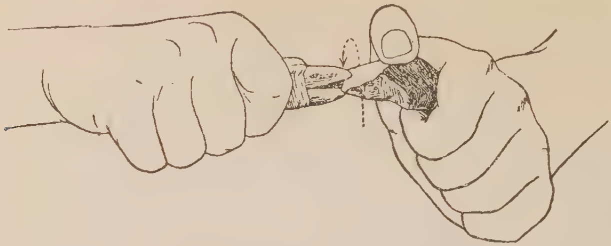


FIG. 8.—Flaking by pressure; bone pincers being used.

An excellent example of the study of art genealogically is Henry Balfour's description of the old British pibcorn or horn pipe and its affinities. (*J. Anthrop. Inst.*, London, XX, 142–154, 2 pl.) The family tree would stand thus:

Prototype.

(Cornstalk or slender reed with vibrating tongue.)

Single reed pipe with movable reed (*e. g.* Arab pipe).

Double pipes. (Arghool.)

Double pipes. (Zummarah.)

Single pipe with reed-cover and horn bell-mouth. (Pibcorn.)

Persian bagpipes. (nei ambánah.)

Double bagpipe form, with two horn bell-mouths. (Arab zouggárah.)

(Hindoo Magoodi, &c., types with horn bell-mouth.)

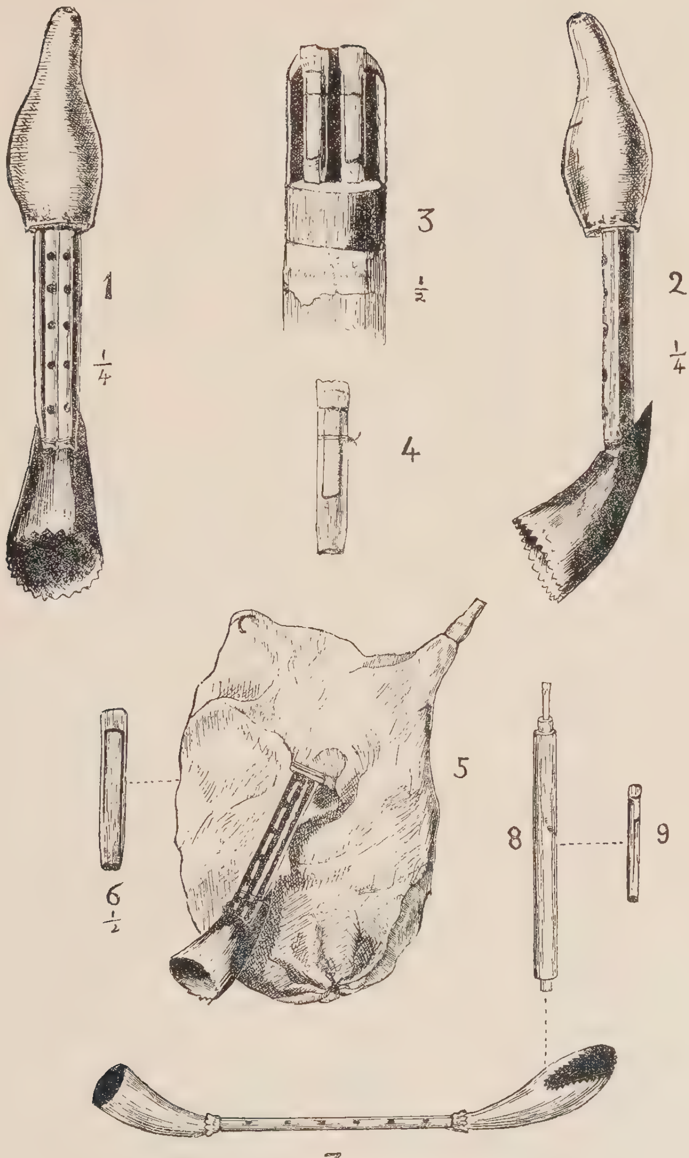
Double pipes with single bell-mouth. (Greek hornpipe.)

Greek bagpipes.

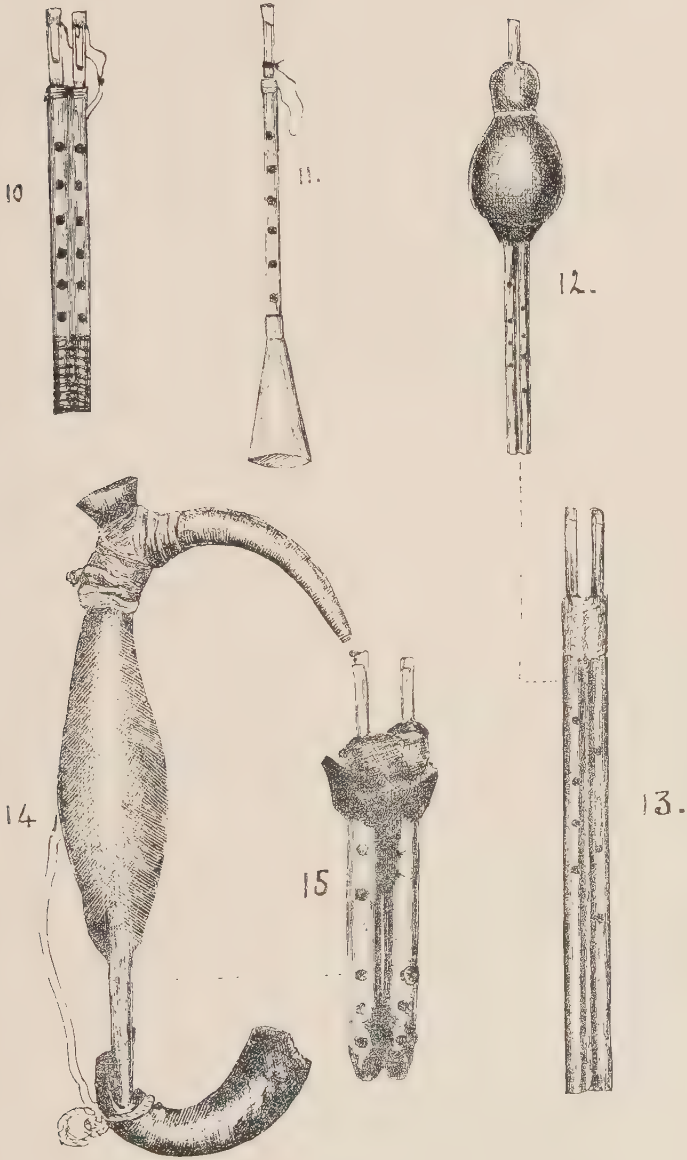
In the accompanying plates the relationships are better presented to the eye. They are marked III and IV.

EXPLANATION OF HENRY BALFOUR'S PLATE III.

Fig. 1. Double hornpipe, from the village—dio Maria, Tenos, Grecian Archipelago.
Fig. 2. Side view of same.
Fig. 3. Upper portion of same, with gourd mouth-piece removed, showing reeds.
Fig. 4. One of the sounding reeds removed.
Fig. 5. Bagpipes from the Grecian Archipelago.
Fig. 6. One of the sounding reeds removed.
Fig. 7. Pibcorn from the island of Anglesea.
Fig. 8. Back view of the pipe, with end pieces removed, showing reed *in situ*.
Fig. 9. Sounding reed of same.



HORNPIPE AND BAGPIPES, GRECIAN ARCHIPELAGO; AND PIBCORN FROM ANGLESEA.



ARAB REED PIPES, DECKHAN PIPES, AND HINDOO HORNPIPE.

EXPLANATION OF PLATE IV.

Fig. 10. Double reed pipes, Zummarah, Arab, from Egypt.
Fig. 11. Single reed pipe from Egypt.
Fig. 12. Double pipes, Toomeri, Deckan, India.
Fig. 13. Same, with gourd removed, showing sounding reeds *in situ*.
Fig. 14. Hindoo "horn-pipe" with double pipes and large gourd reservoir.
Fig. 15. same, with gourd and horn bell mouth removed, front view, showing sounding reeds *in situ*.

Mr. Walter Hough, of the U. S. National Museum, in a very elaborate manner, worked out the primitive methods of fire-making, so that he is much better acquainted with the art than any savage ever was. The geographic distribution of each form is interesting in the light of ethnography, and the gradual elaboration of this primitive art up to the last century an instructive chapter in the growth of invention.

Barr Ferree, of the Leonard Scott Publishing Company, wrote a series of articles on the influence of climate and nature in giving shape and character to primitive architecture. The subject is one of great interest.

J. E. Watkins, of the U. S. National Museum, follows the historic method in tracing the progress of the carrying industry and the elaboration of modern engineering.

W. H. Holmes publishes in the annual report of the Bureau of Ethnology a paper on the evolution of ornament, based on the close study of a large series of aboriginal pottery, basketry, and other fabrics. It is shown that many of the patterns which have had the greatest popularity in the world originated among primitive peoples. A list of important papers follows: Aboriginal Fire-making, Hough; Artistic Anatomy. Richer; Boomerangs, Baker; British Pibcorn, Balfour; Cats from Bubastis, Virchow; Climate and Architecture, Ferree; Culture Plants, Richter; Currency and Measures in China, Morse; Dawn of Metallurgy, Mello; Evolution of the Gondola, Pierson; Evolution of Ornament, Holmes; Fortification, Clarke; Garden Vegetables, Sturtevant; Industrial Arts in India, Birdwood; Japanese Pottery, Bowes; Maple Sugar, Henshaw; Mechanic Art in the Stone Age, Hayes; Music in New Hebrides, Hagen; Musical Notation in the Middles Ages, *sub voce;* The Nephrite-jadeite question, Berwerth; Origin of Bronze, Wilson; Origins of Technology, Espinas; Primitive Surgery, *sub voce;* Proas, Sturtevant; Quarry Workshop in the District of Columbia, Holmes; Sources of Jade, Pierce; Swords, *sub voce;* Throwing Spear, Nuttall; Trade route from Peking to Kashgaria, Bell; Venezuela Pottery, Ernst; Wild Horse of Sungaria, Trouessart; Writing Materials and Books among the Ancient Romans.

VII.—ARCHÆOLOGY.

The two archæologies, classic and pre-historic, have for their official organ the *American Journal of Archæology and of the History of Fine Arts.* (Boston, Ginn & Co.) It speaks authoritatively for the Arch-

æological Institute of America and the American School of Classical Studies atAthens, whose headquarters are at Cambridge, Massachusetts. All branches of archælogy and art, oriental, classical, early Christian Mediæval, and American, find a medium of utterance in the *Journal.* The Institute welcomes to its membership all men and women who desire to aid and share in the advance of knowledge concerning the past of the human race.

American archælogy has its organs in the *American Antiquarian*, the *American Anthropologist*, the reports of the Peabody Museum, the publications of the U. S. Smithsonian Institution, and the U. S. National Museum, the series of publications issued by Dr. D. G. Brinton, and the transactions of local societies.

The Museum of American Archæology in connection with the University of Pennsylvania perfected its organization by publishing its first Annual Report, Vol. I, number 1, containing list of additions to the library, catalogue of accessions, and the first report of the curator, Dr. C. C. Abbott.

The subject of archæology has taken on a vigorous growth during the current year. Professor Putnam, of Harvard University and Peabody Museum, has carefully studied the prehistoric remains in the Ohio valley. In two papers in the *Century Magazine*, especially he has given in a brief space and in a popular manner the result of his minute examinations. Professor Putnam also prepared for the World's Fair Committee a comprehensive plan for an archæological and ethnographic exhibit. Over this department Professor Putnam will preside. The researches of the Peabody Museum explorations lead Putnam to the conclusion that the mound-builder was a short-headed Southerner; that his civilization was broken up by a long-headed Northerner, and that the Indian is the result of a mixture of these two.

The Hemenway southwest archæological expedition bore its first fruit in vol. v, of the Papers of Archæological Institute of America, American series. Mr. A. F. Bandelier contributes in this volume four papers upon the history of the Southwest, to wit: (1) A sketch of the knowledge which the Spaniards in Mexico possessed of the countries north of the province of New Galicia, previous to the return of Cabeza de Vaca, in the year 1536; (2) Alvar Nuñez Cabeza de Vaca, and the importance of his wanderings from the Mexican Gulf to the slope of the Pacific for Spanish explorations towards New Mexico and Arizona; (3) Spanish efforts to penetrate to the north of Sinaloa, between the years 1536 and 1539; (4) Fray Marcos of Nizza, and (5) the expedition of Pedro de Villazur from Santa Fé, New Mexico, to the banks of the Platte River, in search of the French and the Pawnees, in the year 1720.

William H. Holmes, of the Bureau of Ethnology, publishes the result of an extended exploration in a bowlder quarry near Washington City, at Piney Branch. This site turns out to be a veritable workshop, and

a careful study of the débris leads the investigator to the conclusion that the forms occurring here are not implements at all, but failures, which the savage artisan has thrown away. Mr. Holmes has been enabled to demonstrate this by learning the stone-chipper's art and actually repeating the steps in his processes. The value of this careful exploration lies in the assistance which it will lend to other archæologists who visit to review their own work with new light.

Archæologists will be pleased to learn that the Hon. Henry Shirley found in Pedro Bluff Cave, Jamaica, a cranium belonging to one of the aborigines who inhabited the island before the European conquest. It had been artificially deformed during infancy by the depression of the frontal region, or fronto-occipital compression with corresponding lateral expansion. The island of Jamaica has yielded a remarkably small number of evidences of aboriginal occupation.

Dr. Brinton prepared for his "Races and Peoples" a scheme of geologic time during the age of man in the eastern hemisphere, which is here re-produced.

Scheme of geologic time during the age of man in the eastern hemisphere.

Quaternary Diluvial, or Pleistocene Epoch.	1. Preglacial	Europe connected with Africa. Temperature mild. African elephant in England. Tropical animals abundant.	Man homogeneous. Industry paleolithic, with simple implements. Migrations extensive. Language rudimentary.
	2. Glacial	Europe severed from Africa. Temperature low. Reindeer in France. Arctic animals abundant.	Man dividing into races. Industry paleolithic with compound implements. Cave dwellings. Migrations limited; races in fixed areas.
	3. Post-glacial	Continents assume present forms. Temperature rising. Temperate zones established.	Races completely established. Industry neolithic. Beginning of sedentary life. Languages developed in classes.
Present or Alluvial Epoch.	1. Prehistoric	Geographic conditions undisturbed. Wild animals not diminished.	Races develop into contact. Industry of stone and copper.
	2. Proto-historic	Conditions altered by agriculture. Wild animals slain or tamed.	Great migrations begin. Industry of bronze and iron.
	3. Historic	Geographic conditions greatly modified by man. All lower animals subjugated.	Extensive mingling of races. Development of nations

—Brinton, D. G., Races and peoples. New York, 1890, p. 96.

The eighth Russian Archæological Congress was held in Moscow, January 8 to 24. It was the twenty-fifth anniversary of founding the Royal Archæological Society in Moscow, February 7, 1864. The

occasion was one of great importance both socially and scientifically, as the following list of topics will show:

(1) Pre-historic antiquities.
(2) Historico-geographic and ethnographic antiquities.
(3) Monuments of fine arts.
(4) Customs and usages in Russia.
(5) Religious monuments.
(6) Russo-Slavic linguistic and paleographic monuments.
(7) Classic, Slavo-Byzantine and western antiquities.
(8) Oriental and heathen antiquities.
(9) Archæographic monuments.

There is an excellent report of this meeting in the *Mittheilungen*, Wien (xx, 148–164).

An event in archæology worthy of record in 1889–'90 was the removal of the National Egyptian Museum from Bûlâq on the east side of the Nile to the spacious Khedival palace at Gizeh on the western bank.

The death of Schliemann removed one of the most romantic characters in the scientific world. The conception of exploring the site of ancient Troy was formed in his boyhood. His assiduity in amassing a fortune to this end, and his untiring effort to spend his fortune to secure that end have held him up to the admiration of two generations. That his interpretation of his discoveries may not be in every case correct, will not detract greatly from his just meed of praise.

Archæological publications of general interest will be found under the following titles: Aboriginal Monuments in North Dakota, Montgomery; American Antiquities, Peet (under several titles); Antiquity of Man, White (series of papers on the Warfare of Science in *Pop. Sc. Monthly*); Antiquities of Tennessee, Thruston; Archæology, Powell; Archæology of India, Führer; Archæology of Ohio, Putnam; Bronze Age, Montelius; Cliff Dwellings, Chapin, Mearns; Discoveries in Egypt, Edwards, Brugsch, Naville; Fort Ancient, Ohio, Moorehead; French Archæology, Mortillet; Gashed Bones and the Antiquity of Man, Hughes; Oriental Archæology, Sayce; Prehistoric Anthropology, Wilson; Prehistoric Cave dwellings, Bickford; Stone Age in Africa, Andree; Winnipeg Mound Region, Bryce.

VIII.—SOCIOLOGY.

In December of 1889, the American Academy of Political and Social Science was organized in Philadelphia under the most favorable auspices. The list of subscribing members reached the number of 800 in the first six months of the Academy. The most distinguished university presidents and professors are among the governing body. This coöperative action marks an era in a branch of anthropology hitherto difficult to summarize. The resources of sociological study are unlimited.

Census reports, tables of vital statistics, blue books, literature of the Bureau of Labor, of interstate commerce, of education, Johns Hopkins tracts on historical and political science; the great reviews, all of them; the daily press are only a few of the great organs of sociology. The existence of a national society with an official organ will enable the specialists to cull from this great mass the publications in his line of study.

Anthropology comes to the aid of justice in the success of the Bertillon method of measuring and identifying criminals. This has found favor not only in all France, but in the United States, and even in the Argentine Republic. To the ordinary police questions of sex, height, age, and color of the eye are added the cephalic diameters, the length of the foot, length of the middle finger, length of the ear, length of the forearm, and personal scars or individual peculiarities. The many beneficial effects of the certain identification of a criminal, in spite of all aliases and disguises that have already been published, the ability to separate the first offense from the professional villainy, are not the least among the obligations society owes to anthropology.

The discussion still continues upon the subject whether there are certain morphological indications of criminal proclivities so marked that society may use them to protect itself by confining the subject before the crime may be committed.

The wide range of inquiry in the province of sociology is indicated in the following titles: Anthropology of Prostitutes, Tamousky; Artificial Deformation of the Head, Delisle, Nicolucci; Child Marriage in India, Brahmin; Chronology of China, Gordon; Communism, Laveleye; Comparative Criminality, Tarde; Courtesy, Mallery; Crime and Suicide, Corre; Criminal Anthropology, Garnier, Galton, Garofalo, Germa, Lombroso, Paravant, Ellis, Proal; Disposal of the Dead, Taylor; Duk-Duk Ceremonies, Churchill; The Ear as a Sign of Defective Development, Warner; Ethical Problem, Carus; Evolution and Inheritance, Eimer; Gentile System of the Navajos, Matthews; Government, Huxley; Infancy of Criminals, Taverni; Infant Marriages in India, Fawcett; Japanese Women, Loti; Judicial Dictionary, Stroud; Judicial Torture, Gundry; Justice and Political Ethics, Spencer; La Couvade, Meyners; Masks, Boas, Meyer; Marriage and Heredity, Nisbet; Marriage Relation, Wake; Mutual Aid Among Animals, Krapotkin; North American Indian Children, Pajeken; Origins of Common Law, Pollock; Police Anthropometry, Spearman; Political Evolution, Letourneau; Polyandria, Raynaud; Primitive Fashions, Basu; Primitive Games, Thurn; Province of Sociology, Giddings; Racing in 1890, Stutfield; Society Among Animals, Girod; Student Life in Paris in the Twelfth Century, Francke; Survival of Ancient Custom, Gomme; Tattooing in Tunis, Bazin (also *sub voce*); Thief Talk, Wilde; Trephined Crania, Verneau; Young Parisian Criminals, Jolly, Roux.

IX.—RELIGION AND FOLK-LORE.

One of the remarkable results of coöperation in the study of folk-lore is seen in the possibility of such a work as Professor Frazer's Golden Bough. The priest of Diana, near Aricia, took office after killing his predecessor. Before doing this the candidate was obliged to break a bough from a sacred tree in the grove, identified with the Golden Bough plucked at the Sibyl's bidding by Æneas before entering upon his journey to the world below. The two questions, why was the priest obliged to kill his predecessor? and why, before killing him, was he obliged to pluck the Golden Bough? drive the author to consult the whole body of knowledge recently accumulated in comparative religion. The lower forms of animisom are quite familiar to Professor Frazer, who explored them in the preparation of his well-known work entitled Totemism.

Sir Monier Williams has placed within the reach of English-speaking people a study in comparative religion in his work on Buddhism in its connection with Brahmanism and Hinduism and in its contrast with Christianity. There is no better example of the amenability of such matters to scientific treatment than is furnished by Buddhism. At first it was not a religion at all. It recognized no spirit world; it had no ecclesiastical organization, no places of worship, no cult whatever. Out of itself partly and in its association with surrounding religions it became, in the north especially, the most complicated and exacting of cults founded upon spirit worlds of countless number, of every variety of inhabitants intimately associated in every conceivable way with the people of the earth. The study of Buddhism is a chapter in the natural history of religion.

The American Folk-lore Society held its annual meeting in Columbia College, New York, under the presidency of Dr. Daniel G. Brinton. The report of the council gave the most flattering account of the prosperity of the organization. A movement was made toward enlarging the scope of the society's publications.

The folklorist needs no better guide than the *Journal of American Folk Lore*, edited in Cambridge, Massachusetts, by W. W. Newell. Original papers of great merit fill the body of the numbers, but reviews of current literature and a list of all publications upon the subject put the student at once into communication with his colleagues.

In the same manner the *Révue de l' Histoire des Religions*, published under the auspices of the Musée Guimet, in Paris, takes notice of all current literature on the natural history of religions. It is a guide book to this branch of science. During the current year this periodical enters its twenty-first volume. The Annales du Musée Guimet are devoted to memoirs too long and technical for the *Révue.*

Mr. Francis C. Macauley, of Philadelphia, has conceived the idea of a folklore museum. In pursuance of his suggestions Mr. Culin published a paper in the *Journal of American Folk-Lore*, and organized a

department devoted to this subject in the Museum of Archæology of the University of Pennsylvania.

Attention is called to the following titles: Aryan Cosmogony, Veckenstedt; Buddhism, Griffin, Williams; Comparative Religion, Frazer; Diabology, Jewett; Evolution of a Sect, McGee; Folklore, Newell; Humanities, Powell; Mythology of the Menomoni, Hoffman; Natural Religion, Müller; Polytheism in China, Lyall; Prayer Among the Hindus, Roussel; Primitive Religion, Schurtz; Religion of the Semites, Lloyd; Taoist Religion, Benton.

X.—MAN AND NATURE.

Prof. N. S. Shaler published a series of papers on America in its relation to civilization, including aboriginal life as well as that of the white race. One of the most interesting chapters in this study is that which relates to the change from agricultural to hunting life wrought in the aborigines by the invasion of the buffalo; or, rather, it might be called the reciprocal action of buffalo and Indian. The burning of forests encouraged the growth of grass; this invited the buffalos; they enticed the farmer from his stone hoe and laborious husbandry to take up the spear and the bow. Meat was easier to procure than corn; furthermore, the buffalo destroyed the corn and left the farmer nothing else to do but to pursue the occupation of Nimrod.

M. Marcelin Boule brought together in *L'Anthropologie* (I, 89–103) a series of reviews on quaternary geology in its relation to the antiquity of man. This list includes Forsyth Major, on the Mammalian fauna of the Val d'Arno (*Quart. J. Geol. Soc.*, Lond., XLI, p. 1); A. J. Jukes-Browne, on the Bowlder clays of Lincolnshire (*id.*, 114); Aubrey Strahem, on the Glaciation of South Lancashire, Cheshire, and the Welsh Border (*id.*, XLII, 369); R. M. Dilley, on the Pleistocene succession in the Trent Basin (*id.*, XLII, 437); J. Prestwich, on the Date, duration, and conditions of the glacial period, with reference to the antiquity of man (*id.*, XLIII, 393); T. Mellard Reade, on An estimate of post-glacial times (*id.*, XLIV, 291); Rev. O. Fisher, on the Occurrence of elephas meridionalis at Dewlest, Dorset (*id.*, XLIV, 818); J. R. Kilroe, on Direction of ice-flow in the north of Ireland (*id.*, XLIV, 827); James Croll, on Prevailing misconceptions regarding the evidence which we ought to expect from former glacial periods (*id.*, XLV, 220); J. Prestwich, on the Occurrence of palæolithic implements in the neighborhood of Ightham, Kent (*id.*, XLV, 270); Henry Hicks, on the Cac Gwyne Cave, North Wales (*id.*, XLIV, 561).

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A PRIMITIVE URN BURIAL.

By Dr. J. F. SNYDER, *Virginia, Cass County, Illinois.*

On the broad alluvial plain in the southeastern part of the State of Georgia, through which the Altamaha river takes its course to the sea, at a point a mile and a half north of that stream and nearly a mile from the Savannah, Florida and Western Railway, there is a small natural elevation of the ground rising a few feet above the general level of the river valley. On the top of this higher ground is one of the numerous Indian burial mounds of that region, measuring some 25 or 30 feet in diameter at the base and 8 or 10 feet high at its center. In February last (1890) in making an excavation in the western edge of this mound—not for archæological investigation, nor by archæologists,—a few inches below the surface the spade broke into a hollow, spherical-looking object that, on inspection, proved to be the round bottom of a large earthen pot which had been buried there bottom up. The solid, hard-packed earth in which it was imbedded was then carefully removed and the vessel was lifted out of its long resting place. Much to the surprise of the explorers another quaint earthen vessel was discovered within the larger one. This smaller one was standing upright on the natural surface of the ground, securely covered and inclosed by the large pot that had been placed inverted over it, affording it perfect protection from moisture as well as from the pressure of the earth forming the mound heaped over it (Fig. 1.) On examining the smaller vase it was found to be nearly half full of fine white ashes interspersed with calcined fragments of human bones, comprising the charred teeth and cremated skeleton of an adult individual. Lying on the surface of these remains were a quantity of small perforated bone beads (wampum), among which I discovered, uniform in size with the beads, several small pearls that had been pierced through the center for the purpose of stringing, with the beads, in the form of a necklace or other ornament. Whether the mound presented any peculiar features in its construction I have been unable to learn; and no further exploration of it has, to this time, been made.

The large pot, which I have succeeded in completely restoring (Fig. 2) is bell-shaped, quite symmetrical in proportions, and measures $15\frac{3}{4}$ inches in height and exactly the same in width across the mouth. It

is made of compact clay, unglazed, hard burned, and of the uniform thickness of a fraction more than the fourth of an inch. About the bottom, both inside and out, it presents by discoloration unmistakable evidence of having been subjected repeatedly to the action of fire, probably for cooking food. Its internal surface is very even and regular and has the appearance of having been smoothed by the hand, as finger marks are faintly discerned, particularly about the upper portion. The outside is roughened by being ornamented all over with a continuous repetition of the peculiar design shown in detail in Fig. 3, which doubtless was impressed upon the soft clay, before it dried, with a stamp cut in *intaglio*, thus leaving the figure on the vessel in relief, or "raised."

The smaller vase, in which the ashes of the dead had been deposited, is plain, smooth inside and out, glossy black in color, though not glazed; is thinner and more compact in texture than the large one, looking, at first glance, as if molded of papier mâché. It is free from ornamentation of any sort, and was burned hard after drying. In Fig. 4 it is represented, as is also the covering vase (Fig. 2), one-eighth actual size. Obtusely pointed at the bottom, of conoidal form, it rapidly enlarges to near the top, and contracts again for an inch and three-fourths to the mouth; graceful in contour, and almost mathematically true and regular in every proportion; it is 13 inches broad at the widest part, 11½ inches high, and 11½ inches across the opening. The fact that in each of these earthen vessels their height and diameter across the mouth are exactly equal in measurement may be only an accidental coincidence, but would seem to indicate that certain definite principles or rules in the plastic art guided the ancient potters in shaping their vessels.

We are reasonably sure that the wheel and lathe were unknown, as appliances in the manufacture of pottery, to the primitive American Indians. But they must have employed adequate substitutes for them; for without mechanical aids of some description the wonderful proficiency attained by some of the tribes in the ceramic art is difficult to explain. In the early settlement of the country, about the saline springs in Southern Illinois, Western Virginia, and other localities, numerous fragments of very large earthen vessels were found scattered about over extensive areas adjoining, many of them, when entire, 3 or 4 feet in length or in diameter and a foot or more in depth. They doubtless were made and used by the Indians as evaporating pans for obtaining salt from the salt-impregnated water of the springs. These rude earthen kettles were plain on the inside, but invariably bore on the outside the distinct impression of some kind of woven fabric. They excited the curiosity and astonishment of the backwoodsmen; and, at a later time, taxed the ingenuity of the scientist to discover the method by which the ancient artisan shaped and manipulated such unwieldy masses of soft clay and supported them in place while drying. This problem was solved satisfactorily a few years ago by Mr. George E. Sellers. In his valuable paper on "Aboriginal Pottery of

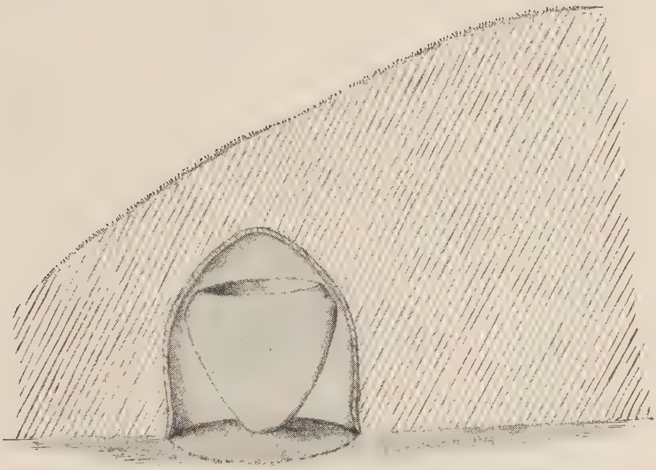

FIG. 1 ($\frac{1}{16}$).

FIG. 2 ($\frac{1}{8}$).

SNYDER: PRIMITIVE URN BURIAL.

FIG. 3. ACTUAL SIZE.

FIG. 4 ($\frac{1}{8}$).

SNYDER: PRIMITIVE URN BURIAL.

the Salt Springs in Illinois,"* he quoted the opinions of the late distinguished antiquarians, J. W. Foster, LL. D., and Dr. Chas. Rau, that "the earthenware has evidently been molded in baskets," an impracticable method because of the impossibility, as Mr. Sellers points out, of "keeping in form and lining with heavy clay fragile baskets of the large size of these old salt kettles." He then states, "I discovered (at the salt springs near the Saline River, in southern Illinois), what at first I took to be an entire kettle bottom up; but on removing the earth that covered it, it appeared to be a solid mass of sun-dried clay. From its position among heaps of clay and shells, its hard, compact, discolored—I may say almost polished—surface, I became satisfied it was a mold on which the clay kettles had been formed, precisely as in loam-molding at the present day." The soft clay was retained in proper position on the mold with bandages of coarse textile fabric that left their impression on the pottery, similar to the imprint that baskets of the same texture would make if the plastic clay had been pressed against their inner surface. This very simple method of casting the large salt kettles—on the outside of the pattern—was probably the same adopted in making the larger of the two pieces of pottery from the Georgia burial mound (Fig. 2.). In its construction the clay when soft must have had firm support on the inner side to resist the pressure necessary to imprint its exterior surface with the carved type. When dried sufficiently to retain its form the vessel may then have been lifted from its mold and smoothed on the inside with water and the open hand. In shaping the smaller vase the thin sheet of tough clay was no doubt taken off the molding block while yet pliable and its upper margin drawn in gradually by careful manipulation. A slightly wrinkled appearance of the indrawn margin of the opening bears evidence of this process.

There is no good reason for believing that these two pieces of earthenware were made purposely for the inhumation of the incinerated remains they finally inclosed, though they are in every respect so remarkably well adapted for that use. By placing the conical vase upright on a support a little more than an inch in thickness, and inverting the large pot over it, the receding rim of the vase exactly fits in the curving side of the pot, as is shown in Fig. 1; the one covering the other so accurately as to well nigh exclude the passage of air between the parts in contact. And this, I have been informed, was their relative position when recovered from the mound. This vase, though "new" and exhibiting no indications of previous use, is of a type—*Amphora*-like—quite common among the earthenware of the pottery-making pre-Columbian Indians. The large pot, or kettle, as before stated, bears evident marks of long-continued use in domestic or culinary service.

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* Popular Science Monthly, 1877, vol. II, p, 573, *et seq*,

The exterior ornamentation of this large kettle is shown in detail, drawn of actual size, by Fig. 3. The dark lines are in relief, standing out level with the original surface of the clay when molded; the white spaces show the face of the carving that sunk its impression, the sixteenth of an inch in depth, in the yielding mass. It will be observed in this unique design that a well-defined cross appears in each circle surrounded by the intricate scroll lines. The figure of the cross is by no means uncommon in the works of art of the ancient races inhabiting America before the historic period. It has been found fashioned in stone and copper, engraved on shell and bone, and in colors on pottery vases and bottles—not to mention the famous carving at Palenque. Its presence among the relics of the mounds has occasioned much speculation and discussion, in the main with no other basis than ludicrous flights of the imagination. At this late day the fanciful theory of a pre-Columbian propagation of Christianity in America by the Apostle Saint Thomas, supported at one time by such distinguished scholars as Professor Tiedemann, is scarcely worth a passing notice. "It has been shown by the preceding examples," remarked Dr. Chas. Rau,* which could be multiplied, if it were deemed necessary, that the cross was recognized as a symbol among the more advanced nations of America." Gomara says the knowledge of the cross as a religious emblem had penetrated all Spanish America before its discovery and conquest; and adds, "This veneration of the cross made them (the Indians) more ready to adopt the Christian symbol." This rubbish has vanished before the march of archæcological science, together with the grandeur and splendor of the "cultured, semi-civilized mound builders." The "sign of the cross," carved and sketched by the early mound-building Indians, is now properly considered a meaningless figure of ornamentation adopted by crude savages because of its simplicity of execution. This advanced opinion is well expressed by Prof. F. W. Putnam, in mentioning a copper ornament of cross shape found in an old Indian grave in Tennessee. He says: "I think it must be placed in the same category with the 'Tablet of the Cross' at Palenque, and be regarded as an ornament made in its present form simply because it was an easy design to execute, and one of natural conception."

Known instances of the preservation of *cremated* human remains in earthenware vases, by our prehistoric Indians, are very rare. It was the custom of some tribes to burn their dead collectively. "The practice of reserving the skeletons," says Col. C. C. Jones, LL. D.,† "until they had multiplied sufficiently to warrant a general cremation or inhumation seems to have been adopted." They were then burned all together and the seething pyre was covered with earth, heaped up into a mound, to be never again disturbed. "Burial vases," he adds, "inclosing human bones (not burned) have occasionally been found in the grave mounds of

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* The Palenque tablet, p. 42. † Antiquities of Southern Indians, p. 190.

Tennessee, Alabama, Florida, Mississippi and South Carolina." And he relates, on pages 455, 456 of his valuable work, a very interesting account of the recovery from a small shell mound on Colonel's Island, in Liberty County, Georgia, of a burial urn, inclosed in two others, containing the bones of a young child.

The urn-burial from the Altamaha mound—the subject of this paper—is original in design and remarkable for its ingenious simplicity. The pottery-ware is practically imperishable; and sealed almost hermetically, as were the ashes of the dead they contained, in that region where frost is scarcely known, they must have endured forever but for some convulsion of nature—or the implements of civilization. The presence, in the funeral vase, of small pearls with the wampum beads, and the chalk-like condition of both, attest the antiquity of this singular sepulchral deposit. Pearls were worn as personal ornaments in great profusion by the Indians of eastern Georgia when De Soto came among them, in 1540. The Gentleman of Elvas says that 14 bushels of them were found by the Spaniards in one charnel-house at Cofachiqui. Pickett remarks, in his History of Alabama, "There can be no doubt about the quantity of pearls found in this State of Georgia, in 1540, but they were of a coarser and less valuable kind than the Spaniards supposed. The Indians used to perforate them with a heated copper spindle, and string them round their necks and arms like beads."

Centuries have passed, with their ceaseless changes, since the hands of affection placed those venerated ashes of the dead and bead ornaments in that mound-covered crypt of clay pottery; and they who mourned on that occasion have, ages ago, been resolved into dust; but in these simple relics, they left—as legible as though graven in letters on polished marble—a record of their crude religious feelings, of their child like faith and reverence, and of their very human yearnings for life everlasting.

NOTE.—Since this paper was written the mound in which the pottery vessels and incinerated human remains were found has been thoroughly explored; and nothing further was discovered but a bed of ashes and charcoal on the ground surface in the center of the tumulus.

MANNERS AND CUSTOMS OF THE MOHAVES.

By GEORGE A. ALLEN, *Colorado River Agency, Colorado.*

Although the Mohaves are giving up many of their superstitions, some of them still cling to the teachings of their ancestors. They cremate their dead, the funeral pyre being made ready for the corpse as soon as life is extinct, and the body is placed on the pile of wood prepared, while all the friends and relations of the deceased gather around and set up a pitiful moan. Formerly they burned all the property of the deceased, and often the mourners would contribute everything they possessed to the flames, thereby showing the affection and grief they felt for the dead; but this custom is not much practiced at the present time. The women usually contributed a portion of their hair to the flames—that is, those who belonged to the immediate family of the deceased—and would even sometimes throw themselves on the fire, such was their grief.

While they have but little reverence, they believe there is a God, whom they call Mat-o-we-lia, and that He is the maker of all things; that He has a son, whom they call Mas-zam-ho, who is king of departed spirits. Mat-o-we-lia conducts the movements of the sun, moon, and stars; sends the rain, sunshine, etc. Mas-zam-ho has full charge of affairs in heaven, or "White Mountain," as they call it.

They believe the spirits of the dead go up to the "White Mountain" in smoke, and that all the property destroyed in the flames with the deceased will go with him to the "White Mountain," where pots are constantly boiling with something to eat.

They had formerly an annual burning of property, and all would contribute something to the flames in expectation of its going up to their departed friends. This practice is entirely discontinued on the reservation, but is still kept up by the Yumas at Fort Yuma, and by the Mohaves at Needles and Fort Mohave, off the reservation.

They also have a belief that all the Mohaves who die and are not cremated turn into owls, and when they hear an owl hooting at night they think it is the spirit of some dead Mohave returned. They are also superstitious about eating any kind of food that they are not accustomed to. They will not eat the meat of the beaver, claiming that if they did their necks would swell. This belief was brought about by

the circumstance of some one having poisoned beaver for their hides, and the Indians who ate of the flesh were poisoned and died; hence, they think all beavers are bad.

After one dies the friends do not eat salt nor wash themselves for four days. But these superstitions are fast disappearing, and in a few years most of them will have died out altogether. The medicine men are most instrumental in keeping them alive.

They formerly practiced polygamy, but this is now discontinued. Their marriage ceremony is a very simple one; they merely agree to live together as man and wife, seldom separating after such an agreement is formed.

They regard the hieroglyphics found on rocks as being the relics of some distinct race, of which they have no tradition whatever. Their animal nature, like that of all aborigines, predominates, and they are most happy and contented when they have plenty to eat. The children are rather bright and inclined to learn when their minds are not diverted by play. When allowed to recreate they play some kind of game from early morn until bed-time.

Some of the women do very artistic work in beads and pottery; they also weave matting from cottonwood bark. The mesquite bean is their principal food in winter; this they gather and put up in large willow baskets, which they place upon platforms for storage. The screw beans they put into a kind of kiln, and thus it goes through a sweating process before they are used. They have the metate for grinding wheat, corn, beans, etc.

Chief Hook-o-row is the head of the Mohave tribe, and he is a good, peaceable Indian, but not very progressive, being inclined to take life rather easy.

Like all Indians they have plenty of dogs, and will divide their last meal with them. The children are all called "Peet," until they arrive at about four or five years of age, when they are provided with a name.

They live in sweat-houses in winter and under open sheds in summer. Those who go to the railroad towns and mining camps soon become demoralized with whisky and contaminated by tramps.

With proper means of irrigation and instruction as to farming they would soon become a thriving community.

CRIMINAL ANTHROPOLOGY.*

By THOMAS WILSON, LL.D.

The First International Congress of Criminal Anthropology was held at Rome in 1885. It opened a new epoch in the history of crime. It was proposed to investigate crime scientifically, biologically, fundamentally; to investigate it in its origin, its causes; to determine, if possible, what share or proportion of responsibility therefor belonged to the criminal, and what to the public. As the causes were to be investigated, so also were the cures. What effect did punishment have for the prevention of crime? What good could be done by education?

I formulate some of the propositions with regard to the commission and prevention of crime and show the relations of different methods to the end sought to be attained.

I.—The commission of crime—how induced:
- 1. By heredity.
- 2. By education:
  - *a.* Environment,
  - *b.* Sociologic influences, chiefly in youth.
  - *c.* Economic influences; as poverty, famine, &c.

II.—The prevention of crime:
- 1. By fear of punishment:
  - *a.* Execution.
  - *b.* Imprisonment.
  - *c.* Fine.
- 2. By restraint:
  - *a.* Imprisonment in reformatory institutions.
  - *b.* Education.

III.—For the prevention of crime which had its cause in heredity:
- 1. Restraint of liberty (of the born criminal) before commission of any crime; this for the individual and for effect in the present.
- 2. Restraint of marriage or the prevention of the birth of children who are certain to become criminals.

IV.—Reformation of criminals:
- 1. By punishment after the commission of crime.
- 2. Restraint before the commission of crime.
- 3. Education:
  - *a.* Religion and morals.
  - *b.* At home.
  - *c.* At church.

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* A report on the Second International Congress of Criminal Anthropology, held at Paris, August, 1889. By THOMAS WILSON, LL. D., curator of prehistoric anthropology of the U. S. National Museum, appointed as delegate from the Smithsonian Institution to the said Congress.

d. In parochial schools.
e. Public schools:
Technical.
Manual.
Night schools.

The Congress of Rome of 1885 adjourned to meet in Paris upon the occasion of the French Exposition in 1889, from August 10 to 17 inclusive. The opening session was held at the Palace Trocadero under the presidency of the minister of Justice and Worship, and Keeper of the seal.

The following officers were chosen:

Honorary presidents: MM. Thevenet, Keeper of the Seal, Minister of Justice and of Worship, France; Benedikt, Professor of the University of Vienna, Austria; Brouardel, Dean, Professor of Medical Jurisprudence at the Faculty of Medicine, Paris, France; Demange, Avocat in the Court of Appeals of Paris, Member of the Council of Order, France; Ferri (E.) Professor of the University of Rome, Deputy of the Italian Parliament, Italy; Garofalo (Baron), Vice-President of the Civil Tribunal of Naples, Italy; Hakim (John), President of the National Italian Committee of the Universal Exposition of Paris, Official Delegate of the Committee of the Italian Congress, Italy; Hamel (van), Professor of the University of Amsterdam, Holland; Ladame (Doctor), Professor of the University of Geneva, Switzerland; Lombroso (Cesare), Professor of Medical Jurisprudence, Turin, Italy; Moleschott, Professor of the University of Rome, Senator of the Kingdom, Italy; Romiti (Doctor), Professor at the University of Pisa, Italy; Semal, Director of the Asylum for the Insane, Mons, Belgium; Taladriz (Alvarez), Dean of the Bar at Valladlolid, Spain; Tarde, Judge of Instruction, Sarlat (Dordogne), France; Dr. Lorenzo Tenchini, Professor at the University of Parma, Italy; Wilson (Thomas), Attorney of the Supreme Court, Curator of the Department of Prehistoric Anthropology, U. S. National Museum, Washington, D. C.

President: M. Roussel (Doctor Theophile), Senator, Member of the Academy of Medicine.

Vice-presidents: MM. Lacassagne (Doctor), Professor of Medical Jurisprudence of the Faculty of Lyon (Rhone); Motet (Doctor), Medical Expert of the Tribunals of Paris.

General secretary: M. Magitot (Doctor), Member of the Academy of Medicine, Ancient President of the Society of Anthropology of Paris.

Recording secretaries: MM. Bertillon (Alphonse), Chief in the Service of Identification of the Prefecture of the Police in Paris; Bournet (Doctor), Secretary and Editor of the Archives of Criminal Anthropology of Lyons; Coutagne (Doctor Henri), Medical Expert at the Tribunal of Lyons; Manouvrier (Doctor), Professor in the School of Anthropology at Paris.

The official delegates were as follows:

Austria-Hungary: M. Benedikt, Professor of Neuropathology at the University of Vienna.

Belgium: Dr. Semal, Director of the State Insane Asylum at Mons; Dr. de Smeth, Professor in the University at Brussels.

Brazil: Councillor Ladislas Natto, Director of the Museum at Rio Janeiro.

Denmark: Hansen (Soren), Copenhagen.

United States: Dr. Thomas Wilson, Curator of Prehistoric Anthropology, U. S. National Museum, Delegate of the Smithsonian Institution; Clark Bell, Esq., Delegate of the Society of Medical Jurisprudence of New York.

France: MM. Dr. Lacassagne, Delegate of the Society of Anthropology at Lyons. Dr. Letourneau, Delegate of the Society of Anthroplogy of Paris.

Hawaii: M. H. de Varigny.

Holland: M. Hamel (van), Professor of the Law Faculty of Amsterdam.

Italy: Hakim, (John), President of the Italian Committee at the Exposition of Paris.

Mexico: M. E. Raphael de Zayas Enriquez.

Paraguay: M. Dr. Hassler.

Peru: MM. Dr. Muniz, Surgeon of the Army in Peru.

Roumania : M. Dr. Iscovesco ; Dr. Soutzo, Professor of Legal Medicine at the Faculty of Medicine at Bucharest.

Russia: M. Dr. W. de Dekterew, Delegate of the Society of Public Hygiene, of Moscow.

Servia: Milenko Vesnitch, Doctor of Law.

Sweden: M. Dr. G. Retzius, Delegate of the Society of Anthropology of Stockholm.

There were twenty-two countries, represented by 192 delegates. At the opening session addresses were made. First, a welcome by the Minister of Justice, by Dr. Brouardel, and Dr. Th. Roussel, which were responded to on behalf of the foreign delegates by M. Moleschott, president of the Congress at Rome. The meetings, after the opening session, were held in the amphitheater of the Faculty of Medicine, the same place as had been held the Congress of Hygiene and Demography.

The questions proposed by the committee of organization to be discussed by the Congress were as follows, the preparation of papers thereon having been assigned to the persons whose names respectively follow them:

The first series:

SECTION I.—CRIMINAL BIOLOGY.

I. The Latest Discoveries in Criminal Anthropology. Prof. Ces. Lombroso, University of Turin, and Prof. L. Tenchini, University of Parma.

II. Do Criminals Present any Peculiar Anatomic Characters? If so, how can we discover them? Dr. Manouvrier, professor of the School of Anthropology of Paris.

III. Establishment of General Rules for Investigating the Occupants of our Prisons and Insane Asylums by means of Anthropometry or Psychology. Prof. Sciamanna, of Rome, and Lawyer Virgilio Rossi.

IV. The Determining Conditions of Crime and their Relative Values. Prof. E. Ferri, deputy Italian Parliament and professor of Criminal Law.

V. The Infancy of Criminals Considered in its Relation to Predisposition to Crime. MM. Prof. Romeo Taverni, Catania, and Dr. Magnan, Director of the Asylum, St. Anne.

VI. Organs and Functions among Criminals. MM. Dr. Frigerio, of Alexandria, and Dr. Ottolenghi, of Turin.

SECTION II.—CRIME IN ITS RELATION TO SOCIOLOGY.

VII. The Determination by Means of Criminal Anthropology of the Various Classes of Delinquents. Baron Garofalo, president of the Civil Tribunal, Naples.

VIII. Conditional Liberation. Dr. Semal, director of the State Insane Asylum, Mons, Belgium.

IX. Crime in its Relation to Ethnography. Dr. Alvarez Taladriz, Madrid.

X. Moral Responsibility; What are its Foundations? M. Tarde, judge of instruction, Sarlat (Dordogne).

XI. Criminal Process from a Sociologic Point of View. M. G. A. Pugliese, Lawyer, Triani, Italy.

XII. The Relation of Criminal Anthropology to Legislation and Questions of Civil Rights. M. Avocat Fioretti, of Naples.

XIII. The System of Solitary Confinement in its Relation to Biology and Sociology. Prof. van Hamel, of Amsterdam.

Questions proposed by volunteers:

XIV. Atavism Among Criminals. Dr. Brouardel, professor of the School of Anthropology of Paris.

XV. Criminal Anthropology considered as a branch of General Anthropology. Dr. Manouvrier, professor of the School of Anthropology.

XVI. The Teaching of Anthropologic Sciences in the Law Schools and Colleges. Professor Lacassagne, of Lyons.

XVII. Anthropometry as Applied to Young Persons from 15 to 20 Years of Age. M. Alphonse Bertillon.

XVIII. The Employment of the Methods of Criminal Anthropology in the Aid of the Police and Arrests of Criminals. Avocat Anfosso and Professor Romiti.

XIX. The Correctional Education and Reform of Criminals in Accordance with Biology and Sociology. Dr. Motet, Paris.

XX. Perversion of Affections and Moral Qualities in Infants. Dr. Magnan, Insane Asylum of St. Anne, Paris.

XXI. Mental Degeneration and Simulation of Insanity; Reciprocity between them. Dr. Paul Garnier.

XXII. Influence of the Professions on Criminality. Dr. Henri Coutagne, Lyon.

XXIII. The Degenerative Characters and Biologic Anomalies Among Criminal Women. Drs. Belmondo and A. Marro, Italy.

XXIV. Vegetative Functions Among Criminals and Insane. Drs. Ottolenghi and Rivono, Italy.

XXV. Causes and Remedies for the Repetition of Crime by the Same Persons. Avocats Barzilai and V. Rossi.

XXVI. Political Crime from the Standpoint of Anthropology. Avocat Laschi.

XXVII. Criminal Sociology in its Application to Jurisprudence. M. Pierre Sarraute, judge of the Tribunal, Perigueux (Dordogne).

XXVIII. Criminal Anthropology in its Relation to Sociology. Avocat A. de Bella.

XXIX. Criminal Anthropology in Egyptian Society in Antiquity. M. Ollivier Beauregard, of Paris.

XXX. Moral and Criminal Responsibility of Deaf Mutes. M. Giampietro, of Naples.

XXXI. The Relations of Criminal Anthropology with Medical Jurisprudence. Dr. Zuccarelli, of Naples.

XXXII. The Effect and Modes of Application of the Penal Law According to the Standard or View Point of Criminal Anthropology. M. Vittorio Olivieri, of Verona.

XXXIII. Criminal Sociology. Dr. Colajanni, of Catania, Sicily.

XXXIV. The Contagion of the Crime of Murder. Dr. Aubry, of Saint Brieuc, France.

XXXV. Political Assassins—a Medico-Physiologic Study. Dr. Regis.

XXXVI. The Role of Woman in the Reduction of Crime. Dr. (of law) Joseph d'Aguanno, of Palermo.

XXXVII. Medio-Physiologic Observations on the Criminals of Russia. M. J. Orchanski, professor of the University of Charkow.

The discussions of the congress were opened at its second session, Monday morning, August 12, by Signor Lombroso, upon the first question, "The Latest Discoveries of Criminal Anthropology." His discussion soon developed the fact that there were two great parties in this congress. One, which was led by Lombroso, and might be called the Italian school, for it comprised a great proportion, though not all, of the Italian delegates; and the other, lead by Dr. Manouvrier, to whom adhered the majority of the French delegates.

Question I.—Signor Lombroso said a Greek philosopher in moving, proved the fact of movement, and it is so to day with the discoveries of criminal anthropology. These discoveries prove the existence of the science better than the most rhetorical amplifications. The most important problem of the last congress, then only half resolved, has been completed by the studies of Verga, Brunati, Marro, Batl, Gonzale, Tonnia, Pinero, and by himself. The number of cases of epilepsy with intervals of consciousness has been extended by genealogic studies of epileptic families, by their derivation from criminals, from consumptives, from aged parents, accompanied with the predominance of awkwardness and clumsiness, by frequent vertigos, occasional delirium, etc. The occasional cases of epilepsy without absence of moral sense, but with erethism or exaggerated sensibilities, explains how some persons, criminals because of their passion, have many times an unconsciousness of their own criminal acts. The role of epilepsy extended itself into the category of the criminal insane, principally among the victims of alcoholism, the hysterics, and other monomaniacs. One has only to take the chart of Esquirol on the homicidal monomaniacs to find the manifestation and extent of psychic epilepsy.

The "criminals of occasion," studied anthropologically, have shown in themselves (as one can say in the language of bacteriology) attenuated, but nevertheless, distinctly visible, characters of the born criminal. His sensibility is less obtuse, his reflexes less irregular, the anomaly less frequent, especially in the skull, but they have always the characters of the criminal born in some degree, such as the blackest hair in the servant who is a thief, awkwardness more frequent among the swindlers, and that they are all more governed by impulse.

In my study of the photographs taken by Mr. Francis Galton, said he, I have found in eighteen skulls of condemned persons, two types which resemble marvelously and with an exaggeration which is evident, the characters of the criminal and approaching those of the savage. Frontal sinuses well marked, cheek and jaw bones very large, orbits large and distant, an unsymmetrical face, the nasal overture of a pheleiform type, and lemurian attachment of the under jaw. The other skulls of the swindlers, thieves, and robbers gave to me a type less precise, but the want of symmetry, the great size of the orbits and the prominence of the cheek bones were well marked, though less than in the former cases. The anomalies were less marked than in the eighteen

skulls above mentioned. This discovery appears to me to have an importance not at first seen, for it serves to increase the signification and importance of the statistics of anthropometry. In order to obtain reliable indications we should investigate only homogeneous groups.

Mr. Lemoine has published in the Archives d'Anthropologie Criminelle of Lyon an anomaly which is perhaps unique: The union of the frontal lobes found in an ex-member of the commune who died at his house in Lille.

M. Severi has shown that compared with the normal type the criminals have a great capacity or size and extent of the fossettes of the cerebellum.

Marino has demonstrated the diffusion of the occipital fossette: 22 per cent. among the Papuans and 25 per cent. among the New Zealanders, while he has confirmed the same proportion that I have found among the Europeans and among the criminals.

Joly has confirmed the strange phenomenon that the physiognomy of criminals loses the stamp or type of their nationality.

Ottolenghi has studied and developed the curious characteristics of criminals in regard to baldness or gray hair. He has found in them an enormous retardation, comparable only to the epileptics and idiots. He also found the wrinkles to be more numerous among criminals, and above all the one naso labial, which he remarked as a characteristic.

The female criminals differ among each other as much as the men, and these characters are almost entirely absent.

The criminals have a peculiar gesticulation. They have a jargon or dialect among themselves, as well as a caligraphy, which latter has been confirmed by hypnotism.

The peculiarities of criminals extend even to their art. They excel in mechanics and in their precision of detail, but they lack in ideality. The study of molecular changes has given some curious results. The average temperature is much above the normal in criminals. It presents but slight variation in pyretic maladies. An analysis of the urine of criminals born gives a greater proportion of phosphoric acid and less of azote.

Lombroso did not continue his presentation at great length nor with great detail. He referred his audience to his last book, which was published with the maps, scales, and tables therein set forth, and he declared his unwillingness to take away from his colleagues the pleasure which they might have in presenting some of their own discoveries.

Dr. Manouvrier followed him and disputed his proposition, and plunged into the discussion of the great question whether criminals were born or made. He pronounced the theory of his opponents to be but a recitation of the exploded science of phrenology, which, whatever good it may have proved, was compelled to fall before the poverty of its experimental statistics and our certain knowledge. He admitted the physiologic and anatomic differences mentioned by Lombroso, but

he declared them to be differences of anatomy and physiology; that they belonged as much to honest men as to criminals, and that the line of difference mentioned by Lombroso bore no relation between an honest man and a criminal. These were structural and other differences of physiology and anatomy, while crime was a matter of sociology.

Baron Garofalo, MM. Drill, Lacassagne, and Benedikt declared their opposition in whole or in part to the theory of Lombroso. M. Drill recalled that the organization of man was far from being simple, that he was an extremely complex being, made up of many component parts and that his life depended upon his surroundings, his education, his training, his companions, and that whatever there might be in the physical or anatomical characteristics of a man which would point towards his crime or the possibility of its commission, that each of these elements entered into and became a factor, and were each and all of them to be considered in deciding this question.

According to M. Dekterew the surrounding circumstances, the social condition, of man played the greatest rôle and had the greatest influence.

M. Pugliese declared crime to be a social anomaly and the consequence of a failure of the criminal to adapt himself to his social surroundings.

M. Benedikt, of Vienna, was of the opinion that criminals were sick men either in body or spirit; and if one examines the exterior morphologic signs to explain and account for the existence of crime in the conduct of a given man, it was equally necessary to investigate the molecular trouble in his cerebral structure. He declared that the physiologic characteristics were a greater factor than the anatomic, and this it was, with the favorable social surroundings, that made the assassin or the robber. The criminal, said he, has no particular stigma or mark by which he can be known from other men. Sometimes there may be signs of a defective organization, but these are marks or signs of the epileptic or of the insane. This was also the view of Tarde. There might be certain predispositions which were organic or possibly physiologic, which were more or less easily developed according to the social surroundings of the individual and which might, under favorable circumstances induce crime.

M. Lacassagne agreed with Tarde that in considering the problem of criminality it was necessary to take largely into account the social influences. Because these influences and surroundings might modify the organic characteristics and thus create these anatomic anomalies which were relied upon by the Italian school. In order to study the criminal it is first necessary to consider his surrounding. It is not atavism, but the social surroundings, the social condition, which make the criminal. If the condition of the humble and the poor and the young and the ignorant is ameliorated you will diminish immediately the army of criminals. It is society which makes the criminals. Society has only the criminals it merits. Criminality was above all a social question. M. Lacassagne said a factor of crime too much neglected was misery, pov-

erty, and he declared it to extend backwards, not only throughout this life, but might have been derived from the parents especially the mother. Garofalo disputed the assertion of Lacassagne. He said the statistics would show that crime was committed in equal proportions by the person who was born and raised, he would not say in affluence, but in such circumstances as to avoid the charge of poverty or misery, and he demanded before these assertions should be made or conclusions accepted that accurate statistics should be furnished. Madame Clemence-Royer invoked a new factor in the genesis of crime which, in her opinion, had a greater responsibility than had before ever been attributed to it, to wit, hybridity—the mixture of races, the mixtures of the blood of different races, one of which was usually if not always an inferior.

M. Moleschott, senator from Italy, thanked M. Tarde and Dr. Benedikt for having spoken of the molecular movements, for, said he, there is the question. The minute researches into the anatomic conditions made by Lombroso should not make us to forget the different stages of life which are presented in each individual according to the different conditions of his life and that the first false step has been approached on an infinite scale. A more or less degree, however small, of irritability on the part of an individual may result in a duel or other crime, because, according to the words of our Lord Jesus Christ, "We are all sinners."

Dr. Brouardel said that in order to resolve the problem it was necessary to apply clinical methods. We do not say that a sick man has the typhoid fever because he has the headache, or the diarrhea, or cough, or fever but we say he has typhoid fever because we have grouped his symptoms and according to their existence and method and the time or period of their apparition we determine that he is afflicted with this malady. Therefore to the anatomic stigmas of an individual it is necessary to add the corresponding psychologic characters. The delirium of combativeness which is due to a poison produced by belladonna is not a cerebral localization. It is due to a modification brought by the presence of the agent in the blood, of the nutrition of the entire cerebral mass.

M. Ferri declared crime to be a phenomenon extremely complex. It was a sort of polyhedron of which each person saw but a special side. The different views sustained to-day are equally true and yet equally incomplete. M. Lombroso, said he, brings to light the biologic side of crime; Drill and Manouvrier showed the social; Pugliese the legal view; Tarde presented the physiological side, and Moleschott and Dr. Brouardel declared crime to be a phenomenon at once biologic and social. M. Lacassagne said in the first Congress at Rome that the criminal was a microbe which propagates only in a certain condition. Without doubt the conditions and the surroundings make the criminal, but like the bouillon without microbes within it, the surroundings without crimes are powerless to bring forth the criminal.

As the bouillon is complementary to and as necessary as the microbe, so the biologic defects and the favorable social surroundings are the fundamental aspects of criminality.

Question II.—Do criminals present any peculiar anatomic characters? If so, how can we discover them? Dr. Manouvrier said that, in order to study the anatomy of criminals, it is necessary to consider their physiological elements, and to divide and subdivide those elements in the attempt to attach one or more to each specific crime or series of crime. It is necessary first to discover a method by which it can be determined whether criminals differ anatomically from honest men, and at the same time whether criminals differ from each other, and wherein. As soon as one can recognize certain special anatomic characters as more frequent or more pronounced among criminals or among such and such category of criminals, one will then be in the right path to make an analysis of the subject. This is called to-day, in a vague and indefinite manner, the tendency to crime, or the tendency to particular crimes. These tendencies ought to be resolved into their true physiologic elements, corresponding to certain elementary anatomic characters. But the problem is complicated by the intervention of sociologic elements, so that one becomes lost in a labyrinth of speculation. If one supports the theory that criminals are born, it is but a return to that ancient but now exploded science of phrenology, which from an examination of the head, and so of the brain, the expert could determine from the relative size and value of what he called organs, the virtuous or the vicious character of the individual, which in particular cases was called the tendency to crime. Dr. Manouvrier insisted that this theory was completely exploded, that these characteristics were not confined to criminals nor to criminal classes, for all the anatomic distinctions and psychologic characteristics quoted by Signor Lombroso were to be found among honest men as well as among criminals. And he argued that it was not sufficient that you should find a greater proportion of them among criminals than among honest men. If Lambroso's theory, that the man was born a criminal, was to be taken as the rule, then it must be universal, and that men thus born inevitably committed crime. If it be the rule then it must operate in all cases. That it did not so operate proved that it was not the rule, and therefore he concluded the proposition of anatomic characteristics peculiar to criminals did not exist.

Manouvrier asked had any one seen an anatomic character which would serve to characterize exclusively the criminals of any certain category, such as robbers, thieves, assassins, burglars, etc. No anthropologist believes in the existence of such a character. There are many epileptics, drunkards, imbeciles, degenerates, and inferiors of all sorts who have never committed a crime; their action has been such as that they stand fair to the community, and they have a right to be classed with honest men; no one has a right to class them with crimi-

nals. If some of them have been criminals, who can say that they would not have been honest if subjected in early life to favorable education and sociologic influences? But, on the contrary, who can say what may not become of the man who has a sound mind in a sound body if he be subjected to the continued pressure of adverse sociologic surroundings. Take as a single illustration the feeble cranium capacity which is not without certain relation to feebleness of mind. The feebleness of mind may make its owner commit crime under certain deplorable circumstances, but at the same time this may render him more inoffensive under other circumstances. His unfortunate anatomic character may itself conspire to make him more peaceable, honest, and virtuous. In any event it would be hard to affirm that there was a greater proportion of feeble-minded men among honest men than among dishonest. And as with feeble-mindedness, so with the other anatomic criminal characteristics.

Some one has used the phrase "all other things being equal," a man with such and such anatomic characteristics would be more likely to become a criminal than a man with other characteristics. Manouvrier assailed this position, saying that it was founded in error. It was because "all other things" were *not* equal that the man became criminal. He asked what were these things, and suggested the infantile life, familiarity with vice and crime, the surroundings, the want of moral training, sociologic conditions; and these, he said, were the conditions which produce the criminals rather than the anatomic characters. He asserted that the man with characteristics the opposite of Lombroso's criminal, if subjected to the conditions, influences, and temptations which lead towards crime, was as likely to become a criminal as was he who possessed the characteristics described by Lombroso. He assailed also the idea of a criminal type who stood for the criminal classes. He declared that, in his opinion, there was no such type. The criminal, the thief, might have a head shaped one way in one case, and another way in another case, with crania or facial anomalies, with deep occipital fassettes, and so forth. But these did not form a type; they were different characteristics which had no relation to each other, and which he did not believe had any relation to crime or criminal tendencies. It was as though a man with a long head commits a crime; according to this theory, that forms a criminal type. A man with a broad head commits a crime; that forms a criminal type. And, using different peculiarities as illustrations, where a man with long arms or long legs, or one with short arms or short legs, commits a crime, then each of these become in their turn criminal types. Thus you have as criminal types the long and the short, the round and the square head, the long and the short arm, and the long and the short leg. Therefore he declared his opinion that, properly speaking, there was no such thing as a criminal type. The criminal type was the man who, having submitted to the sociologic influence of crime, having been born and raised therein

and always submitted to them, finds himself in an atmosphere of crime to which he adapts himself, and so commits it in the same kind of way as he breathes the air of the ill-ventilated tenement house or cellar in which he lives. In order to create a type there must be a continuation of characteristics, a recurrence in given directions, which is repeated again and again until it becomes fixed, and the required characteristics are manifested in every normal individual of each generation. This forms a type: without this continued re-appearance of characteristics, no type is formed.

Manouvrier declared that no account had been taken of the different kinds of crimes, crimes which were different in their motives, requiring different kinds of individuals to commit them, and that a type for one would not stand as a type for the other. He divided these thus:

First: Strange crimes, those inexplicable to the normal man, such as were committed by the insane, by the epileptic, idiots, and the delirious. This ground belongs to pathology and to teratology.

Second: Crimes committed under the influence of passing troubles or delirium, such as anger, drunkenness, jealousy, fear, etc. It is necessary to distinguish in these criminals thus deranged whether they be habitual or accidental; that is to say, the irascible, the habitual drunkard, the insanely jealous, etc.

Third: The crimes accomplished in cold blood, after a certain fashion—deliberate, intentional, with malice aforethought; and he asserted that it was to the latter class and to that alone the investigations of this congress should be confined. To the two others it went without saying that they might have had predispositions to crime as they had predispositions to the various maladies which influenced them to crime, some of which they could possibly avoid, others of which they possibly could not. In these cases it was the malady that caused the crimes, for which it was responsible, and that the crime in these categories was not the deliberate act or intent of the criminal.

The distinction between the normal and the pathological state, based on a physiological analysis, is indispensable in the study of this subject. But to do this satisfactorily, how great the difficulty? If this be difficult, how impossible to classify properly the doubtful and intermediate cases? Without these doubtful and intermediate cases being fully classified we will have naught but physiological disorder. It is necessary also to distinguish physiologically and anatomically between the normal and the abnormal state (this of the same persons?). Physiologically it is abnormal to murder or to rob without motive, or at least without other motive than the mere pleasure, whether it be the gratification to the criminal or the pleasure he may receive to see another suffer. But one must be an optimist to believe that it is abnormal to covet the property of another, and so coveting to seek to appropriate it. It is idle not to recognize, in addition to the imperfections of human nature, the pernicious influence that is exercised by

the evil education, the evil examples, the natural or factitious needs; the seductive occasions, the improper liasons, the repugnance to labor, the pleasures of idleness, the apparently natural willingness to eat the bread and enjoy the fruits of another's labor, or the satisfaction of a former escapade which brought profit, and went unpunished; and, in a word, it is useless to refuse to recognize the thousand different sociologic conditions which may serve to form a million of combinations, any of which may lead towards crime. With what care is one not obliged to guard the child and the young person from the hardening effect of evil influences or from the corruption of his childish innocence and innate honesty and virtue by the persuasions and example of evil associates.

Without doubt theft appears execrable, while murder is horrible, to those young persons who, thanks to a careful education or the precepts of a good mother, or the influences of a Christian family and surroundings, have acquired the habits and situation of honest people; and, nevertheless, one can easily imagine a combination of circumstances, an acquaintance with vice and crime, by which such an individual has or may become a criminal.

> Vice is a monster of such hideous mien,
> That to be hated needs but to be seen;
> Yet seen too oft, familiar with her face,
> We first endure, then pity, then embrace.

And there are all sorts of crimes, and that which might be no temptation in one case might be overpowering in another. With all these difficulties is it not impossible by any system of classification to draw the line between a normal and an abnormal physiologic state, which will separate the criminal classes from the honest men?

We have still to consider that there are many physiologic peculiarities which become good or bad qualities according to the circumstances, and these circumstances are simply the surroundings, the environment. An amorous temperament might be highly appreciated and complimented in one case, and yet become extremely dangerous in another. The audacity and courage which might be a source of pride in the soldier, would become execrable on the part of a robber. An excellent salesman, the successful drummer, the best newspaper reporter, might, with a change of circumstances, a change in his surroundings, his environment, become a most dangerous swindler, or the best mechanic may become a most dangerous bank burglar or counterfeiter; and his eminence in crime is attained because of his apparently natural excellencies, which might have made him, and which went so far towards making him, an honest and successful man.

Crime is, therefore, not necessarily bound to physiologic peculiarities, nor is it produced by abnormal or disadvantageous anatomic characters.

It must be remembered that the man, healthy and normal though he be, is not a man without faults or without tendency to vice. To seek

for this is to seek for the impossible. All men, however honest or virtuous, will be found to have some defect or some vice, otherwise they would be perfection, which is not to be expected of human nature.

A defect or a vice, whether anatomic or physiologic, does not become an anomaly simply because one finds it in a criminal. Anatomically the same remark is to be made; we do not consider as abnormal or inferior every man who is not perfect.

Dr. Manouvrier proceeded to examine the results of anatomic researches made, up to the present time, upon criminals.

No one has yet accomplished or discovered an anatomic character by which the criminal can be classified into categories, like robbers, swindlers, burglars, etc. The most one can do in investigating the tendency to crime by the examination of the criminal himself is to seek for the specific characteristics, but even these, if found, do not prove that they are specifically criminal or special to criminals.

All that can be done in this direction, and it is quite another question from the former, is to discover if the criminals examined present certain abnormal anatomic characters more frequent and in a higher degree than honest men. To answer either affirmatively or negatively as to the whole aggregate, or even to the average, would be a hardy and even dangerous undertaking. There are honest men who are affected in all the unfortunate and much to be regretted ways suggested by Signor Lombroso—epileptics, imbeciles, degenerates, and even the vicious and inferiors of all sorts; while those who have been classed as honest men are capable of becoming criminals of the darkest dye, and have no more morality or virtue than the most incorrigible robber and thief.

Dr. Manouvrier referred again to the saying, "All other things being equal," the abnormal, the inferior, etc., were more likely to become criminals, etc., "but" he demanded, "is it certain that all things are equal for the criminal?" It is in vain that we have remarked the small number of individuals becoming criminals out of each hundred persons snbjected to these defective sociologic circumstances. The conditions and circumstances which are so difficult to weigh, and above all the infinitely variable combinations, whether taken by themselves or by their complex tendencies, have a different effect upon each individual. Among a hundred individuals thus environed, is it not possible to believe that the ten or twenty who become criminals are those which have been subjected to the combinations, sociologic and physio-sociologic, the most evil, the most powerful, and the most effective in leading them in the wrong path? It is therefore wiser to permit the facts to decide each case for itself.

The documents published are numerous, but they are not yet sufficient to convince an incredulous anthropologist who finds himself opposed to either view, and who proposes to examine them critically. Occasionally monstrous criminals have been exhibited, but that does

not prove that criminals are anatomic monsters, and no more does the fact that some criminals are epileptics prove that all criminals are epileptics, nor that epileptics become criminals. The statistics obtained and the averages sought to be established have been based upon insufficient data. The series have not been sufficiently extended, the figures have been obtained by defective processes, the observations have been uncertain and different, and the observers or investigators have been novices in many cases, and in others have proceeded upon different lines, if not by different processes, each one of them more uncertain and defective than the other. They have cited insignificant differences which they say exist between honest men and criminals, but which differences may be found in equal proportion among honest men, if they were so examined, and might also be found between criminals. They have compared the series of criminals with series of soldiers; that is to say, with men who are chosen for their exemption from infirmities or deformities, and have calculated the relative frequency of these deformities in the two series, or in the series of the two classes without regard to the difference in their condition. They have cited cranial peculiarities observed by different persons operating in different methods and by different rules, with different standards. And from all these discordant and inharmonious elements they have sought to establish averages in the respective classes whether of criminals or of honest men.

In spite of all this incoherence and erroneous and defective process, whether of gathering facts and obtaining evidence, or of ratiocination, they have obtained statistics, which, aided slightly by preconceived opinion, have almost persuaded some of our wisest and best men that the criminal classes present in their average a proportion of abnormal or inferior characters greater than those belonging to the classes of honest men. The number of these abnormal or inferior characters are multiplying themselves day by day in the estimation of these wise men, and this is being pushed to such extremes as that soon the man who is believed to be honest will find himself possessing a half dozen of these criminal characteristics. Thus the system is in danger of breaking down of its own weight.

We might with propriety ask, what constitutes a criminal type? If, in making this examination of criminals, one unites the characters abnormal, pathologic or inferior, taken in an examination of say a thousand criminals, without considering and arranging upon the other side the characters found therein which are incompatible with each other, it will be apparent that the investigation will be without value and the conclusion based thereon erroneous. One criminal is plagiocephalic, another has long arms, another a vermien fossette, etc. But it is not any one of these that forms a type whether criminal or otherwise.

In order to form a type one should unite the common characters, eli-

minating the anomalous and pathologic manifestations. In order to obtain an abnormal type, it is necessary to choose for each species of anomalies or alteration an individual in which this anomaly or alteration is well characterized, and then there will be as many types as there are sorts of anomalies or alterations. We therefore can not have a type criminal any more than we can have a type of human monsters.

In order to characterize criminals in general, it is necessary to obtain the averages, which can be compared with the averages of other individuals of the same race, the same sex, the same social class, etc. These latter individuals must themselves be the average of their respective race, sex, or class, and their averages thus taken should become the type or standard.

Honest or virtuous men (a category not less vague than that of criminals) will then be without doubt the metatypic. But these have not yet been studied nor their type settled. Nevertheless it is these metatypes that we should compare anatomically with the criminals if we would make comparison between the anatomic characters of the two classes. Who form this class of honest and virtuous men that furnish the standard by which the criminal classes are to be judged? They may be idle, vicious, evil disposed, imbecile, passionate, brutal, and all that, if they have but escaped being declared by the law to be criminals. In this condition of affairs is it possible that any one can determine anatomically, or physiologically, or psycho-sociologically what physical characteristics form a criminal type of man?

What are the results? This is a question to be resolved by anatomic anthropology, of which the comparative anatomy of criminals is no more than one chapter. The anatomic study of criminals in order to become of value has need to be extended to a greater area and in greater detail even than has been here indicated.

There was, of course, a large discussion among the members of the congress over this question. Nearly every one had a different idea concerning it.

Professor Lombroso responded to Dr. Manouvrier. He demanded how he would distinguish the criminals. The criminals of occasion has presented abnormal characters. It was not the occasion that made the criminal, but it was the occasion which was presented to an individual predisposed to commit the crime. It has been objected that the woman criminal had no anatomic characteristics, but they who made that objection forgot that prostitution was the form of the feminine criminality. He believed somewhat in the idea emitted by Madame Clemence-Royer on the relation between crime and hybridity, or mixture of races, one being inferior. If the crime is not an anomaly, what is it? Is it a virtue? He agreed with Dr. Manouvrier that the cranial capacity is not a characteristic of criminality. Bearing upon the question of atavism he stated that he had found among criminals a great number or proportion of hernia. This was a regressive char-

acter. The role of ptomaines in criminal manifestations appeared to him certain.

M. Tarde responded to Lombroso apropos of the criminal woman. He maintained that an honest woman presented the characteristics ascribed to the criminal woman as described by the Italian school, and nevertheless, woman is less criminal, or takes to crime less than man. Prostitution is the occasion and not the offense. He declared there were no anatomic characters proper or peculiar to the criminal, and, nevertheless, there were organic and physiologic predispositions to crime. The function made the organ, and the nerve would model the bone; as the river determines the valley, so the crime makes the criminal. If in criminal anthropology one can come to show the localization of criminal characteristics, as has done Broca for the articulate language, the base of the scientific edifice might be considered established.

M. Moleschott and Dr. Brouardel complimented these gentlemen upon the profoundness of their studies. The latter considered the search for the criminal anomaly in physical or anatomical characteristics as illusory. He could admit the malformations of the pavillion of the ear reported by Morel, the occipital fossette and the characters of the same kind, but these were no cause of criminality in themselves, but only simple indexes of an abnormal development of which the consequences could be many. The epileptics, the insane, show the presence of ptomaines in their urine. He recalled the observations of an epileptic woman in his service. Her urine contained a convulsive ptomaine, which injected into a frog produced the same physiologic effects as strychnine. The ptomanic products or the leucomanic toxique found in the veins of the insane and the melancholy result from troubles in general nutrition. Are they cause or are they effect? The question demands to be studied.

Dr. Brouardel responded to M. Tarde that if the function made the organ, it could only do so in the presence of muscular fiber. A woman without any calf to her leg could never become a dancer.

M. Bajenoff, director of the Asylum of Riazanne, Russia, could not accept everything he had found in the works of Lombroso and his colleagues, but his and their methods seemed to be scientific. His own studies cephalometric had shown to him that honest men had a larger frontal development, while the criminals were better developed in the parietal and occipital portions of their brain or skull.

Baron Garofalo said that crime might be considered always the result of an organic anomaly. In speaking of crimes we should consider only those which were declared so by the public conscience and not always those declared so by the law. Those, for instance, of great cruelty or extraordinary improbity. But one could perceive that criminals always manifested moral anomalies and physical anomalies that were found less frequently in honest men.

Lombroso insisted upon his fundamental distinction between the

criminal born and the criminal of occasion. But he conceded that the existence of criminal anatomic characteristics might be limited or even absent in the latter class. He declares woman to be a criminal of occasion, except with prostitution, wherein she represented the born criminal. But in the criminal born he insisted upon the existence of physical signs which he declared to be undeniable, and that while their number and importance vary from one individual to another, yet when considered together, had a value and signification "absolument incontestable." While he would not deny the influences sociologic, mesologic, geographic, and orographic, yet the effect of these influences was only to intensify the criminal characteristics which existed anatomically and fundamentally. Thus it will be seen that in the discussion between these two representatives of the different schools, in spite of the apparent diversity of their opinion they came nearly together by an exchange of partial and reciprocal concession. Yet this harmony was more apparent than real, for in the subsequent discussions of the Congress, whenever anything was said favoring the existence of a criminal type, it immediately precipitated a return to the former discussion.

In the discussion of the seventh question the whole argument was gone over again. The skull of Charlotte Corday, which belonged, with all guaranty of authenticity, to the collection of Prince Roland Bonaparte, was presented as an illustration of a born criminal because of the depth of the occipital fossettes. This immediately brought out Lombroso, who returned to the attack with all his ardor and power, and after him Benedikt, of Vienna, Garofalo, Ferri, Brouardel, and at last, M. Herbette. The latter, with Dr. Brouardel, seemed to be the most conservative. They presented, each of them, in calm and considerate but elegant language, the necessity for careful study and profound investigation. *Festina lenta* was their motto. While they recommended the investigation and study to be made with ardor, and pushed to the extreme, they counseled that the conclusions should not be made hastily, changes should not be made brusquely, opinions not be announced dogmatically, or by going too rapidly, this science might compromise its force, its authority, or its prestige.

The importance of this question or the value of its discussions in this congress can not be overestimated, for while the substance may have been argued pro and con in years past, yet here for almost the first time the scientific men of the world were assembled in an international congress for its discussion, with full opportunity for preparation, and with the knowledge that they were here to be brought face to face with their opponents or those who held different opinions from themselves, and here they were to appear with what arguments, reasons, statistics they might have in defense of the position which they claimed to be right. Accordingly as this question shall be decided, so should there be a change in the fabric of our criminal jurisprudence. If men are born criminals then they are not to be punished as they would be if

otherwise. If, on the other hand, they are educated to be criminals, then ought our system of education to be seriously and radically changed. I repeat my impression of the profound importance of this science.

Question III.—Establishment of regular rules for investigating the occupants of our prisons and insane asylums by means of anthropometry, or of psychology, by Dr. Sciammana of Rome, reporter.

The study of the criminal had its origin in the purest love for science and the greatest desire to obtain the truth. Perhaps those who commence to gather the history of celebrated criminals, to trace their organisms, to study their special physical conditions, the environments in which they have lived, or to search for the idea or theory that possessed them at the moment of their crime, or the cause which pushed them to it, did it for naught but scientific curiosity. But in the study of criminal anthropology in these latter days these things have changed, and now, thanks to the civilization of our epoch, its truth is sought for its own sake as well as for the practical benefits which may follow. Every one has recognized the practical importance of the study of criminal anthropology. There are, nevertheless, scientists who deny the fecundity of the researches and who believe that crimes are nothing but the result of the free will of the criminal, and that the influence which pushes him to commit the crime had its origin in the same free but evil and wicked will. But we are not obliged to occupy ourselves with these scientists, however wise they may be, because they have confined their investigations only to the field of theory and have never come down to test of investigation by means of experiments.

Our scientific academies, our medical congress, the administration of the prisons, are all now occupying themselves over the questions, what are the individual characteristics of criminals, whether anatomic, psychologic, physiologic or sociologic? And in studying these questions they are moved by the highest order of both charity and pride. They are moved to discover the most rational and satisfactory method for the prevention of crime and the reformation of criminals. Various scientific societies and bodies have taken steps in this direction.

The Society of Anthropology of Brussels organized a commission charged to study the characters of professional criminals, and in the bulletins of that society the members published their investigations on the criminals confined at the prisons at Louvain.

In 1885 the Medical Congress at Antwerp following a communication made by Dr. Semal on the relations of criminality and insanity, voted unanimously to continue these studies, to extend the commission to include the magistrates who tried the criminals, the administrators of the penitentiary and the medical profession.

The International Medical Congress of Barcelona recognized the importance of criminal anthropology and declared that the scientific

inquests were now sufficiently advanced to demand their practical application.

The scientist who desires seriously to study the psychology of a criminal is fairly well received by the prison authorities in all civilized countries, and a good opportunity is given him for study, whether it shall be during the life of the criminals or upon their bodies after death.

In these conditions it is our duty, as we find ourselves representing one of the principal sciences in the world, to report, each one, to this Congress of Criminal Anthropology, what he has done, what he can do in his own country, and thus to gather and unite the largest possible number of discovered and verified facts. This congress, representing all countries, may thus agree upon certain facts as the result of a once separate but now united series, and a law be thus established. That law it is our duty to formulate and proclaim.

In 1884, in Italy, when the general direction of prisons was confided to M. Beltrani-Scalia, one of our most illustrious savants, the Government ordained the autopsy of all criminals who die in the prison of the kingdom. It was thus intended to gather from the cadavers of criminals, a series of anatomic and physiologic facts, by which their history relative to crime, aided by the documents of the prison, could be made known.

Dr. Sciammana said he had been charged to formulate a series of questions, to which all the doctors of the prisons of the kingdoms would respond, relative to the exterior examination of the cadavers, but not including anthropometric researches. To respond conscientiously to the questions by doctors who were entirely unused to them and whose time was already engaged, required much labor and the consumption of much time, and it was concluded by them that the work was too heavy. Therefore, the scheme has not succeeded as well as was expected, and we have to renounce hope for the present of obtaining this scientific material for studies in criminology. To obviate the difficulty, a new formula of questions has been prepared, which while it has reduced somewhat our scientific information, has also so far reduced the labor of answering them, as that the result is even more satisfactory than before.

But there is something to which, in relation to the statistics of crime, the attention of the congress is particularly called. It is not difficult to report all the information concerning the crimes found in the records made by the magistrates or courts who tried the prisoners and the attorney-general who prosecuted them. Also such notes as have been made while the criminals were in prison. But these things are of small utility if there is not also gathered the more precious material concerning the personality of the criminal, the material psychologic, anthropologic, teratologic and anatomo-pathologic, which should be studied by competent medical authorities. To accomplish this it is necessary to follow a single method of study and investigation by which the facts gathered can be compared as though they were done by the

same person. Following this system, those who study the materials of criminology will be able to note the most valuable observations and pursue researches which they believe to be the most profitable. It is one of the important works of this congress, or of its successors, to formulate a code of observation and to establish the common means of recording the results.

These researches, made for the purpose of establishing a system of comparative international statistics, ought to be made both upon the criminal while living and upon his cadaver when dead. The first should be an investigation as to the intellectual capacity of the individual, the modes and manifestations of his affections and moral sense, and the degree of his vital energy and will power. This psychologic investigation ought to be preceded by an anamnestic interrogation of the individual or by an examination of the criminal process against him. Every investigation should include the study of his heredity and neuro-pathology. These anthropologic and clinical researches should be made before the criminal has suffered a prolonged imprisonment; if not, his peculiarities or characteristics may be effected thereby.

The second of the researches should be upon the cadaver, as to its conditions anthropologic and pathologic, so that it can be determined whether the alterations are due to the pre-eminence of morbid tendencies or whether they are the result of an abnormal development due to some other cause. These researches should be made both upon the criminal and the insane, and one can thus see the links which form the psycho-pathologic chain of human life, at one end of which we may find insanity and at the other criminality. Many insane asylums are confided to the care of zealous savants who make these studies and note the results. Attention is called to the exceptional importance of these researches that can be made in the houses of correction, not alone in the interest of science, but that they can serve as a complement to the observations which one may make later upon the same individual if found in the prison. They also may serve as a guide for the treatment and reformation of those who are in the house of correction.

But it is necessary to have a special accord among the savants and the medical authorities of the prisons, insane asylums, and houses of correction so that one can obtain the same researches and results throughout this, whether among the living or upon the cadavers. It is therefore proposed that a commission should be charged to formulate the questions and to establish what might be called a national code of researches, to which it is hoped all nations will accord their favor and adopt.

Question IV.—The conditions determinative of crime and their relative value.

M. Ferri, professor of penal law in the university at Rome and deputy of the Italian Parliament, was the reporter.

The natural genesis of crime obeys a fundamental law by which all crime is only the result of the simultaneous or indivisible concurrence of the conditions of the individual, whether they be biologic or of the surroundings where the individual was born, lived, and acted.

Every crime, no matter who its author, no matter under what circumstances committed, can be explained in one of two ways—either as the act or fiat of the individual's free will or as the natural effect of natural causes. The first of these explanations being without scientific value, it is impossible to explain scientifically a crime (like every other action, human or animal) if it is not considered as the product of an organic constitution or psychic personality which is called upon to act under certain conditions, either of physical or social surroundings.

It is therefore inexact to affirm that the school of criminal positivists can reduce crime to a phenomenon purely and exclusively anthropologic, for, on the contrary, that school has always maintained from its beginning that crime is the effect of multifarious conditions, anthropological, physical, and social, and that these operate together and may determine the crime by an action simultaneous and inseparable; and if the researches into the biologic conditions are more abundant or more apparent because of their novelty, that does not contradict the influence of the sociologic condition upon crime.

We are to consider on this occasion the relative value of these three orders of condition in the natural determination to the commission of crime. A response can not be given absolutely or categorically. Besides, the question is frequently misunderstood and misstated. Those who think that crime is nothing but a phenomenon, purely and exclusively social, without the concurrence in its determination by the criminal of his organic and psychic anomalies, misunderstand the universal union of natural forces and forget that one can not limit in an absolute fashion the infinity of causes, which far or near, direct or indirect, may combine or conspire to produce every phenomenon. This position is as erroneous as to say that the life of a mammal is the effect of the action of a single organ, whether lungs, heart, or stomach, or to say that it is maintained alone by food or drink or the oxygen of the atmosphere, and that each of these produces the entire effect without the aid of the other. If crime be the exclusive product of the social surrounding, how is one to explain the fact known to us every day of our lives, that in the same social status and under equal circumstances of misery, poverty, and ignorance, out of each one hundred individvals sixty are not criminal, commit no crime, and out of the remaining forty, five prefer suicide to crime, five become insane, five become beggars or vagabonds, and only twenty-five out of the hundred become criminals; and among the latter the crimes committed differ in variety,—from those the most bloodthirsty, frightful, and inexcusable, to those which are the mildest misdemeanor, and for which the prisoner may be discharged with only a reprimand. The

secondary differences in social conditions which may be found even among the members of the same family are evidently not sufficient in themselves to explain the enormous differences of these resulting actions.

It is necessary, therefore, to consider this question in a relative sense and to discover which of the three orders of natural causes of crime has the greatest influence in the determination to the commission thereof.

A general or categoric answer can not be given, for the relative influence of the anthropological, physical, and social conditions, vary with each criminal action according to the psychologic and social characters of the individual.

When we consider, for example, the three classes of crimes, those against persons, those against property, those against morality and virtue, it is evident that each order of the determining conditions, and, above all, the biologic conditions and the social conditions, have an influence altogether different in the determination to the crimes of murder, robbery, or violation. And this can be repeated for all categories of crime.

The undeniable influence of social condition, and above all—economic condition in the determination to rob or steal, has much less effect in the determination to murder or violation. And in each category of crimes the influence of the determining conditions is much according to the special forms of criminality. Certain classes of murders (those of occasion) are evidently the effect of social conditions, as, for instance, alcoholism, gambling, public opinion, etc., while certain other murders are the effect of the ferocity or the moral insensibility of the criminal, or else arising from the psycho-pathologic condition which corresponds to organic abnormal conditions. And it is the same with certain offenses against good morals which are in a great part the effect of a social condition which condemns some communities to live together in habitations more as herds of wild beasts than as human beings, with a brutal promiscuity of sexes and ages, parents, children, strangers, boys, girls, etc., which will have the effect to prevent every normal sentiment of virtue or modesty and to efface any such sentiment already formed.

Other crimes of the same nature, but more brutal, are derived from the biologic conditions of the criminal, whether they be the result of a sexual psychopathy or a biologic anomaly. While simple theft or larceny may be somewhat the effect of social or economic conditions, yet these influences have but slight effect in comparison with the impulsion given by the individual constitution, whether organic or psychic, in higher crimes, as robbery with violence, or in murder with intent to rob or steal, or other crimes committed in cold blood.

The same observation can be applied to the conditions of the physical surroundings, for example, the augmentation in the number of crimes against property committed during the cold or winter months,

while on the other hand the augmentation of crimes against the person, whether those of blood or against morality, during the warm or summer months. The reason for these things is that we find the individuals affected, to be in that biologic condition wherein they have the least resistance against these evil influences.

The limits of this paper do not permit the proofs, whether anthropologic, psychologic, or statistic, of these conclusions, but these are only the synthesis of numerous studies and positive investigation made upon the tendency or inducement to crime, by observing the criminals and the causes which affect them. It has been said that for certain crimes and criminals the largest influence ought to be recognized or accorded to the physio-psychic conditions of the individual, which may go from the anthropologic anomaly, scarcely recognizable, to the pathologic state, the most accentuated, yet this does not exclude the possible fact that crime may be a consequence of social condition; that the physio-psychic anomalies of the individual are nothing but the effect of a deleterious social environment which condemns those which it surrounds to an organic and psychic degeneration. This objection might be good when taken in a relative sense, but is without foundation if one seeks to give it an absolute value.

First, it is necessary to remember that cause and effect are themselves only relative, for each effect is in its turn a cause and *vice versa;* so that if misery, poverty, degradation, etc., whether material or moral, is a cause of degeneration, the degeneration becomes in its turn a cause of the misery, poverty, and degradation. And so the discussion becomes metaphysical. Investigators into the relations of crime in different countries (criminal geographers) have claimed a great value for their statistics when they have given the quality of the crime and the number of the criminals in various countries or provinces, and sought to compare one with the other. Instead of these being the differences in biologic condition, as of race; or of physical conditions, as of climate, etc.; they may be governed largely by social or economic conditions; that is, those arising from the differences in agriculture, industry, labor, wages, homes, schools, service in the army, etc.

In the absence of any positive verification, the student of this question may with propriety ask if the social conditions of a given province or country have any real effect upon or relation to its criminality, and whether the social conditions may not be themselves only the effect of the ethnic characters of intelligence, energy, etc., of its inhabitants and the conditions of its climate, soil, etc.

But with more precision one can also aver, even outside the conditions profoundly pathologic, that there are a great number of cases in which the bio-psychic anomalies of the criminals may be the effect of an environment which is physically and morally mephitic.

In each family of several children, in spite of the same surroundings and like favorable conditions, with the same methods of instruction and

education, there will be individuals of different intellectuality, to be remarked from the cradle, as well in the quantity or in the quality of their talent as in their moral and physiologic constitutions. And this phenomenon, although it be evident only in a small number of cases of the most accentuated characteristics, whether normal or abnormal, does not cease to be true also in the more numerous class of cases of mediocre characteristics. The physical and social conditions may have an influence less patent according as the physio-psychic constitution of the individual is stronger and healthier.

The practical conclusion of these general observations upon the natural genesis of crime is this: That each crime is the result of individual physical and social conditions; and because these conditions have an influence preponderating more or less in different crimes or in different forms of criminality, the most sure and certain means that society has or should employ in its defense against or for the prevention of crime, is twofold; and both ought to be employed and developed simultaneously. On the one hand, the amelioration of social conditions, which will serve as a natural prevention of crime; on the other hand, the elimination of those biologic conditions which determine crime; these measures of elimination should be perpetual or temporary. according as their influence on the biologic conditions are permanent and radical, or as they are temporary and changeable.

There are, said Ferri, five kinds of criminals, which should be distinguished each from the other and treated accordingly; the born criminal, the insane criminal, the criminal of occasion, of passion, of habitude. To prevent crime the government or society should, on the one hand, ameliorate the social conditions, and, on the other, eliminate from society either partially or entirely those with defective characters, according to the degree of danger and the possibility of cure.

M. Alimena declared the essential causes of crime to be the social condition and hereditary transmission. According to him the criminal was produced by the same processes as were employed by stock-raisers to rear new races as an improvement of the present races, and adopting the words of Dr. Lacassagne at Rome, "society has no criminals except such as it merits."

Dr. Manouvrier took up the battle. He said they had reduced the importance of the surroundings. If their theory be true that the occasion makes the criminal, then society will make a criminal of the man who is the most inoffensive, and an inoffensive man of him who is most disposed to crime: and he argued his side of the question at length, and with vigor and eloquence.

M. Tarde said we have the agricultural type of man, the military type, the sailor type, and why should we not have the criminal type? Lombroso took it up by saying that it was undoubted that we had among the criminals the type of the assassin, the type of the robber and burglar, and the type of the thief and swindler. M. Moleschott, senator

of Italy, mentioned an influence towards crime that had not been noticed, to wit, the heredity social influence; that is, the tradition which is instilled into the mind of every child, before he knows the difference between right and wrong, that by which he obtains the rudiments of his knowledge of right and wrong. Whether it be correct or not, it is the child's standard. He gets it not from any knowledge or theory of justice, but from the tradition of his own neighborhood, as it is taught by his parents and associates, by the people, and as it is believed by them.

Dr. Manouvrier responded: The argument of M. Ferri on the pre-disposing importance of the anatomic characters proves nothing, because he has taken account of only the general sociologic influences, and not enough of the daily events of infinite details which happen to every man continually from his birth, and while each one of them was of the minimum in itself, yet aggregated made a sociologic surrounding in the life of the man to such extent as to change its form, and make him become what he is. The study of criminality among animals proves that education can change him to be contrary to all his hereditary instincts, even contrary to his essential anatomic organization. M. Rabourdin succeeded in rendering his wolf an honest and respectable animal, so that it would not attack or devour sheep, but would content himself with his regular meals duly served. The regular meal to the wolf played the same rôle that the daily income does to man, by the grace of which many persons who might easily become criminals pass their days with high heads in society and enjoy the confidence of their neighborhoods with a reputation all their lives of being honest men. He elaborated the necessity of consideration in this matter, not only of the number of the conditions and circumstances which had an influence upon us, but still further the arrangement and position relative to these conditions. The possible combinations became infinite and not to be measured, and the realization of two cases apparently alike, theoretically alike, might be practically unlike, and what became in one individual entirely possible became in the other entirely impossible. As to his illustration of the wolf, he said that this was introduced to show how difficult it was to educate any animal to disobey his instincts, but still the illustration proved that it could be done.

Question V.—The infancy of children in its relation to a predisposition to crime. Dr. Romeo Taverni, professor of the University of Catania, Italy, and Dr. Magnan, director of the insane asylum at Sainte Anne, Paris, reporters.

First part by Dr. Romeo Taverni. The science of anatomy can not alone tell us the genesis of crime in an individual man, and it never will, because the moral life of humanity, the most simple phenomenon, will carry us to many causes for its explanation, and must be searched for among many sciences, and will never be found in a single cause nor by a single method. The problem is to search the brain of the

criminal, and find if there be any anomalies which would authorize the idea of a degradation or physical degeneration predominating among that class of men. This problem remains yet an object of study. The results which have come to us up to the present are not conclusive. Among those who make these studies, some have observed too small a number of cases, and others have occupied themselves solely upon the cranial anomalies without interesting themselves with the anomalies of the brain, or *vice versa*, and the researches have not always been exempt from influence or conception *a priori*. They have supposed their task to be to establish imaginary relations between particular dispositions, altogether accidental, of the cerebral convolutions of criminals, and certain normal dispositions of the same convolutions among other persons. The observers have been rare who have sought among criminals for the peculiarities which the surface of the cerebral hemispheres present, and their relation with the type of skull corresponding, and whether these things are or not the same which the anatomist has already found to exist among individuals not criminals. Nevertheless, the observation of several scientific anatomists appear to affirm that there does not exist any special type of skull or of brain in criminals, and this invites us to consider whether there exists any normal type of skull or brain of non-criminals, honest men.

In the skull and brain of criminals the degenerate characters appear with greater frequency than in those not criminals. But the precise value of this comparative frequency is yet insufficiently determined as well as the manner in which these degenerative characters are proven, so that their full power to cause crime or to create a pre-disposition to crime, does not appear as yet established by any law that can be called invariable. No order of somatic anomaly encountered among criminals possesses by itself any signification of a material cause of the delinquency nor a physical pre-disposition to delinquency. Taken together they indicate only the existence of, (1) a degeneration, (2) an organism by which their development has been arrested, or (3) the return of a regressive atavism.

But the physical degradation which is recognized by every fact cannot, according to our experience, be found separated from a moral degradation. Observation has taught us that the brain *sous-micro-cephalic* is perhaps not apt in its function to conceive principles of which the presence in the understanding is a force necessary to the existence of moral life. So that we have learned that a human skull which recalls by its structure the animal form which it resembles, approaches more to the ancestral form than another in which the archaic forms have been effaced.

The moral degradation which physical degradation teaches, belongs exclusively to the general operation of the moral life. We do not possess sufficient experimental knowledge of the anatomic structure of any individual to enable us to say, from this, that he had any determining tendency towards crime, nor that it had in any way a bearing upon

his moral sense. There is no scientific method by which the relationship between his physical structure and his moral sense can be determined, whether the study be made during his life or by autopsy.

(2) The first principle of the science of criminal anthropology, as taught in modern times, is to study the criminal rather than the crime. We have lived among criminals in the prisons of several of the cities as much of the time as was possible. During several years we have kept anamnestic observations and have recorded everything which had relation to the past life of the criminal; but we are not occupied solely in determining, according to the physiognomy of their crime, whether there is any such thing as criminals by instinct. We have never omitted an occasion to interrogate the criminal concerning his parents, his tutors, his friends, his master, his nurses, doctor, all that could give testimony concerning the infancy and youth of our criminals. One hundred and twenty-three of these numerous anamnestic tables have been recorded and give an abundance, an exactitude, a minutia of historic information of such nature as to cause us truly to believe that future researches upon this point can do no more. The tables are of persons condemned for those grave crimes which have been effected by destructive means, whether against the person or of property, or one or both. The sex, age, origin, *etat civil*, profession, the economic condition, religion, intellectual culture of criminals have all been investigated and recorded. There is much variation according to our observation, but we have considered all descriptions and classes of these criminals and have formulated this interesting scientific conclusion: That there is an inaptitude for education in infancy that is evidence of a natural pre-disposition to crime. We have met with cases and occasions where we could base a veritable scientific prognosis which has confirmed the truth of this experimental doctrine.

A methodical investigation has shown to us seventeen children having this inaptitude for education, that we have foreseen with assurance they would become criminals. And they became criminals contrary to the expectation and belief of a number of savants who were obstinate in their opinion that these infants were only backward in their education, and who prophesied that they would succeed if their pedagogy was appropriate. In order to resolve the grand question as to the natural predisposition to crime, the science of criminology ought to demand critical experience of the pedagogic biology. We deeply regret that the general bureau of criminal statistics can not give official information in answer to the two questions: How many children and young people already gathered in the houses of correction become criminal adults? And its complement: How many condemned adults had in their youth been placed in houses of correction?

(3) Our modern civilization has so improved, that it exceeds the natural capacity of many individuals who live in our midst. Modern civilization represents the last and final effort of the individuals who are

the best equipped. Many persons who now might be regarded as more or less criminal would have been esteemed honest if they had been destined to live in the primitive condition of man at the origin of civilization, or, at least, in the civilization of ancient times when our ancestors formed the barbaric races of Europe. Each political government is a vast organism for the social education of all its citizens. Nevertheless there are persons who, by virtue of an instinctive and invincible opposition, reject the possibility of modification by the adapting efficacy of political government. Out of this opposition grows instinct ive criminality. Because of it, criminals perform their actions without being conscious of evil. Giving free course to their instincts, they have only the consciousness of the good of their own individuality. Their selfishness seeks only their own good, and if they are not to be charged with the evil which their acts cause, no more are they entitled to credit for the good. The family is a small copy of society. The historic evolution of the family is that of society in general. There is a law which gives the highest importance to the good order of general society. There is also another law, only second to this, the good order of the family. The law of general society is the same in a greater sense as is the law of the family. The law of good order in the family is intended for the adaptation of the individual to the social law. It is easy to recognize by observation and experiment that there are some individuals, however small the number, who present an insensible, instinctive, and obstinate resistance to the law of the family. This repugnance to family government is sometimes revealed during their infancy. These are the individuals who rebel against education and good order, whether of the family or of the State. The initial adaptation of these individuals to the social law, on which are to be found all ulterior adaptations to law and order, are in a great part achieved by these individuals during their infancy. We ask, in what consists this opposition of the individual, the student, the infant, to the good order, whether of the family or society? How is it explained? It appears to consist in the physical impossibility of the individual to bring into subjection certain of his nervous centers, and his inability to require them to accommodate themselves in their structure so that they can execute with facility all those molecular movements on which depend the acts of obedience to the domestic law, whether of the family or of society. These should be repeated and executed with so little friction as to become habitual, and they can be taught by the ordinary pedagogic process. This want of power in the nervous center brings about in the young person a default in the impressions necessary, by which the moral life of the individual is made to correspond to that of society. As a consequence of this default all idealization which leads to this end, is absent in the student without possible substitution, nor can he effect it by any spontaneous appreciation of his intelligence.

The sentiments of these individuals not only are closed against every civilizing action which educative objects commonly exercise, but the presence of these civilizing influences in the world, and in society or in the family, excites their opposition. They repulse with great efforts their educators and teachers when they would direct them toward their moral teaching, the object of the educators being to prevent this development of antagonism to the laws of society. The efforts even of the educators and teachers to prevent this opposition itself begets an opposition and increases the antagonism of the scholar. The inaptitude for education on the part of the individual arises because of a natural and irremediable defect or a physiologic inaptitude to the social laws of the family that one observes among some children, sometimes without regard to their life or surroundings, education, or example. This constitutes their pre-disposition to crime, and thus has grown up the saying used by many people without knowing that it is true science, sometimes expressed concerning an incorrigible infant, "*Ce fils est né pour la guillotine*," "He was born to be hung."

Dr. Magnan, the head of the insane asylum at Sainte-Anne, Paris, was a joint reporter with Monsieur Taverni upon the foregoing question. Dr. Magnan differed largely from Taverni. He said the question as thus presented seems to admit as an accepted fact an infantile pre-disposition to crime. That, he said, is an assertion maintained by many criminalists, but one to which he refused his adhesion. He said that the opinion that attributes to the most of the criminals an ancestral origin, which considers the criminal born and raised as a savage surviving our present actual civilization, which contends that the infant criminality is only a prolonged example of savagery;—this opinion, he says, has always brought forth contradictions, and he cites certain recent publications:

Tarde, "La criminalité comparée," Paris, 1886. Topinard, "L'anthropologie criminelle—Revue d'anthropologie, No. 6," November, 1887. Ch. Féré, "Dégenérescence et criminalite," Paris, 1888. H. Joly, "Le crime, Etude sociale," Paris, 1888.

Continuing his discussion concerning this supposed pre-disposition to crime, he asked, "Can any one dare to say that there are primordial forms of crime and that they, with the germs of crime, are natural attributes; in other terms, that the infant is naturally disposed to crime and that the criminal is a man deprived of moral sense?" We think this to be an erroneous determination of observed phenomena.

At the moment of birth and for some days after, the infant has nothing more than a vegetative life. It came into the world where it has to live finding itself surrounded by elements which conflict with its organism and provoke re-action. These are only the instinctive expressions of its emotions. All acts of the respiratory, circulatory, digestive, and other organs are a reflex order and do not demand the intervention of either mind or brain; mere life is sufficient for their accomplishment

But soon the acquisitions of the new being begin, and the functions of the brain increase. The door opens to an exterior world; the sight, the hearing, the taste, the smell, the sensations within the periphery of the body permits relations more intimate and complete with the outside world. These new operations bring into play that region in which experimental physiology and pathological anatomy have demonstrated reside the brain centers perceptive and sensitive. This is the organic substratum of our remembrances. In these differences are deposited the lingering images of all our sensorial impressions and it is thence that the centers of ideality draw the necessary material for intellectual elaboration in the formation of ideas. The images passing first to the frontal region, become the representative signs of thought and furnish the elements of our determinations.

The excellent work of Meynert on the structure of the brain has taught us the system of the fibers of association and of projection which are the evidence of this functional evolution. If nothing abnormal intervenes, if none of the wheels of the cerebral mechanism are broken and nothing interferes with the activity of the *sensori-motrice* of infancy then the intervention of the center moderators substitute the active *ideo-motrice* which, under the influence of the attention, based on experience, gives place to, or is followed by, the volitional act of reason. At a very early day in its life the infant begins to obtain or assume control of itself, say of its hands first, which produce the phenomenon of attention and of those conflicting motives, agreeable, or the reverse, which preside over the acts of volition. A chart given in the *psychiatry* of Meynert shows the succession of phenomona in one of these simple mental operations; the image of the flame of the candle thrown by the apparatus of vision on the center cortical posterior, transmits its representation into the frontal region and provokes immediately an involuntary movement of the arms and hands towards the brilliant object. A painful impression, such as a burnt finger however, following an analogous act, acts in an inverse sense upon the *psycho-motrice* region, and a movement of shrinking is apparent. The two sensations, the one pleasurable, the other painful, are compared, the attention is attracted, the education of the moderate center is affected, recognition and memory are called into play, and in what before was only an act of impulse becomes in fact, or at least has the aspect of, deliberation. From the simple vegetative life of the first few days of the infant (simple reflex) it soon passes to the instinctive life (*activité sensori-motrice*) thence to the intellectual life (*activité ideo-motrice*). These three different estates are but three stages of the evolution of one and the same function. The different modes of cerebral activity, the sentiments, will, attention, memory, judgment, reason, etc., that constitute the psychologic faculty develop themselves and become perfected successively by the harmonious action of all parts of the brain. There is a progressive evolution of the mental faculties, until they arrive at that state of conscience which enables us to

discern the true from the false, and the good from the evil; that secret testimony of the soul which gives approbation for good actions, which makes reproaches for evil actions, and is a characteristic of moral sense. The normal individual is not naturally disposed to crime. If he becomes a criminal (criminal of occasion as well as a criminal of habit), he does so under the influence of passion, or of vicious education. The influence of education is well marked in the infant and it takes an exceptional importance in the categories of these unhappy little ones of whom Monsieur Theophile Roussel has taught us so much in his remarkable report made to the Senate on the subjects of abandoned or mal-treated infants, and his project of a law for their protection.

Many of these unfortunate criminals fall under the influence of deplorable surroundings and examples because they are the subjects of a heredity, which may be only nervous or may be the result of alcoholism of their ancestors. This is not a natural pre-disposition for crime, but is a pathologic tare, a degeneration that troubles the cerebral function. Sometimes the center moderators of the brain are not sufficiently strong to repress the unhealthy appetite and curb the improper instinct. Sometimes the center moderators are too feeble to repress the appetites and refuse the unholy demands of these other centers which are in a state of erethism; sometimes, on the contrary, the center moderators are out of equilibrium with themselves and have not that ponderation which, in their normal state will regulate these instinctive phenomona. But this is a pathologic state, and this study of the degenerates of these sick people belongs exclusively to the medical profession and should be submitted to a clinical investigation.

With this preliminary discussion the question is separated from theory and gains in precision and in certainty. It is now reduced to a question of diagnosis. The examination still belongs to the doctor. That these individuals will commit offenses and crimes is of small consequence. The investigation of the doctor goes beyond the commission of the act which is charged as a crime and the inquest should embrace the life of the subject, his atavisms, his physical troubles, as well as the intellectual, moral, and affective modification which they have produced. This detailed analysis and attentive research into the past life of the subject will serve to clear the question and will furnish the best of elements of appreciation upon which the doctor can have his judgment.

We now pass to the discussion proper of the question. The degenerate hereditaries are born with the mark of their origin. Their physical stamps are well known and we do not stop to investigate them. They are here questions of but secondary importance. We pursue at present the study of the anomalies of cerebral development. According to the seat and generalization of the lesions, according to the locality of the functional troubles, the clinical types will be variable, but in spite of their diversity the insensible transitions conduct from one

extremity of the scale to the other, from the degraded idiot to the degenerated superior, intelligent though out of equilibrium.

We have but little here to say of the idiot who lives after a fashion purely vegetative, occasionally even only by instinct. The peripheric or surrounding excitation provoke the cerebral or medullary reflexes; but they are naught but simple reflexes and the center moderators do not intervene. From the time the frontal regions become free the subject commences to penetrate the dominion of realization and of control. He ceases then to be an idiot and is elevated to the dignity of an imbecile. The localization of the lesions in such and such a perceptive center, or of more or less extent in the anterior region, explains to us that such and such faculties have survived the general destruction and thus there exists the partial genius, the learned idiot. The study of the dis-equilibriums, which as a class furnish the delinquents, belongs to mental pathology; and there is in them no great anatomic lesions, but rather the functional troubles on which depend the modification of the activity of the cerebro-spinal axis. The predominating trouble in this class is the want of harmony, the failure of equilibrium, not solely between the mental and intellectual faculties upon one part, and the sentiments and desires upon the other part, but there is a want of harmony of the intellectual faculties between themselves. The want of equilibrium extends to the moral character. A degenerate hereditaire may possibly become a savant, a distinguished magistrate, an eminent mathematician, a sagacious politician, an efficient administrator, and yet he may present from the moral point of view those profound defects, those strange and unaccountable actions; and as on our moral side our sentiments and desires are the basis of our determination, it follows that the brilliant faculties of this individual may be put at the service of an evil cause, that is, at the service of instinct, appetite, unhealthy sentiments, etc., which, owing to the feebleness of the will, push him to acts the most extravagant and sometimes the most dangerous.

The abnormal action of the cerebral and spinal centers gives rise to curious functional troubles which are of the psychic kind. The syndromic episodes, the extreme manifestations of dis-equilibrium, bring to light by their exaggeration, the false psychic mechanism which is found, though in less degree, among these degenerates. For example: The illustrations of the effect of the dis-equilibrium are many, and in their manifestations are different, yet they are all referable and traceable to the one cause—disturbance of mental and moral equilibrium. An individual affected with some malady or just recovered from a spell of sickness, who becomes haunted, tormented till he shall have recalled the desired word, or fixed in its proper place the face of a passing stranger he has somewhere seen before, is conscious that it is only a phantom, yet is unable to throw off the spell, to banish the image which possesses his cortical center; or another case a person is driven as by power, uncontrollable as it is unexplainable, to make an attack upon an

inoffensive and possibly unknown person whom he may find within reach of his fist or weapon; or one torn with a desire for drink; all these are phenomona of the same features and are illustrations of disturbed equilibriums.

In these cases a conflict is engendered between the posterior brain (of which this particular center is in a state of erethism), and the moderating centers. The facts which show these unhealthy impulsions of syndromatic degenerates are analogous to those of other degenerates whose acts are criminal, while in the preceding similar cases the center moderators, in spite of their decreased energy, can, for a time at least, interpose and hold a check or counterbalance on this impulsion. Upon the contrary, among degenerate criminals these centers are scarcely represented. They have small energy, are content to remain idle, will not carry on the contest, and their feeble compulsion leaves the individual without any protest from the anterior region. He is then ruled by his instinct alone, and this without any counterbalance or government.

Conclusion: The infinite changes under which are presented the mental differences of those who are hereditarily degenerate, though they may appear much varied, can be definitely classed as follows:

A. Predominance of intellectual faculty, but moral state defective,—degenerate criminals.

B. Moral state preponderate, but intellectual faculties and aptitudes inactive or wanting.

C. Apparent equilibrium of the faculties, but prominent defect in bringing them into usage, as in application, effort, emotion, etc.

Having gotten this conception of the degenerates, it is not astonishing that cerebral anomalies should manifest themselves in their infancy. These are the original tares which manifest themselves in the psychic life. From the age of 4 or 5 years, even before a vicious education has had time to influence or modify them, these young subjects will present characters of impulsiveness, phenomena of mental arrest, intellectual and moral anomalies, their strange decisions and uncertain actions as though possessed of an evil spirit and by which they can be segregated from their fellows and established into a separate class. These are examples of perverse instinct, cruel impulses, cruelty to animals. Usually these strange anomalies belong only to a special part of the brain which may have been gravely affected by cerebral lesions, or thrown completely out of equilibrium by functional troubles which may provoke in certain centers a great excitement and in others a diminution of their activity. In these children one sometimes encounters a hereditary pathology which may explain the troubles of their cerebral development. The individual cases which serve as illustrations of these propositions are to be found in great number. They are set forth in medical journals and are given by the standard medical authors. In each of these cases and in all others known, it is remarkable that in spite of

these moral monstrosities one does not find any physical modification, or, if so, they are almost imperceptible. Neither is there to be found any physical brand of hereditary or ancestral degeneracy. But a scrutiny of their pathologic life will reveal that from their infancy they have been marked by the breaking out of anomalies of character, of instinctive perversion, by affective sentiments which show themselves in numberless ways. From the very beginning of their psychic life they have been subject to cerebral anomalies. The history of the infancy of a degenerate adult will show the evident defective side of a mental organization from its earliest years and in the case of degenerate infants we know well what signification to attach to the precocious manifestations of a morbid heredity.

Dr. Maugan presented several cases and showed the photographs of many, many more which he said were the hereditary degenerates. Curious enough the most of them were girls, mainly infants from 7 or 9 years old, to 12 and 14. Their conduct as depicted by him was most vile and abominable. It was unnecessarily and unprofitably wicked. Only a few can be given as samples of the best, the worst can not be presented:

Marguerite V., of 12 years, of good physique, and without any apparent mark of physical degeneration, rather good looking, intelligent, but full of vanity, of turbulent and variable humor, subject to violent fits of anger when she broke anything, beat her mother, stole what she could lay her hands upon, and excited her brother to steal. She would bite her little brother without motive and without cause, would take a pin within her mouth and then invite him to kiss her that she might wound him. Her memory was fairly good, but it was sexual troubles which dominated her. - - -

Emile M. would laugh and cry easily and without reason. She had frequent and violent bursts of temper, stole upon every occasion, stole the money from the pockets of her father, took whatever lay about of personal property, would hide in the ashes and cinders the bread, sugar, etc., destroyed the tools and merchandise in her father's shop, declaring she would like to ruin him; she tried to poison him, and on her starting for school in a gay and laughing manner, left a cup of coffee for her father in which she had deposited phosphorus. She tried to kill her twin brother, declaring she would like to kill herself. Then followed the sexual troubles. - - -

Louise C., 9 years old, was the daughter of an insane father. She lived in a state of continual excitement. Her intelligence was debilitated, the evil instincts were highly developed, but nevertheless there was no evidence of malformation, no physical stigma. She was incapable of attention, turbulent, was discharged from several schools. The tendency to steal manifested itself at the age of 3 years, and she indulged it upon every occasion and against the property of every person. At 5 years she was arrested after a most violent resistance. She

was a vagabond, would cry without reason, her memory was feeble, she could read and write, but did not understand arithmetic. She seemed to have no moral sense, was without modesty and knew not virtue. Her actions and conduct was such as not to be described.

Augustine L. was 14 years old. She entered St. Anne at 10 years. Her family back to her grandparents had been seriously affected with epilepsy, alcoholism, delirium, etc. Her physiognomy was agreeable and there were no signs of physical degeneration. She had an excitable disposition, her humors were unequal, sometimes she worked with facility, other times she was incapable of attention. She had alternations of excitement and depression, was unstable, passionate, idle, liar to an extreme degree, was tormented by sexual pre-occupation, was without any moral sense, without modesty, pity, or affection. Nevertheless was not un-intelligent, although her memory had been neglected. Upon occasions she was a good worker, but usually she engaged in all sorts of vagabond, idle, evil life and conduct. - - -

Gorgette J. was 12 years of age. Her physiognomy was agreeable, without any physical stain or stigma that would give the idea that she was a degenerate. The contrast between her physical appearance and her moral state presented a series of deformities unbelievable. She was undisciplined and so could scarcely read or write. Evil practices commenced at 5 years of age and were frightful. Their relations are shocking and impossible to relate.

And so there were others: Jeanne D., Lizzie X., and others again and again quoted by Dr. Magnan, many of whose photographs he exhibited to me. He said those were cited simply as illustrations. The numbers which had come within his observation were many, but even this frequency does not cause us only to accord a secondary importance to these physical signs which are inconstant, and even with the aid of all they seem very difficult to form or constitute a type.

It is not the general contestable characters as yet undetermined, that can be used to clear the conscience of the magistrate. Medical jurisprudence demands from the medical faculty greater certainty. The medical expert can not attain to that necessary degree of precision without complete clinical examination in each particular case. Each case, he said, requires a positive diagnosis in order to respond to the enigmas of the case or the demands of medico-legal inquest.

Dr. Motet presented some statistics and with them general considerations in order to complete the communication of Dr. Magnan. Of the children brought to the house of correction during the 10 years from 1874 to 1884, there were 2,324 children admitted; 680 were illiterate; 1,119 had been abandoned. He was in favor of a strong organization which would give to these unfortunates an education which was at once physical, intellectual, and moral. The agricultural penitentiary colonies were not his ideal when it concerned a child of the large cities. He declared that the State alone ought to have charge and direction

of the education of these unfortunates, and to organize a school of industry where they would be taught proper trades, which trades, he said, could easily be arranged for what is known in commerce as the "articles de Paris," and the needed knowledge taught to the abandoned and illiterate child. He gave as his opinion that this was the duty of the State to provide and care for these children and to so rear them as they should become honest, respectable, and industrious men and women instead of the ignorant, illiterate, degenerate criminals, to become which they were now on the high road.

This report gave rise to a great discussion. MM. Motet, Dalifol, Roussel, and Herbette deplored the condition of the law that placed in the houses of correction—children at an age from 10 to 15 years. If not already criminals, they soon become perverted and ready to become criminals. A more humanitarian law would have sent them to school and to church.

Lombroso said that the perverse instinct of human nature appears even in the first years of the life of the infant. The infant in his first months is likely to be vain, proud, selfish, cruel, without moral sense, without honesty or truth, without knowledge or care for the rights of others, and without affection; and this, said he, is a criminal *embryonnaire.* He thanked Dr. Magnan for having explained many obscure things found in Meynert. Lombroso explained the origin of his studies upon the criminality of infants, and said he had done nothing else than to copy the observers Perez, Spencer, and Tain. In the cases submitted by Dr. Magnan which he had described and many more of which he had exhibited the photographs, Lombroso declared that he could recognize in them the physical characteristics of true criminals. Those which Dr. Magnan declared to be the evidences of a general paralysis, were to his (Lombroso's) mind naught but those of the criminal born. He could see in the degenerates the criminal epileptic, the imbecile, with their stigmas each peculiar to itself. Of the seventy-eight photographs in Dr. Bronardel's album he had found but two who had not the criminal traits.

MM. Moleschott and Van Hamel came to the defense of the infant and invoked its inability of discernment. They declared there were no such things as innate ideas, nor yet was there either criminality or virtue innate. The infant was born unconscious of either. In its early infancy it is not chaste because it is unconscious of shame. It has no respect for the truth, because it does not know the difference between the truth and a lie. The instinct of destruction is very strong, and it destroys with pleasure and satisfaction. M. Moleschott called to mind a trick of Goethe, recounted by himself, in which he described his delight in a scene in his infancy when in the absence of his mother he committed an absolute carnage among the glass and pottery ware. But the sentiment of honesty and virtue and truth developed with age. It is the law of evolution, but it is necessary that we do not confound this phase of evolution with physiologic malady or with criminality.

This view was emphasized by M. Roulet, who said he depended largely upon the physiognomy of the child, to which was added the reports of its conduct. But he declared that during the early infancy there was almost always an absence of discernment. He pleaded for precise detail, close and accurate investigation, and report among the doctors in order to determine the exact nature and degree of capability; and this, he said, was the mission of the anthropologist, who was destined to establish the differential diagnosis of the infant and determine whether it was a natural-born criminal or not, so as to apply the proper measures, whether it be the house of correction, or a simple education.

M. Roulet was a lawyer before the court of appeals of Paris, was secretary of the French union for the defense and the tutelage of infants in moral danger. He said that he had defended during the month of October more than four hundred infants before the tribunal of Seine; infants who were arrested in Paris for insignificant offenses, as vagabondage, begging, and little thefts. He had always pleaded that they were without discernment; that they should be acquitted of the crime, but that the state should have charge of their education. If the infant was acquitted, he demanded that it should be confided to the French Union for the Saving of Infants. Under the operation of this society, the infant was placed in the country and watched over by charitable ladies. If the infant was still evilly disposed, he demanded of the tribunal that he should be sent to the house of correction until he was 20 years of age, where he became the veritable ward of the state. The society of the French Union for the Saving of Infants had been organized in 1887. It was in close relation with the police and with the magistrates and courts: it had sought and obtained their confidence, and there were now remitted into its care a great many children who otherwise must be sent to prison, there to be swallowed up for all time in the everlasting whirlpool of crime. He asked the aid of some anthropologist, who was at the same time an anthropometrician, to visit the Palais de Justice each morning, and go with him through the crowd of arrested children and make the necessary scientific examination that could be perpetuated in the form of statistics; and to this response Dr. Manouvrier promised his assistance by making that appointment for each morning. Their rendezvous would be at the anthropometric laboratory of M. Bertillon.

M. Eschaneur, a Protestant pastor, declared the problem of saving and regeneration of the infant could be brought about only by love.

Dr. Brouardel gave an interesting description of the physical and mental state of the gamins of Paris, so bright and intelligent during their infancy, but which, as has been observed by Lorraine and Tarde, early present the phenomena of a singular degradation. Near their fifteenth year their development was arrested, and a sort of physical decay was produced which led to sexual debasement and perversion, although it did not exclude certain intellectual aptitudes. Some

became musicians, poets, and painters. These indicated troubles of development, which in certain cases produced subjects degraded and debauched, and who, under favoring circumstances, were disposed to the genesis of crime.

M. Theophile Roussel, senator, declared that to properly discuss this question it was necessary to occupy an entire conference. The legislation, however incomplete it might be, had already done much for the protection of infants. The state, which was the head of the grand family, assumes more and more of guardianship over the abandoned or neglected. And he quoted a proposed law which corresponded exactly to the present preoccupation of this congress.

M. Herbette pursued the same course. How should the infant be treated by the state? If it is deprived of the care and protection of its family, the state should become its guardian, its protector, its educator, its father. The state is now largely the protector of infants, whether they be deprived of family or not. It protects the infants in the family against the stupidity, immorality, or crime of the parents; it protects the unfortunate, whether criminal or not, in the house of correction; it protects him before the tribunal and it protects him against himself. because it refuses to give up its guardianship until he shall have arrived at majority. The state endeavors to preserve the infant from ignorance, vice, or crime. While man lives physically, no one has a right to say that he is morally dead. M. Herbette exhibited a chart of the penitentiaries of the country. He insisted that the rôle of education was prevention of the evil in its course, and, without rejecting the intervention of the societies of charity and protection, he demanded above all the surveillance and control of the state.

Question VI.—The organs and functions of sense among criminals. Dr. Frigerio and Dr. Ottelinghi, of Turin, were the reporters.

First part by Dr. Frigerio.

I.—The eye of criminals.—(1) The color of the iris: I have examined the color of the iris of 700 persons normal and 1,500 criminals. I have encountered a predominance of the chestnut-colored iris among the criminals, a considerable proportion of blue among the violators, offenders against public morals.

(2) The chromatic sense: This has been examined in 460 criminals with the method of Holingren. I have encountered but 0.86 per cent. of daltonism, a proportion which is feeble compared with the observations made upon Italians, which has usually given from 1 to 3 per cent. of dischromatopsy.

(3) Visual acuteness: Observations were made upon 100 criminals with the method of Smellen. For refraction we have met with an apparent predominant emmetropic. This visual acuteness is much more developed than among other Italians in the corresponding conditions of life though not criminal.

II.—The skeletons and the form of the nose among criminals.—My observations upon the skeletons have been based upon 609 skulls, among which 397 belong to the normal man, 129 to criminals (75 women and 54 men), 50 were insane, 13 epileptics, and 20 idiots.

The nose of the living person has been studied in 830 persons normal and 392 criminals, of which latter 193 were thieves, 37 swindlers, 28 robbers, 40 murderers, 22 violators. We also examined 60 insane, 40 epileptics, and 10 idiots.

For the observations made upon the skeleton I have encountered the anomaly of the nasal echancrure, that furnishes a new abnormal character of the criminal man, and which I believe to be atavic. To this must be added frequent irregularity of the nasal overture, osynchie, and deviation of the nasal bone.

Among the living the larger number of criminals show a nose square or wavy, of average length, but rather large and often twisted. The robber has often the broken nose; not large, short, wide, mashed, and twisted: the assassin straight, long, excessively large, wide, nearly always protuberant and twisted.

III.—The sense of smell among criminals.—I have examined 80 criminals (50 men and 30 women) and 50 normal persons, 30 men, the most part the guards at the prisons, and 20 women of average culture. I composed for that purpose an osmometre made by twelve aqueous solutions of the essence of giroflée in order of increasing concentration from $\frac{1}{50000}$ to $\frac{1}{100}$, of which 50 cubic centimetres were each placed in a glass bottle with ground stopper. The following were my conclusions:

(1) An inferior sense of smell among criminals as compared with normal persons.

(2) The sense of smell more feeble among women than among men.

(3) The sense of smell more feeble among criminal women than among normal women.

IV.—The sense of taste among criminals.—I examined 60 habitual criminals, born criminals, 20 criminals of occasion, those which yielded to passion, sudden impulse, etc., 20 normal men of the inferior classes, 50 professors and students, 20 women of average intellectual culture, 20 criminal women. All were between 20 and 50 years of age.

Observations were made of the taste bitter, taste sweet, and the taste salty. It was accomplished by a delicate solution of strychnine $\frac{1}{800000}$; of sugar $\frac{1}{1000000}$, and salt, $\frac{1}{500}$. The tables are omitted but the conclusions are given as follows:

(1) The taste is less developed among criminals than among normal persons of the same class.

(2) The taste is less developed among those who are criminals born than among the criminals of occasion.

(3) The sense of taste is slightly less among women than among men.

(4) The sense of taste among criminal women is inferior to that of normal women, but is more delicate than among criminal men.

(5) Several cases of partial failure of taste among criminal men.

V.—The sense of hearing among criminals.—Second part by Dr. Ottelinghi, of Turin.

No organ of sense comes to such perfection in criminals as that of hearing. We have come to this conclusion both from our direct examination and from the information received from the prison guards. It is without doubt true that the disuse of one sense will serve to sharpen another. As is the sense of touch among the blind, so is the sense of hearing among those prisoners who are condemned to silence. In our prisons where silence is required the prisoners have succeeded in establishing means of communication which might rival the telegraphic apparatus. The cells are divided by a corridor along which constantly passes one of the guards, so that the prisoners have no opportunity of communication with each other. It has come to be known definitely and certainly that they communicate with each other by means of a tapping or striking upon the wall or other substance. This sort of telegraphic communication may be likened unto the old Morse alphabet; one stroke for *a*, two for *b*, and other changes and variations for the other letters. They did not use the letter *h*: no reason was given for the omission. Thus it happens that a prisoner will continue his work even in the presence of the guard who is watching him, yet by the strokes which he may make in his work he can communicate with the other prisoners who may be within earshot, and it does not seem to make much difference to them whether the surroundings are in silence or amidst a deafening noise. In case of the latter they seem to be able by their fineness of hearing to pick out the taps or strokes which form the letters, as one would read a book or paper silently, while around him was such a noise as that if he spoke aloud he could scarcely hear his own voice.

Although the guardians wore slippers shod with cloth or felt, intended to enable them to walk noiselessly, yet every criminal detects the difference in the step of the various guards so as to tell which one was approaching.

These examinations were made upon 280 criminals in the prisons. For the most part the sense of hearing was in excellent condition. With their eyes bandaged, standing at a distance of 1 or 2 metres, they could hear the ticktack of a watch. We attempted an experience with the transmission of sound by the aid of the os craniens, but without any conclusion. Our examination of insane criminals was also without conclusion. In the number of autopsies which we made upon insane criminals we have always found the convolution *temporo-sphenoidal* in a proportionate normal state, and have never found that among the criminals condemned to silence, there seemed to be any difference in

the convolution of that portion of the brain, which would tend to show any other than a normal condition or normal activity. If the sharpness of hearing among criminals is engendered by the inertia or disuse of the other senses we were unable to find any physiological or anatomical evidence of it in the brains of those whose autopsies we made.

Question VII.—The determination by means of criminal anthropology of the class of delinquents to which a given criminal may belong. Baron Garofalo, vice-president of the civil tribunal of Naples, reporter.

For the determination of this question a psychological study of the criminal is indispensable, and this is possibly the principal branch of criminal anthropology. The anatomic characters can only furnish indication, and it is necessary to complete the moral figure of the criminal by the investigation of his psychic anomaly.

(1) In order to recognize this psychic anomaly the kind of offense will suffice sometimes. But it is necessary that the phrase "kind of offense" should be employed distinct from the language of the penal code or the judicial theory. Thus, for example, in the case of murder the word premeditation may be insufficient to authorize us to class the offender along with murderers, for one can kill, even with premeditation, the murderer of his father or the seducer of his sister without being thereby classed among the criminals born. All the vengeances of blood, the vendettas, are of the same kind, because there is not a seeking for that egotistic satisfaction which compels the man to murder or makes him criminal born. These offenses are oftener the effect of an altruism, such as *amour propre* or case of honor. On the other hand a man may have the most monstrous criminal nature and yet be a simple murderer without being an assassin; nor is it any better to determine the assassination from the motive, for either murder or assassination may take place without any of the motives which influence the average man. Men in all the enjoyment of their psychic faculties will kill sometimes as though they were savages; sometimes from vanity, sometimes to show their force, their address; sometimes to acquire notoriety. And again, the murder with an apparently sufficient motive, may be nothing more after all than the work of a maniac, epileptic, hysteric, etc. Even in the case of brigandage one can not be sure of the nature of the criminal without having examined him physically and morally. Where brigandage is endemic a son follows his father or his older brother on an expedition which has no other end than to rob the passing travellers and to kill them if they should resist, still he is not to be classed by anthropologists among the born criminals. It may happen that the brigand who, if investigated anthropologically, ethnologically, or morally, would pass the whole examination with high credit marks, would yet in the cases cited follow his father or older brother in his trade or profession and be a brigand.

A classification of the penal code might make no differences between these offenses, while anthropologic and psychologic investigations would have to take account of them.

In order to place a criminal in the degenerate classes of monstrous criminals it is necessary that he should exhibit an innate or instinctive cruelty, such as is found in certain savage peoples. In that case the murder is committed with a purely egotistic aim, that is to say, that the criminal has been moved by a desire of some individual satisfaction; when there has been on the part of the victim an absence of what would constitute provocation on the part of a normal man; when the murder has been accompanied by brutality made with intent to prolong the agony, that it may give pleasure to the fiendish character of the criminal. It is in these terrible crimes, by which the monstrous nature of the criminal is to be recognized. After this be once established there is still to distinguish between the born assassin and the insane or epileptic individual, who is either impelled by an imaginary superior force or else from want of perception of the nature of crime is held to be not responsible.

(2) The cases cited are confessed to be of extreme anomaly. Sometimes the circumstances themselves in which the crime has been committed are sufficient to show the nature of the criminal. In cases where this is in doubt and it is desired to determine to which class he belongs, there should be the examination psychologic and anthropologic. The anthropologic characters are of an importance and often times decisive when taken from the diagnosis of infants or young criminals. There are those who are recognized as having this taint of born criminality by their light offenses, their fighting, lying, cruelty, wantonness, truancy, theft, etc., and those bad boys, incorrigible youngsters, always doing things not simply mischievous, but things which they know to be wrong, though they may not be high crimes. But these individuals, being examined by anthropology, may present at the same time the characters of moral insanity and of innate criminality. The sanguinary instinct manifests itself frequently from the first infancy by a series of acts just described as slight offenses, but which are unjustifiable, frequently repeated, yet of which the parent or teacher in authority takes no notice, because of the youth or feebleness of the child. Arrived at manhood, when he has finished his evil career by assassination, murder, and the higher crimes, then is remembered these minor offenses in his infancy which were the fore-runners of graver and more hideous crimes. In these and similar cases one can find the typical physiognomy of the assassin, the cold regard, the fixed eye, the marked cranial deformation, an excessive length of the lower part of the face, the forehead narrow and retreating, and other regressive signs; or, perhaps, such atypic anomalies as *plagiocephaly* and *scaphocephaly* and among those who commit rape the thickness and grossness of the

lips. And as for the moral sentiment, there may be shown a complete indifference for the victim. Apathy and egotism may be shown by the preoccupation of the criminal as to the possible duration of his punishment and the pleasures of which it will deprive him. If the anthropologic student will charge up against the delinquent the kind and the frequency of these small offenses in his extreme infancy, will note his psychologic and anthropologic characters, and take into account the heredity of vice, of insanity, or of crime, he can prophesy that the infant or young person with these mental and moral characteristics will, if the provocation or opportunity arise, become an assassin. It is not rare for the psychopathic form to manifest itself in subsequent time, and then one may fairly conclude it to be a case of either insanity, epilepsy, or a born criminal.

(3) The physical observation of the delinquent should be continued, to the end that one may distinguish the impulsive characters; that is to say, those characters which impede or prevent moral resistance to the passions which excite to crime, principally anger, vengeance, alcoholism, insanity, epilepsy, and certain other characteristics which descend by heredity. This class of delinquents are midway between the malefactors by instinct and those of occasion. Although this tendency to crime is a germ in their individual organisms, which becomes semi-pathologic, yet the germ will rest latent and unproductive, if there is not added to it an impulsion from the exterior world. This impulsion is required in order to cause them to commit crime which leads us to class them as criminals of occasion. As soon as this exterior impulsion is found to be not necessary, or, if the crime is immoderate as compared with the impulsion, then the delinquent is to be classed as a criminal born.

The regressive anomalies of the skull and of the physiognomic type of inferior races that has been so frequently remarked in the criminal born are nearly always absent from the impulsive criminal. But on the other hand these latter are characterized by nervous anomalies, and by other striking maladies. It follows as a result of this theory that in murders or assaults arising from a quarrel or riot, one can easily understand how there can be two classes of criminals—the criminal impulsive, and the criminal by chance. The first, which are partially criminals born, are much more dangerous to society than the latter. They may commit crime from disease as much as from instinct and ought to be made objects of particular treatment, as much by the medical man in the hospital as the policeman in the prison.

(4) The terms used in jurisprudence for the description of a great number of crimes signifies nearly nothing for the anthropologist. In the science of criminal anthropology the author of a given crime may be ranged under different classes of criminals. He may be a criminal born; he may be a criminal impulsive, or only a criminal of occasion.

According to the penal law there are but two terms: the criminal and the punishment, while criminal anthropology, the new science, has three terms: (1) the crime, (2) the criminal, and (3) the punishment or the adapted repressive measures. These repressive measures are to be again divided according as they are applied to the different classes of criminals.

(5) In classing as criminals those who commit offenses against property, such as robbers, thieves, swindlers, forgers, etc., psychology plays a rôle even more important than anthropology. The sentiment of probity is less instinctive than that of charity or pity and is not dependent upon the organism because it is more recent and less transmissible by heredity. It happens that exterior causes, such as the surroundings, conditions, examples, education, and economic conditions may have a greater effect upon this species of criminality. In the case of the robber or thief, along with the morbid form, kleptomania, there is an instinct to steal caused by heredity or atavism, which is often manifested by anthropologic signs and above all by special physiognomy. The most striking characters are those mentioned by Lombroso of the extreme mobility of the face and hands, small and bright eye, heavy and continuous eyebrows, the *camus* nose, small and retreating forehead, etc.

When these characteristics are found upon the recidivist, that is, the incorrigible criminal, one can be sure that he has to do with a criminal born. It is frequent that among vagabonds, robbers, thieves, and other criminals against property there is a physical and moral neurastheny, a term coined by Benedikt, of Vienna; that is to say, an aversion to labor and to every moral combat for the right, derived from a nervous constitution, and which is combined with, or perhaps has produced a desire to enjoy the pleasures of life and to indulge in its luxuries quite beyond his means. When the circumstances of life are hard upon such an individual, and he is subjected to an economic or social crisis, he is more likely to become a criminal, because crime may aid him in the satisfaction of his desires. To this neurasthenic class belong the vagabonds, thieves, and swindlers, whose improbity may have commenced by unfortunate circumstances, such as being out of work, loss of place, evil company, bad example, and improper moral education, and which ends in his becoming an instinctive criminal. The neurasthenic and the habitual or instinctive criminal ought therefore to be grouped together, because they are equally incorrigible, until at least the social and economic situation of the former shall become so changed as to offer them the enjoyment of all pleasures and luxuries which they desire without the need to work. It is necessary, however, to make exceptions for young persons who are driven into vagabondage and are thieves by bad examples, and evil surroundings and associations. Although they may have become habitual criminals,

yet they may not be incorrigible, certainly not until they shall have arrived at the age when the character is fixed.

(6) It follows as a necessary conclusion that as each of these classes of delinquents may be determined with anything approaching precision an enlightened legislature should adopt a special treatment. It is not astonishing that the legislators and magistrates who make and deal with the criminal laws should repulse the services and the aid of psychology and anthropology, and should persist in their *a priori* perceptions and in uniform precepts, without giving consideration to the infinite variety in criminals produced by so many different causes and influenced so differently by surroundings, all of which go in such supreme degree to form the guilty and reprehensible intent with which the crime was committed, or which on the other hand may take away that intent and form either a justification or excuse.

M. Puglia gave his unqualified assent and support to the propositions advanced by Baron Garofalo.

M. Alimena, on the contrary, assailed the entire classification. According to him the examination, whether anthropological, physical, or psychological, was insufficient to more than raise presumptions and invent theories, while certainty was required in dealing with judicial questions and cases. If exterior and physical anomalies are appreciated, why not apply the same rule to internal anomalies? What, he demanded, did it signify as to the depth or size, more or less, of the occipital fossette in the skull of Charlotte Corday which we now saw in the collection of Prince Roland Bonaparte? If it indicates, as is claimed, that she was a born criminal, then instead of being a heroine who rid the world of a monster, she was naught but a common, vulgar, impulsive murderess.

The difference should be recognized between a purely scientific treatment of criminals and the practical treatment which they must receive under the law. If science advances so does the law. But they go at different rates. Science flies on wings of the mind, while the law marches along in stately and dignified tread with leaden sandals. Scientific errors are easily corrected. They do no harm. They come down upon us and envelop us as does the fog the earth, but like the mists of the morning which fade away before the sunlight of heaven, so do they under the light of investigation; while the jurisprudence of the country, solid and enduring, and, more like the earth which has been hidden, remains after the fog has been dissolved into a few drops of dew.

He expressed his opinion that of all these sciences, psychology would be most productive in results, and he much regretted that the schools of law and of medicine did not teach this science.

Lombroso responded that his works or his opinions were not opposed to nor contradicted by any psychologic diagnosis. He returned to the skull of Charlotte Corday, which he said demonstrated anatomic char-

acters of the criminal born, such as platycephalic, the occipital fossette, and other characters of the viril skull.

Dr. Topinard responded to him by affirming that the skull of Charlotte Corday was normal, and that it presents all the proper characters of the skull of a woman. The platycephalic was a normal character and the vermicular fossette was not an anomaly, and there was nothing irregular in the skull unless it should be its platycephalic, and he said it was rare or never that a skull was the same in all its parts and on both its sides. Nearly all skulls showed a difference or distinction on the one side or the other.

M. Benedikt opposed this theory of the craniometric methods and also the psychologic characteristics enumerated by Baron Garofalo, which, he said, would belong equally to the dyspeptics and the neuralitics. It was easy to make hypotheses, and according to his belief one had as much right to say that the occipital fossette was an indication of a pre-disposition to hemorrhoids as much as it was to crime.

Ferri and Lombroso replied vigorously to Dr. Benedikt, while Senator Moleschott came to his aid.

Dr. Brouardel recalled the speakers to the discussion of the report of Baron Garofalo. The problem proposed by him was a classification of criminals. The crime itself is insufficient to class the criminal. The decision must be upon all the evidence. One insane act is not sufficient to characterize an insane person. It must be established by the antecedents of the subject, his former life, his peculiarities, and his physical signs. This was the only true system to be pursued, and any purely physical or purely psychologic examination would be insufficient and was to be repulsed entirely. Suppose the theories of Baron Garofalo to prevail, then a criminal born, according to his views, should be arrested at once and confined in some special establishment.

M. Herbette took up the discussion and enumerated the results obtained by the administration of the penitentiaries. We have, said he, at one time the prisoners and the sick people. The prison is not a hospital. The hospital is an association for the good of the sick and where they may furnish a subject of study and experience. In the most of them the entry is free, and in all the departure equally free. In the prison the situation is entirely different. The prisoner is imprisoned as a result of the penal right of society to protect itself.

M. Lacassagne protested that for the sake of science, for the sake of society, for the sake of investigation into crime and its causes, the law should give to the prison authorities the right to investigate the biology of the criminal and the sole control of the cadaver of the criminal, whether his death was inflicted by the law or came from other causes.

But M. Herbette declared he would not go so far, and he counseled patience, study, careful investigation, great conservatism, regard for the feelings of the public, so to the end there should be no revulsion on their part, for the reforms which were forced might bring great risks to science and compromise its success.

Question VIII.—The conditional liberation of criminals. Dr. Semal, director of the insane asylum of the state at Mons, Belgium, reporter.

(1) In studying the right of society to punish a criminal, one is struck with the insistance of the law upon the characters and circumstances of the offense, without the slightest examination into the personalities or conditions of the delinquent. Dr. Semal advocated a psycho-moral examination of the delinquent in order to determine his condition, whether he was a confirmed criminal or only a criminal on occasion; and whether he might not in the one case be given a conditional liberation, and in the other be continued indefinitely in confinement. One of the theories of the penal code which forms a foundation for the right to punish, is the possible reformation of the delinquent; but the idea of a fixed term of imprisonment as a punishment for one class, and another term for another class of offenders, is opposed to the theory of possible reformation. To give this idea of reformation full effect, there should be a conditional liberation which should take effect sooner in one proper case, and later, or not at all, in an improper case. He declared a scheme of conditional liberation could be provided which would be more rational, more humane, and more successful in the reformation of criminals.

The jurist, in writing on this subject, contents himself to remain within the limits of the written law, and declares himself satisfied by the uniform and inflexible application of formulas which have been crystallized in the codes. The decay of these doctrines will appear where to the safety of the public or society is added the desire to reform the criminal. But their destruction will not be complete until crime is regarded as a natural phenomenon which can be prevented by a study of the social and individual causes which lead up to it. From this there are to be made two deductions: (1) If the punishment is the principal object of the repressive system, why should it be prolonged when it has contributed all it can to the reformation of the condemned? This is the foundation of conditional liberation. (2) If the penal condemnation is sufficient to awaken in the heart of the delinquent his heretofore smothered sentiments of right and justice, and if the moral effect of his offense is complete by the fact of his condemnation, why should he be compelled to serve, or even enter upon, a term of imprisonment? And from this has sprung the theory of conditional sentence. These two propositions contain the germs of the radical reform of the repressive system. They tend to give to the convicted criminal the opportunity to determine by his conduct if he will have his sentence postponed indefinitely, and his liberation made at once, even though it be on probation and under surveillance, he to be returned to prison on his first movement towards a return to his former criminal life.

(2) The proposed law of conditional liberation would operate upon the sentiments of the condemned person, of which we can suppose the

existence; and in order to establish with certainty this proposition, it is proposed to give him a scientific psychologic examination.

Man can be judged only by his acts. There may be a sort of latent criminality always ready to explode under the shock of propitious circumstances, as an expression of a diathesic stage dominated by heredity, and of which biologic science can enumerate the signs. A psychologic analysis is indispensable in order to determine these questions. The necessity of a psychologic examination of the delinquent is imposed because it is the only method by which one can determine the existence of such sentiments as will authorize the conditional liberation or ought to postpone the punishment.

(3) As to the practicability of this we have to remark that the present theory and past experience has only resulted in a multiplication of punishment without having reduced the extent of criminality; and this, whether in the number of the crimes, their frequency, or their grades. By the old system neither the genesis or evolution of crime has been studied; neither the legislator nor the jurist seem to have ever considered why an evil-minded minority should persevere in the commission of crime while the majority of people are honest, well disposed, and of good repute. It is therefore towards the modern school of positivists that we must turn for a solution of this matter, because it alone seems to have studied crime as a natural phenomenon arising from multiple causes.

(4) The principle of the reformation of the criminal by the operation of the penal system is in contradiction with the fixation in advance of the duration of the cure to which the delinquent has to submit. The new theory of jurisprudence will permit whoever or whatever criminal shall show himself to be repentant and inoffensive to be conditionally liberated, and this offer should be made or the opportunity given even to those who refuse or those who find themselves in the impossibility to reform. The reformation of the delinquent, or at least his resignation to and respect for social laws, is the essence of this theory of conditional liberation. But, as one can count to a certain extent upon the vitality of the criminal instinct, and with the persistence of the social conditions which nourished it, it is necessary to prepare for the eventuality of a prolonged incarceration which may be regarded as the result of incurability on the part of the criminal. The idea is to proportion the length of the imprisonment according to the nature of the delinquent, to the degree of his perversity, and the danger of his return to society before his evil tendencies shall have become enfeebled or neutralized. It is evident that this is more rational than to fix a time certain for his imprisonment according to the condition of his offense, which may furnish only an isolated system of the moral malady with which he has been attacked and which was the cause of the commission of his crime. The proposed law of conditional libera-

tion can correct any erroneous verdict or judgment or work any reduction of the term of imprisonment.

(5) The proposed law of conditional condemnation is upon the same principle as that of conditional liberation. It corresponds somewhat to the practice prevailing in some States of the United States of suspension of sentence during the indefinite period of good behavior. It is a measure generous and wise, is addressed to delinquents of tender years,—those who have been arrested for the first time, who may be the victims of circumstances, who are without criminal intent, and who, if the sentence be suspended, would probably never be guilty of the offense again, while, if their sentence should now be carried into execution, it would almost certainly result in the loss to society of one who might become an honest and respected member thereof, and gain in his place he who might easily become a hardened criminal. But the application of this principle is or will be surrounded by researches extremely delicate, which ought to be highly scientific and so lengthened as to include the antecedents of the delinquent, his life, his raising, his surroundings, and to get if possible into the interior of his soul. The word "delicate" has been used, and truly this is necessary, for the responsibility is great, for as the judge may by refusal to suspend sentence lose a member of good society, so also he may by a suspension of sentence grant indulgence to unworthy subjects and be deceived by hypocritical pretenses and promises, crocodile tears manufactured for the occasion and practiced upon him by a hardened and instinctive criminal.

(6) The instinctive delinquency of the young criminal is not absolutely in relation with the enormity of the crime. This imposes upon the jurist the necessity of a proper selection from among the arrested as well as among those imprisoned as to whom, in justice, to apply the different systems of treatment. The operation of these two systems, the one of which operates upon those subjects which can possibly be reformed, the other with the prolonged and continued punishment and incarceration, even in solitary confinement, of incorrigible subjects, who, if allowed their liberty in the least degree, will use it only for the contamination of their fellow-prisoners and the preparation and arrangement for themselves to enter into a wider sphere of crime upon their release. These are the foundations of the two systems.

(7) Individualization is necessary in order to recognize and class the delinquents, and to determine whether the medicine to be administered to him for his cure should be of incarceration or liberation. Sometimes it might be better to adopt the plan of solitary confinement in order to conduct properly this individualization. An anthropologic examination or a psychologic analysis may not be sufficient to determine to which class he should belong, and therefore he should be tried under different conditions, always bringing out his real and heartfelt sentiment, thus enabling one to determine to which class he belongs and

whether he should be conditionally liberated or continued in solitary confinement. To this end an opportunity must be given both by restraining his liberty until he shall be in solitary confinement or extending it until he shall be conditionally liberated. His actions and the psychologic effect which this has upon him must determine the future course to be pursued with him. In doubtful cases the conditional liberation is the most rational, as it is the most humane. It gives the delinquent an opportunity to reclaim himself, and gives him a guaranty that his attempts at reformation will be well seconded.

(8) After having returned to society those of whom we have nothing more to fear in the way of criminal offenses, after having taken all necessary precautions for those who are to remain under surveillance and possible return, it is necessary to take steps for those individuals who are by nature rebels and refractory, who reject all ordinary means of reformation, who are delinquents by habitude, and are instinctive criminals. For these individuals their detention, even to solitary confinement, with severe and hard labor, should be kept up until they give proof of their repentance. If this is refused then we in France and on the continent can only relegate them to a penal colony in a distant ocean or else to solitary confinement in one of our home penitentiaries. The relegation of a recidivist or an incorrigible to a penal colony, solitary confinement, or some other form of severe punishment, or else treating him as sick or insane and sending of him to a prison asylum; these are the logical corollaries of the propositions for conditional liberation.

The criminal, conditionally liberated, should be required to report for examination whenever needed, and thus the prisoners who are under condemnation of the law would become physical subjects for the study of crime in its psychologic as well as anthropologic phases, and the prison become as well an asylum and a hospital, affording a clinic for the lawyer, for the doctor, the judge, and the lawmaker.

M. Alimena called the attention of the congress to the fact that this question had been discussed for a long time and in many places by legislators and jurists, and he referred to the first congress of the International Union of Criminal Law, held at Brussels, in 1889, where the discussion took place upon the thesis presented by Senator Michaud on the law of pardon. He said three methods had been proposed—the conditional sentence, which was enforced in Belgium; the suspension of judgment, which was practiced in England, America, and Australia; and finally that of blame, set forth in the German code, the Russian, Spanish, Portugese, and in some of the cantons of Switzerland and provinces of Italy.

M. Drill remarked that the system of conditional liberation required the exercise of two functions—that of the judgment of the court passing upon the guilt of the criminal, and the ulterior or subsequent treatment of the criminal, and that these were functions entirely different and ought

to be separated. The first belonged to the judge and the court, and the second belonged to the administration of the penitentiary. He thought these ought to be kept separate, and it was clearly his opinion that the judge or the court alone should decide upon the culpability of the individual and the application of the penal law. The administration of the penitentiary should be composed of, or should call to its aid, the most competent scientific gentlemen, who would be able to pass upon any question concerning the physical, physiological, or psychological characteristics of the individual, and this, taking in consideration his antecedents, his social condition and surroundings, his education, companions, etc., together with his conduct while in prison, would enable them to decide upon the application of the conditional liberation.*

M. Bertillon, while giving all credit to the scientific investigations mentioned, begged the congress not to forget that the final end was primarily for the safety and well-being of society, and the reformation or well-being of the criminal only secondary.

Question IX.—Crime in its relation with ethnography. Dr. Alvarez Taladriz, of Valladalid, reporter.

M. Ferri had already described the ethnic influence upon crime, so Dr. Taladriz sought to establish a tendency towards crime on the part of a whole people; the criminality of a nation or of races. He sought to show how the crimes in the Northern, Middle, and Southern Spain, were different, and also the difference in criminals. He declared this difference to be due to the advent of Charles I and Philip II, as Kings, and that it was but an exposition of the ferocious instinct of the primitive inhabitants of the forests of Germany.

The mesologic influences are confirmed by history in such manner as that it ought to recall to the student of sociologic influence the statistics of offenses committed in the cold and warm countries, those between the region of the North and the region of the South. These questions have not been studied from a geographic or ethnic point of view. It is proper that they should be. There probably is no place in which this ethnic influence upon crime could be studied with greater success and accuracy than in Spain, where there are such ethnic differences between the people of the different parts of that country, and where one will find a corresponding difference in the crimes committed. In the north of Spain offenses are of a character distinct from those of the center and south. Crimes against person and property are rare. Those which exist are the result of inherited, primitive usages and customs like in the vast mountainous Basque provinces of Catalonia, the kingdoms of Galicia, the Asturias, and Leon, where assassination and homicide show the terrible characters of the sediment of population

* The legislatures of Massachusetts and New Jersey have lately adopted a system of conditional liberation.

deposited by the preceding races of Germany during the grand period of invasion of the tribes of the north who occupied these regions more than any other part of the peninsula.

The miners of the center of Spain do not present those characters of ferocity, because their elements are a concourse of varied and multiplied antecedents of the successive dominations which have come to pass in the peninsula.

In the kingdoms of Valencia and the Andulasian provinces, the criminal customs of the Arab race were handed down as a souvenir of the Kabyles, where the inhabitants organized themselves into a band of malefactors. The crimes of homicide, assassination, in the majority of cases were only the result of the passion of jealousy coupled with a hate truly African and which considerably augments the number of offenses against persons and property. Nevertheless, we recall certain acts of nobility, the Arab hospitality, etc. True, there may be exceptions found, as there will always be, to general rules, but the conclusions are:

(1) The physiologic characters of the criminal type manifest themselves in a constant and uniform manner in all epochs and in all races, and without other variations than those imposed by accidental and external circumstances from these epochs and races.

(2) The conditions of race, climate, geography have their influences upon the senses and passions of mankind and upon the development of crime, as well as upon sociology, religion, economics, or politics.

(3) The grand offenses committed by races and nations ought to be an object of an international penal code by which they could be punished with a certainty and uniformity that would bring them to an end; while in the same code could be declared the sacred right of nations and of individuals, which should be recognized by all the world.

Question (37).—Medico-psychologic observations upon Russian criminals. M. J. Orchanski, of Charkow, Russia, reporter.

M. Orchanski is professor of the university at Charkow. He was not present to read his paper, and it was presented by Dr. Brouardel in connection with Question IX. Only the conclusions were read and they were in opposition to the Italian school. The paper consisted of arguments and deductions, and did not deal in testimony or statistics.

Dr. Topinard took the opportunity to present his opposition to the title "Criminal Anthropology" and thought it should be replaced by that of "Criminology," as being shorter, easier, better understood, having a clearer meaning, and with everything to recommend the change.

Dr. Manouvrier preferred the the term "Anthropologie Juridique."

Question X.—The ancient and new theories of moral responsibility. M. Tarde, juge d'instruction at Sarlet, Dordogne, reporter.

This was a long and learned disquisition upon moral responsibility.

The opening paragraph declared that moral responsibility depended upon free will, which, at least, in its relation to crime, was a hypothesis without foundation in truth or justification in law. The discussion became more philosophical and metaphysical than practical. The most careful report would fail to do it justice or render satisfaction to its author, and it is therefore deemed wise to omit it.

Question XI.—The criminal process considered from a point of view of sociology. M. A. Pugliese, of Trani, reporter.

The moment appears opportune to make the criminal process an object of the study of penal sociology.

(1) The criminal process is an institution of State established in the social interest, having for its end the search for and repression of crime. The general rules of its formation provide for the discovery and appreciation of crime, the punishment of the author, and the conciliation of the social and individual interest. To do this properly requires a magistrate who has technical as well as general knowledge. It is not sufficient in these times of the discovery and investigations of anthropology that he should be simply a judge or even a jurist. It is necessary that he should be acquainted with the studies of anthropology and sociology; that he should understand the social surroundings in which the crime is committed as well as the men who commit it. Whether the State should found the necessary institutions of learning for the training of these magistrates was a question for discussion, but it is indisputable that they should have a special training. Prosecutors are charged with the trial of criminal offenses. In western Europe these things are not satisfactory; a juge d'instruction, or prosecuting officer, scarcely possesses any special training or had any special qualification to fit him for his position. Perhaps he has never written a criminal process, never seen a cadaver, or attended an autopsy. He knows nothing of anthropology nor of penal sociology, and yet he is called upon to exercise functions the most delicate, most difficult, on which depends the safety of the citizens and their social surety. He obtains his experience *in corpore vivo;* he learns at the expense of society. In doing blacksmith's work he becomes a blacksmith, and when he shall have become habituated to his position, and qualified in even a mediocre manner, he will be changed to another place with another duty, and another person will replace him to begin again this new life of study and practice. This is not a system but is only education. The faults, and the scandal are enormous. Sixty per cent. of criminal processes fail. The real culpables have a good chance of escape, while the innocent run the danger of losing their honor, their liberty, and, possibly, their life.

It is evident that the criminal process should not, as at present, be limited to the gathering of the proofs pell mell. On the contrary, the prosecutor ought to study the evil and secret causes of the criminal actions, and from them deduce the true reason of punishment. They

ought to seek also for the precedents somatic, psychic, and social, and discover the conditions, surroundings, environments, not only of this particular criminal but of all that have gone to produce such criminal phenomena. It is now time to search for such indications as can be furnished by anthropology and by criminal statistics, not only for identity, as given by the works of Bertillon, Voisin, and Herbette, but also the biology of crime as has been investigated by Ferri, Garofalo, and Righini.

(2) The investigation and trial should be confided to those who have been technically educated, experts of special training, one for the prosecution and another chosen by the defense. The defense ought to be admitted to take measures, to ask questions of medical jurisprudence, such as he may need in the interest of his client, and upon these questions the debate should take place and the judgment rendered. This would not be a mere opinion, but would be a true decision of a technical commission, which would settle at once and forever all debate upon that question. It would be a trial before a technical jury as to the questions of medicine or medical jurisprudence or psychiatry. It would also raise the professional dignity of the medical jury, and would assure the world that, cost what it might, the research would be in the interest of truth. The right of the judge to demand the decision of science, and along with it the right and the power to trample the decision under his feet is a manifest contradiction. We who have always maintained that it is not reasonable to submit to a common jury questions of medical jurisprudence, think it time to overturn the ancient maxim that the judge is the expert of experts. The maxim may flatter the vanity of the judge, but it is not true. Each one to his place is the truth. When a question of medical jurisprudence arises the medical jurist ought to be the judge.

This question was brought up at the session of the congress at Rome. Drs. Tamassia and Laccasagne presented it. There was an important debate thereon, and the principle here laid down was approved with a single exception We propose that questions of medical jurisprudence, of psychiatry, should be tried before a technical jury, and that they should be authorized not simply to make a suggestion and give an opinion, but to render that which is a real decision and a final judgment. We believe the proposition laid down in the Holy Scriptures to be the true one, to give to Christ that which belongs to Christ and to Cæsar that which belongs to Cæsar.

(3) There should be established a system of preventive detention, that is to say, there should be a detention for the purpose of preventing crime by means of imprisonment of the individual before he has committed it, rather than to imprison him after as a punishment for having committed it. The penal process or code in the Latin countries consists of the two steps, one of instruction and the other accusation. In the first the presumption of innocence prevails, and there the preventive

detention should be the exception, but in the second it ought to be the rule. But these things are to be determined by the psychic condition of the delinquent and the nature of the causes which impelled him to crime. If the psychic conditions have been verified there should be no further hesitation, but the imprisonment or detention should be enforced with rigor.

(4) The judge gives his judgment in three forms: Condemnation,—acquittal for inexistence of the crime or of his innocence;—acquittal for insufficiency of proof. This corresponds to the ancient formula: *Condemno, absolvo, non liquet.* The jury, on the contrary, except in Scotland, have only two formulas: Yes, no; guilty or not guilty. If they are in doubt as to his guilt, they respond not guilty. This does not appear just. The jury should have a formula of *non liquet*—not proven; the laws would then be equal for all.

(5) There should be an appeal in criminal cases as well in acquittals as in convictions. This question was treated by Garofalo, Ferri, Maino, and by Pugliese in the *Revue de Jurisprudence* in 1885. It has been argued in the affirmative by Mittermaier in his *Die Gesetzgebung und Rechtsbildung.*

In this principle it has received its first legislative recognition in paragraph 388 of the Austrian code and paragraph 399 of the Germanic code. But in these cases it is confined only to corruption or false testimony. It is time, however, that the principle of appeal in the social interest should be recognized without restriction and appeals be taken as easily by the prosecution as by the defense. The law ought to be equal for all. The interest for the one and of the other are the same. No reason in justice can be given why one should have an appeal and the other not. It would serve to correct many erroneous, not to say corrupt, judgments and prevent many scandals upon the law.

Dr. Brouardel accepted much said by M. Pugliese, but he combatted some positions. He denied the propriety of making an expert to be a judge or making judges only of experts. The responsibility was too great and the result would be unsatisfactory.

M. Benedikt agreed with Dr. Brouardel and said that while the education of the magistrature should include certain prescribed medical studies, they should be always auxiliary to jurisprudence and never above or beyond it. This was in accordance with the opinion of M. Lacassagne.

Question XVI.—Instruction in medical jurisprudence in the law schools. Professor Lacassagne, of Lyons, reporter.

In the presentation of this paper M. Lacassagne repeated largely the ideas which he had put before the congress at Rome upon the necessity of instruction in medical jurisprudence in the law school. There was a large discussion over this question, but it was confined to the details,

all the speakers, Brouardel, Moleschott, Van Hamel, Ploix, Féré, Tarde, Soutzo, Ferri, and Madame Clemence-Royer, were in accord with the proposition. It was finally agreed to recommend the examples of the universities of Holland and Belgium, to which might have been added Trinity College, Dublin, all of which have a special course of medicine in their law schools. It was recommended that even in these courses should be extended to include a large proportion of anthropology, for Madame Clemence-Royer recalled that according to Socrates the first study of man should be man himself.

M. Soutzo insisted that to teach criminal anthropology was to teach medical jurisprudence, and he cited examples among the insane. A paralytic by virtue of his delirium becomes a robber or a thief. In his perverted senses he falls into dipsomania. Another, which, attacked by the mania of persecution, becomes a murderer or a suicide. Another category of individuals who are on the frontiers of insanity may be found in the degenerates, the morally perverted, the drunkards, and all that train of individuals capable of committing crime according to their conditions and surroundings, and among which are to be found the stigmas, physical, moral, and intellectual, that have been taught to us by the professors of criminal anthropology before us. These individuals are not, like the first, absolutely irresponsible, but they are partially or conditionally so. Therefore, said he, the great necessity for the teaching of criminal anthropology, not by the side of, but including medical jurisprudence, and that this should be carried on in all the schools of law, and taught to all those who would become lawyers or judges, or who would have dealings with criminals or insane before the courts or under the law.

ANTHROPOMETRY.

There were two papers before the congress on this subject: No. XVII, "Anthropometry as applied to persons from 15 to 20 years of age," Alphonse Bertillon, reporter; and No. XVIII, "The employment of the methods of criminal anthropology in aid of the police and for the arrest of criminals," MM. Avocat Anfosso, of Turin, and Professor Romiti, reporters.

Anthropometry is a branch of the science of anthropology by which the physical characteristics of man are studied, the investigation being made by measurement.

The application of anthropometry is twofold. One, the more extensive and more scientific, was largely the result of the investigations of Broca, though there were others who practised the science independent of and even before him. Quetelet of Belgium, Virchow of Germany, Roberts, Francis, Galton, and Dr. John Beddoe of England, and our own doctors Morton and Baxter have all practised anthropometry independently of Broca. In France Drs. Topinard and Manouvrier have taken up the science where Broca left it at his death. The former has

been pursuing his investigations into the races of men found in France as determined by color, and he investigates and studies that of the eyes and hair as well as that of the skin. The latter succeeded Broca in the Labratoire d'Anthropologie, and is professor and lecturer upon this subject before the School of Anthropology.

The second use of anthropometry has been more practical, for, while it is conducted scientifically, it is employed in Europe, principally in France, as a means of identification of individuals, whether required in the army, by the law, by the police, or for private and scientific uses. It was with regard to the second application of anthropometry that the congress of criminal anthropology occupied itself in the two papers set forth at the head of this chapter.

The discovery of the use of anthropometry for identification is due to Dr. Adolph Bertillon, himself a professor in the school of anthropology, who died in 1883 at the age of 62 years, leaving his two sons to follow in his footsteps, with prospects of becoming equally as eminent as their father. It was the son, Alphonse, who presented question XVII, in which he was assisted by MM. Anfosso and Romiti, the reporters of question XVIII, both of whom were aided in the discussion by M. Cantilo, advocat from the Argentine Republic.

M. Herbette, chief of the penitentiary system of France, early perceived the benefits of this system and adopted it. It is now in use throughout France, thanks to his initiation. He was its ardent advocate at the congress in Rome, and there made it the subject of an address, which was translated by Mr. Edward R. Spearman, a portion of which was adapted and published in the Fortnightly Review of March, 1890.

M. Alphonse Bertillon is attached to the department of justice and assigned to duty with M. Herbette at police headquarters in Paris, there to use his talent and knowledge in the identification of such persons as may be brought before him. This, of course, means the identification of criminals, or persons arrested.

The morning of Friday, August 16, was devoted to a visit by the congress to the establishment in charge of M. Bertillon to witness the operations of his methods and to hear his explanations. We, however, were favored with a private view on the day before, by the means of which we were better enabled to understand the operations.

The establishment to which we were introduced would correspond to and probably be known in most cities of the United States as the rogue's gallery. In our country a criminal once arrested, whom they may desire to recognize at some future time, is marched down to a photographic establishment and has his photograph taken by a single negative, *carte de visite* size, of more or less front view, from which a print is made, which in due time is delivered to the detective corps at police headquarters, where it is placed in a rack for public inspection. It is by comparison with this photograph, and the recognition of wit-

nesses, that the individual criminal will be identified in future, if he should be again arrested. It goes without saying that these methods are extremely unreliable—unreliable at best, but in Paris impracticable and valueless, for there they have no less than 100,000 photographs of criminals who have passed through the police headquarters within the past 10 years. It will be recognized as practically impossible to search through a pile of 100,000 photographs to find one which shall bear a likeness to the individual under investigation. It would be impracticable, if the photograph, when found, should prove to be the picture of the identical criminal whose case was being investigated, but when we consider the differences of appearance of the same individual, and the similarity of different individuals, as shown by the photograph, the impossibility of successful identification becomes indisputable. To be of any value as means of identification, there should be two photographs taken of each person, one full face, the other a profile. If this be done with the small size, $2\frac{3}{4}$ by $3\frac{1}{2}$ inches, it would require 10,600 square feet surface measure for 100,000 photographs. These displayed on a wall in a strip 5 feet in height would require a space 2,120 feet in length. A search through such a dreary extent of photographs in order to find the particular one to compare with the criminal, whom the officer leads around, and thus be able to identify him, would be like a search among the sands upon the seashore, or the leaves in the forest, and its impossibility, or, at least, impracticability is demonstrated.

M. Bertillon has so arranged his system of anthropometry, and classified it—together with the photographs—as that his usual search does not extend beyond twenty, and rarely above ten, and can easily be reduced as occasion demands, and be accomplished in a few minutes. Upon the occasion of my visit he gave to Professor Mason and myself a descriptive card of a given criminal, who was brought and measured in our presence—upon the visit of the congress M. Moleschott, senateur from Italy, was given a like chart; and we were instructed to make the search for ourselves and so understand the classification and find and identify the criminal. The system proved so perfect that we three, strangers, making our first visit to the establishment, hearing the description for the first time, were enabled to understand the classification and find the box in which his description belonged, with no more than ten cards in it, and so identify the man in question, and this we did within two minutes time. I will describe the method of procedure and the system of classification:

The instruments.—These are few and simple. Their cost is about $25. A series of them were displayed by their maker, M. Colas, at the Exposition in the department of anthropology, and I have described them in the chapter on Anthropology at the Exposition.

A wooden right-angle for taking the measure of the height. Calipers for measuring length and breadth of head; two sliding measures of

different lengths for other parts of the body, and the necessary stands, stools, etc. These will all be understood as the operation proceeds.

The batch of "arrests" have been brought in for measurement and identification; under the necessary guard they are conducted to a room divided around its walls into open lockers after the fashion of public bath houses. The individual is stripped to his shirt and pantaloons and these lockers are provided with hooks on which to hang the clothing, and a bench with a drawer. Thence he is marched into the measuring room. The services of two men are required; one to take the measurements, the other to write them on the appropriate card. The subject may have already been examined, or he may be examined here as to his name, residence, place of birth, and former convictions, if any. If he be a hardened criminal, an incorrigible, called in French, *a recidivist*, he will probably give a false name and declare this is his first arrest.

The report of the bureau at Paris shows the following list of persons who did this and were recognized by this system and their descriptive cards found in the boxes as hereafter explained:

	Persons.
1883	49
1884	241
1885	450
1886	352
1888	615

The report for 1886 in full was as follows:

	French.	Foreigners.	Total.
Subjects measured for first time	9,517	1,140	10,657
Same returned under same name	4,521	173	4,694
Same returned under false name and identified	303	49	352
Total measured			15,703

All measures of anthropometry should be taken by the metric system and reported in millimetres. By common consent among the principal nations the metric system has been adopted for anthropometry. Comparisons are made much easier and more correctly from a single and universal standard, and therefore it becomes the duty of the United States to fall into line with her sister nations.

To measure the height of the individual.—By a simple mechanical contrivance the operation can be done rapidly, accurately, and without risk of deception. The subject is barefoot and placed with his back against the wall; a strip of wood has been fastened upon the wall so as to furnish a perpendicular edge; a door or window jamb may serve the purpose equally well. The wooden right angle spoken of can be placed against this edge and moved up and down, the broad bottom of which can rest lightly upon the head of the individual. Lines painted upon the wall, or stripes with the necessary measures of height marked upon them, will show with accuracy the height of the individual.

Maximum length of the head (skull).—The subject being seated, for convenience, one point of the calipers is placed in the hollow above the bridge of the nose, together while the other point is used to find the greatest length at the back of the head. This should be done with accuracy, and so that the length will be given exactly. If done with care the true length can be obtained within 1 millimetre, which is about one twenty-fifth part of an inch. It is admitted that the skull of man developes but little, if any, after his maturity, 21 years of age. No one possesses any power to alter or in any way change the size or conformation of his skull. The same thing is true with regard to the length of bones in the human body, and this had afforded the key to the system of anthropometry adopted by M. Bertillon, as he has chosen for his identification those portions of the body over which the individual has no control, and in which it is impossible for him to make any change in their size or length. The length of the head thus taken is a measurement at once accurate, unchangeable, and beyond the control of the individual or the possibility of deception.

Maximum breadth of head.—This is measured from one parietal bone to the other in the same manner as the length of the head is measured.

Maximum length of arms, extended.—This is a measurement which is popularly supposed to be always equal to the height, but in reality it may vary from 5 to 20 centimetres. It assists therefore in classifying even after the height.

Length of middle finger of left hand.—This is the best of our indications, for it can be measured to a millimetre, provided care is taken that the finger is bent at an exact right angle with the back of the hand; there can be no cheating with this and it undergoes no alteration from adult to old age. Notice must, however, be taken of any unusual length of nail in the person being measured.

Maximum length of left foot.—In taking this measurement the subject must, of course, be barefoot, and in order to avoid any chance of cheating the subject should stand on the left foot only, with the left knee bent. This is not quite so good a measurement for our purposes as that of the middle finger, and can only be measured to within 2 millimetres.

Color of the eyes.—A special table has been framed for the color of the eyes, which gives seven categories. These are based on the intensity of the pigmentation of the iris. Firstly, we note the exact shade of the pigment when it exists, and secondly, the approximate shade of the deep stratum of the periphery of the iris.

Hence the seven divisions:

(1) Iris azure blue and slaty blue with aureole concentric pupillary aureole more or less pale but destitute of yellowish pigment.

(2) Iris inclining more or less to blue or slate color, but with a light yellowish aureole.

(3) Same shade but with a further aureole, approaching orange.

(4) Iris reflection more or less greenish and with a chestnut aureole.

(5) Same shade with brown aureole.

(6) In this class the chestnut is no longer clustered in an aureole around the pupil, but spread on the whole surface of the iris and only shows some greenish yellow irisations.

(7) Eye entirely brown.

This grouping enables us to pass by almost imperceptible transitions from the light blue eye to the pure brown eye. To examine the eyes the operator should place himself in the angle of a window, his back to the light,—avoid using the word *gray*. For further details read the *Revue Scientifique* of July 18, 1885; also, *Annales de Démographie*, 1881–'82, "*La couleur de l'Iris en lanthropologie*," by Alphonse Bertillon.

This procedure gives six measures of each individual, but upon necessity they can be increased indefinitely. The effect is twofold. One is to procure a reliable means of identification of the individual by means of an accurate measurement of certain portions, the bony structure of his body, which in the case of the adults does not change. Fatness or leanness, well or ill condition, has no effect upon these measurements. They are and always will be (except the height) the same, and neither by will or trick can any one make them different. The other effect is to provide an arrangement by which the cards may be segregated and classified so that the individual can easily be found.

The cards on which these measurements are recorded are of a regular size and pattern, with printed forms, so as to always give the same indication. The size used by M. Bertillon is 5¾ inches square. Both sides are utilized for description, and on the one are placed the two photographs—front and profile view—the full face on the right, profile on the left.

These cards are then arranged in boxes or drawers after the manner of call cards in the U. S. National Museum; that is, on edge, the face to the front, the depth of the box being not more than half the height of the card so that it can easily be seen and read during examination without being taken out.

The classification of these cards and photographs in their boxes is such that the descriptive card of any individual will fall into a subdivision of not more than ten or twenty other cards, and can be found, as was done by Signor Moleschott, Professor Mason, and myself within a space of 2 minutes.

M. Bertillon has at Paris 100,000 photographs of criminals and arrested persons, and these are increasing at a wonderfully rapid rate. The proportion of 40,000 may be excluded from our present consideration, being those of women and children. Sixty thousand are of men of mature age, and as we have already seen the measurements were made of those portions of the body of the bony structures, the size of which or length of which can not be changed.

The principle of the classification of M. Bertillon is to divide each one of these measurements into three classes: the large, the small, and the medium. This classification, beginning with the length of the head, then to its width, extends through all the measurements indicated, and ends in a division containing about ten cards, but which must not exceed twenty. The lines of demarcation between these divisions are made arbitrarily and with the sole intent to make each division approximately equal in point of numbers. So he has found the numbers for line of division for the length of skull to be at 184 and 189 millimetres. All heads the length of which fell between these two numbers inclusive, constituted the middle division; all of 183 and less formed the division of short heads, while all of 190 and more constituted the division of long heads.

For the breadth of the skull the two dividing figures were 153 to 156, and these formed the middle division. Those 152 and less formed the shortest, and those 159 and over formed the broadest division; and this system was continued throughout all other measurements.

It was found in practice that this slight difference of 5 millimetres, being only about one-fifth part of an inch, taken, as it were, out of the middle of head measurements, would contain about an equal number with those in the other two divisions.

The divisions made by the measurement of the middle finger of the left hand established for the medium class from 110 to 115; all middle fingers from 109 and under are classed with the short; from 110 to 115 with the medium, and 116 and over with the long fingers. So also with the length of the foot, the spread of the arms, and, as I have said, by the color of the eyes.

In practice the 60,000 photographs would be first divided according to the length of the head, large, medium, and small; and this would separate them into three divisions of 20,000 each, in the case of drawers. The width of the head would again divide each one of these 20,000 into large, small, and medium, which would give practically 6,000. The three divisions arising from the spread of the arms and the length of the middle finger will reduce it to 600. The length of the foot will again reduce it to 63, and the further reduction by the color of the eyes of seven classes to 9 photographs in each division. The principal divisions are made in the cases of drawers, while the smaller are made within the drawers themselves.

The anthropometric establishment under M. Bertillon does not abolish the use of photography. The photographs are taken in double, a full face and a profile, and this should always be done. The change of face arising either from accident or intention on the part of the subject is much less easily controlled by him in profile view than of the full face. He can at best only change the lower part of his face, and in making comparisons by photographs, where such a change is suspected, it is well to cover the lower part of the face on the photograph by a spot of

paper and make comparisons of the contour of the head, the shape of the face, the position of the ear and its appearance, and thus one is enabled to make much better and more satisfactory investigation. If one would rely upon the photograph there should also be added the other position of a full-length standing portrait.

At Paris the studio for taking the photographs of criminals is attached to the establishment of M. Bertillon and is over his office of measuring. Another suggestion which he makes concerning photographs and their benefit and advantage concerning identification is the necessity of having them the same proportion, the same relative size, and so he insists that the instruments and the subjects shall always be at the same distance. Therefore he has the chair in which the subject sits, and also the stand for the camera fastened firmly to the floor so that they give the same proportionate size of the subject.

M. Bertillon also remarked the importance of including in the photograph a view of the bust. If the head only be shown it gives it an enlarged appearance and so is deceiving, and besides the setting of the head upon the shoulders is as much a means of identification as is the head itself. He said also to throw back the hair off the ears of the subject when taking the profile view, for it is an organ unchangeable upon its owner and with its characteristics may serve as a means for identification. But with all this M. Bertillon uses the photograph more as an auxiliary, and depends principally upon the measurements.

How to make a search.—Our man, whose photograph and measurement is given on the card, is supposed to have just arrived, the measurement made, and his photograph taken. We desire to know if he has ever before passed through the depot and whether his card of measurement is here to be found. The length of his head is 191, therefore we find it in the highest division; that is, with the longest heads, and we know it will be in this row of drawers. The width of his head is 157. That falls within the medium class, and we therefore know it will fall within this row of drawers. We have now, by exclusion, reduced the number of cards to be examined from 60,000 to 6,000. The length of his middle finger is 127, which throws it into the highest of that division, and that has reduced it to 2,000. The like investigations with regard to his foot, which is 278, and the spread of his arms, which are 151, reduces it, as we have said, to an average division of 63 cards. These are divided among the seven distinctive colors of the eyes, and so the package of cards within which his description will be found, if at all, is reduced to an average of 9, and in practice is never to exceed 20. And this by depending solely upon the measurement and without consulting the photograph.

As a precaution additional to the normal sizes of the various portions of the body which were selected for measurement, there would be naturally employed any abnormal marks which might be found. If these were agreed in the two descriptions we would declare the identification com-

plete. Every person has on his body some particular marks, such as moles, scars from cuts, boils, etc. Three or four of these corresponding would be quite enough to identify a man out of a million provided always that the nature, etc., of the marks has been accurately recorded.

It is very seldom that one finds on an individual identically the same mark and in the same place that has been previously noticed on another, but that two persons should be found bearing three or four scars precisely similar would be a co incidence which appears impossible, and we have certainly never met with such a case.

These marks and cicatrice are set forth under the appropriate head on the back on the card of Feillier.

I will not attempt to translate that description. It is too intricate and with too many abbreviations and private marks for me to do so with certainty. But as an illustration I may quote those which were presented by M. Bertillon to the congress at Rome, and which had been translated by Mr. Spearman:

(1) Oblique outward scar between second and third joint middle of first finger left hand.

(2) Scar oblique inward of 5 centimetres, left palm, 3 centimetres above third finger.

(3) Mole 8 centimetres below left nipple, and at 10 centimetres from center of body.

(4) Mole 4 centimetres left of spinal column, 20 below prominent vertebra of neck.

If this series of private marks be found to correspond on the two cards, one would say they were both made from the same individual and that the identification was perfect.

It is not to be expected that an inexperienced person will be able to do this work of anthropometry without error. In the beginnings of the system there were fewer identifications of former criminals and more failures, but as time has progressed and a certain expertness with regard to measuring and accuracy in making and keeping the records, these errors and failures have been so far eliminated as that Monsieur Bertillon claims it to be practically perfect.

The anthropometric service in the penitentiary and police system of France was established in 1882. The annual examinations were as follows: In 1882, 225; 1883, 7,336; 1884, 10,398; 1885, 14,965; 1886, 15,703; 1887, 19,150. Up to this time the service was considered more or less experimental, and only certain classes were subject to measurement.

In the year 1888 the application of the system was extended to include all persons arrested for any except the lower grades of offenses, and the number in this year who passed through the depot at Paris was increased to 31,849. This gives an average of about 100 measurements per day. M. Bertillon told me that in practice it took the two men, one to measure the other to record, about 7 minutes to each prisoner, or 8

prisoners per hour. As it is important that prisoners should be examined in court without delay the entire day is not at their disposal, and so they have four squads of operators, who endeavor to conclude their measurements each day before breakfast, as they call it; that is, before 12 m., the afternoon being devoted to the routine business at the office.

Of the 31,849 offenders or suspects measured in 1888 615 were recognized as having been measured before, but who sought to conceal their identity by giving false names and reporting falsely the number of their arrests. There were only four failures of identification. Four failures out of 31,849 measurements was considered by M. Bertillon to be practically perfection.

This system of M. Bertillon for identification of individuals by means of anthropometry is having much success. The most superficial examination seems to convince every one of its efficacy and superiority. M. Cantilo, Procureur General at Buenos Ayres, the delegate from the Argentine Republic, bore his testimony before the congress of the marvelous results obtained in the determination of individual identity. He said that the method had been adopted by several of the States of the United States of North America, and also by his own country, the Argentine Republic, the capital, Buenos Ayres, already possessing an installation of the anthropometric system of Bertillon. He spoke of the necessity for its adoption by all civilized countries, and he proposed to the congress a resolution inviting all governments to adopt it whenever they might have need for the identification of any considerable number of their citizens, which resolution was unanimously adopted.

M. Bertillon stated that after France the Argentine Republic was the first government to adopt the anthropometric system by law or official decree. He complimented the admirable application made in the State of Illinois, principally at the penitentiary of Joliet, by the private efforts of MM. Mac-Claughry, Gallas, Muller, Porteous, of Chicago, etc.

Monsieur Herbette, in his presentation of this matter at the congress of Rome, following the communication of M. Bertillon,* pointed out how this verifying of the physical personality and the indisputable identity of people of adult age should in modern society fulfill real requirements and under the most varied services. If it were a question, for instance, of identifying the soldiers of an army, or travellers going to distant lands, they could have personal cards having recognizable signs enabling them always to prove who they are; if it were a question of completing the records of the *etat civil* by sure indications to prevent error or substitution of persons; if it were a question of recording the distinctive marks of an individual in documents, titles, contracts so that his identity could be established either for his own interest, for the interest of third parties, or for that of the state, the full benefit of the anthropometric system would be realized. If there should arise a question of identity in a life certificate, a life insurance

* Translation by Mr. Spearman.

contract or proof of death, or to certify the identity of a dead person, or one badly wounded or disfigured, the body having been partially destroyed or had become difficult to recognize in case of a sudden or violent death, the result of a crime, an accident, a shipwreck, a battle, how great would be the advantage of being able to trace these characters, unchangeable in each individual, infinitely variably as between one individual and another, indelible, in great part, even in death.

There is still more cause to occupy oneself with it if it is a question of identifying distant persons or after the lapse of a considerable time when the general appearance, the look, the features, and the physical habits have changed naturally or artificially, and that without moving or expense, by the simple exchange of a few notes or figures sent from one country to another, from one continent to another, to be able to know in America what sort of a man it is who has just arrived from France, and to show clearly whether a certain traveler one finds in Rome is the same person that one measured in Stockholm 10 years before.

In one word to fix the human personality, give to each human being an identity, an individuality lasting, unchangeable, always recognizable, easily proved, this appears to be the extended aim of the new method.

It may consequently be said that the extent of the problem, as well as the importance of its solution, far exceeds the limits of penitentiary work, and the interest, not inconsiderable, which final action has exercised amongst various nations. These are the motives for giving to the labors of M. Bertillon and their practical utilization, the publicity they merit.

Question XIX.—Correctional education—reforms in accordance with our knowledge of biology and of sociology and their relations to crime. Dr. Motet, reporter.

Dr. Motet, in accordance with M. Dalifol, presented the necessity for a considerable development in moral education as well as professional. Especially should this be so in the agricultural schools, and M. Van Hamel came to their aid in showing the success which had attended the moral education in his country of Holland.

Question XXI.—The relation between mental degeneration and simulation of insanity. Dr. Paul Garnier, reporter.

The boundary between crime and insanity is very narrow and one which gives to the medical jurist sometimes the greatest difficulty. It is here that the real criminal will simulate insanity before the courts in order to escape the responsibility of his acts, and here is to be found the greatest number of the simulators. The degenerate individual, he who has come to be of a lower scale, whether mentally or psychologically, is closely related to and liable to become either epileptic or hysteric. If he shall simulate either one of these or the insanity growing

out of them, he may be his own dupe, and finish by becoming the insane person that he at first only pretended to be. The simulation, even when successful, does not necessarily give evidence of intellectual ability. It does not in these cases require a high order of intellectual ability to deceive; deceit is not intelligence. It is many times difficult to detect insanity in a given individual, but it is much more difficult to detect the simulation of insanity. To do this with certainty requires the most skillful and best trained scientist. A moment's consideration of the proposition will serve to confirm the opinions so many times expressed by members of this Congress as to the necessity for an anthropological education and training on the part of the judges and law officers dealing with criminals.

Question XXII.—The influence of professional life upon criminality. Dr. Henri Coutagne, of Lyon, reporter.

The object of this memoir was to present the importance of those studies which had for their object a research into what the reporter called "professional psychology," or the psychology of professional life. He said the psychic functions of the individual were greatly influenced by the profession he chose to exercise among his fellows. That the vocation or profession showed the tendency of races or of individuals. He spoke of the special aptitude of the Hebrew race for financial affairs. His memoir was as much graphic as written, and showed nine classes of professions, and the criminals which had belonged to each. This had been continued and kept up by him and his predecessor since the year 1829, and was devoted largely to statistics as well as enforcing their value and importance. These statistics showed that much the larger proportion of criminals is to be found among the agricultural and industrial population. He enlarged upon the necessity for statistics, and invoked the various societies, as the bar associations, the medical societies, and those representing other trades and professions, to gather with thoroughness and detail the number of criminals, the habit of life of the various individuals, and especially this with regard to their course in crime. The congress drifted into a discussion as to the importance of statistics, those to be gathered as well by the state as by the different societies and organizations mentioned.

M. Herbette enlarged upon the necessity for complete and accurate statistics gathered by the penitentiaries and prisons, and spoke of the necessity of what he called "a bulletin official individual," which should show every act in crime and in life and in the surroundings of the individual, his temptations, opportunities, his first tendencies to crime, and his criminal life both in and out of prison, so far as possible, and to this should be added the anthropologic and psychologic investigations.

Dr. Wilson, from the United States of America, after noticing the necessity for a general plan of gathering statistics with accuracy and detail, and making a collation and classification of reports for purposes of comparison, and the fact that thus defined there were scarcely any

statistics in the United States in relation to crime and criminals, went on to say that only in some of the States were records kept so that statistics could be obtained.

New York and Massachusetts are the most prominent. But their records are kept, each on its own plan and without relation to the plan of the other, and therefore they lose the benefit of comparison with each other. In most of the States of the Union there has been only a slight attempt to keep vital statistics. Marriages, births, deaths, conviction for crime, are intended to be made a matter of record, but the penalty provided by law for the neglect is so slight and so rarely enforced as to be ineffectual. Ours is a new country; our people have never been accustomed to strictness in making or keeping such records. The population in many localities is sparse, the people change their residence often, they go and come at will, there is no military service demanded of them, and it is exceedingly rare for a pauper to be returned to the place of his original domicil that he may be supported at public expense. So the needs which exist in Europe for such records fail in the United States. The only necessity for such statistics is believed by our people to be for historic or sociologic purposes. This has not yet been sufficiently appreciated by them to overcome the difficulties. There are also more difficulties than exist in European countries. Our country is large; compared with European countries it has a vast extent. It was also as compared with these countries, discovered only a few years ago. It has had only about 100 years of life. One hundred years ago it had but 3,000,000 souls; it extends from the Atlantic to the Pacific, a distance of nigh 5,000 miles, and its center of population remained, until within 50 years, practically on the Atlantic coast, and even now has not gone beyond 600 miles to the westward. Our country had to be rescued from the possession of the barbarian, and a people thus engaged have but little time and less inclination to keep records and statistics which in their opinion have only a sentimental utility. So it has as yet been scarcely attempted. We may accomplish it after a time; not at present. The difficulties are increased by our form of government. We have that anomaly of two sovereignties within one country, two governments over one people; and I explained the difference between our State and national governments, each of which has its own jurisdiction over crime, and yet each is independent of the other. So I said the United States Census is dependent largely for its statistics of crime upon information obtained from the State authorities. If, on the other hand, it be a State census, each will be separate and distinct, and may be different from any other. So it was that in the State of Pennsylvania the statistics of crime showed the number of convictions to be 2,930, while the State of New York, with but a slightly increased population returned 58,670 convictions; twenty times more than that of Pennsylvania. The explanation given was, that in the former State convictions only in the courts of

record were reported, while in the latter the convictions were of every kind, whether for small or great offenses.

The meager statistics of crime in the United States, taken from the census of 1880, and reported by Mr. A. R. Spofford in his American Almanac, are given in the following table:

State.	Criminals in prison.	Population in 1880.	State.	Criminals in prison.	Population in 1880.
1. Alabama	Unknown.	1,262,344	20. Missouri	1,294	2,169,091
2. Arkansas	Unknown.	802,562	21. Nebraska	....	452,432
3. California	615	864,686	22. Nevada	144	62,265
4. Connecticut	251	622,683	23. New Hampshire	180	347,784
5. Delaware	....	146,654	24. New Jersey	823	1,130,892
6. Florida	71	266,566	25. New York	3,576	5,083,173
7. Georgia	590	1,538,983	26. North Carolina	....	1,400,000
8. Illinois	1,900	3,078,636	27. Ohio	1,362	3,197,794
9. Indiana	1,231	1,798,358	28. Oregon	104	174,767
10. Iowa	353	1,624,463	29. Pennsylvania	1,861	4,282,738
11. Kansas	406	995,335	30. Rhode Island	....	276,528
12. Kentucky	983	1,648,599	31. South Carolina	625	995,706
13. Louisiana	625	940,263	32. Tennessee	1,153	1,542,463
14. Maine	221	648,945	33. Texas	....	1,598,509
15. Maryland	170	935,139	34. Vermont	175	332,286
16. Massachusetts	757	1,783,086	35. Virginia	1,105	1,312,203
17. Michigan	809	1,634,096	36. West Virginia	218	618,193
18. Minnesota	235	780,807	37. Wisconsin	309	1,315,386
19. Mississippi	997	1,131,899			

Question XXVI.—Political offenses from the point of view of anthropology.

This study, written by M. Laschi, an avocat from Verona, was made with the assistance of M. Lombroso. It dealt with race, genius, and the density of the population in the older and better settled countries. The author distinguished revolution from revolt. The first he called psychologic manifestations, and the latter pathologic. He spoke of the influences of climate and orography, not to mention those social and political, upon the race which might belong to or inhabit a country. He gave as his opinion, derived from his investigation and the statistics, that the short-headed races, brachycephalics, were conservative, while the long-headed, dolicocephalics, were revolutionists; that the mixture of these races could modify their character and so change them as a nation, but that occasionally, by reason of atavism, or something similar, peculiar circumstances, changes in social conditions as well as in political, the dolicocephalic individual of modern times and in modern countries might break out in revolution, which was naught else on his part than the return, through heredity, to the original revolutionary characters of some remote ancestor. He said the most revolutionary cities of Europe, like Paris, Florence, Geneva, were those which manifested the greatest genius and the most vivacity of thought.

Drs. Brouardel and Motet believed, on the contrary, that the influence of political crimes was to show the inferiority of intelligence, the

fanatism, the impressionability, and the exaltation of the individual These, said they, were particular factors in political crimes.

Professor Lombroso cited M. Taine, and said that these political crimes were what the anthropological historian might well call political epilepsy.

Question XXVII.—Jurisprudence applied to criminal sociology. M Pierre Sarraute, judge of the tribunal at Perigueux, Dordogne, reporter

The punishment for crime ought to be against the individual. The particular individual criminal should be made to feel that he received the punishment for his offense. To accomplish this with satisfaction the juge d'instruction should be able to investigate the anthropologic and social factors which have entered into or operated upon the mind of the criminal in causing him to commit the offense. The juge d'instruction must himself be educated, and it must be remitted into his hands entirely to judge of the utility, and extent of the examination, and to control the results. To do this successfully it will be necessary to open a course of lectures upon criminal anthropology and medical jurisprudence in the various schools of law, and to educate the students in these sciences. The reporter proposed as a remedy for some of the lapses in the law, and the miscarriages of justice, an indeterminate sentence by the judge; he proposed profound modifications in the jury system, requiring of them in particular cases, special aptitude, special preparations or educations, enabling them to deal properly with the subject in hand. He would reduce the number of the jurors and would require them to give their answers to the questions submitted to them by the court, which answers should establish the facts in the case with which they as jurors alone had to deal, leaving the questions of law to the judge of the court; leaving the anthropologic questions, those of psychology and physiology, to the trained scientist, who should be a criminal anthropologist. With a training of the lawyers and judges in these various sciences, and then a division of their various duties and responsibilities, with higher courts which should combine in them these various branches of scientific knowledge, the right of the criminal would be guarded, while crime would be lessened and society protected.

Question XXX.—The moral and criminal responsibility of deaf-mutes in their relations to legislation. M. Giampietro, of Naples, reporter.

He argued the defective physical organization of deaf mutes, and seemed to say that there was a corresponding want of responsibility which should be recognized by the law and the court. The important part of his paper, which can not be here followed, was the scientific portion, the physiologic investigations into the conditions of deaf-mutes and the formation of articulate language. He described certain brain centers which were possessed of such functions in this regard. He called them the centers *auditif*, *phonique*, *volitif*, *mnemonique*, *ideosymbolique*, and *moteur*.

COLOR-VISION AND COLOR-BLINDNESS.*

By R. Brudenell Carter.

It is a matter of familiar knowledge that the sense of vision is called into activity by the formation, on the retina or internal nervous expansion of the eye, of an inverted optical image of external objects—an image precisely analogous to that of the photographic camera. The retina lines the interior of the eyeball over somewhat more than its posterior hemisphere. It is a very delicate transparent membrane, about one-fifth of a millimetre in thickness at its thickest part, near the entrance of the optic nerve, and it gradually diminishes to less than half that thickness at its periphery. It is resolvable by the microscope into ten layers, which are united together by a web of connective tissue, which also carries blood vessels to minister to the maintenance of the structure. I need only refer to two of these layers: the anterior or fiber-layer, mainly composed of the fibers of the optic nerve, which spread out radially from their point of entrance in every direction, except where they curve around the central portion of the membrane; and the perceptive layer, which—as viewed from the interior of the eyeball, may be likened to an extremely fine mosaic, each individual piece of which is in communication with a nerve fiber, by which the impressions made upon it are conducted to the brain. The terminals of the perceptive layer are of two kinds, called respectively rods and cones; the former, as the name implies, being cylindrical in shape, and the latter conical. The bases of the cones are directed towards the interior of the eye, so as to receive the light; and it is probable that each cone may be regarded as a collecting apparatus, calculated to gather together the light which it receives, and to concentrate this light upon its deeper and more slender portion, or posterior limb, which is believed to be the portion of the whole structure which is really sensitive to luminous impressions. The distribution of the two elements differs greatly in different animals; and the differences point to corresponding differences in function. The cones are more sensitive than the rods, and minister to a higher acuteness of vision. In the human eye there is a small central region in which the perceptive layer consists of cones

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only, a region which the fibers avoid by curving round it, and in which the other layers of the retina are much thinner than elsewhere, so as to leave a depression, and are stained of a lemon-yellow color. In a zone immediately around this yellow spot each cone is surrounded by a single circle of rods; and as we proceed outwards towards the periphery of the retina, the circle of rods around each cone becomes successively double, triple, quadruple, or even more numerous. The yellow spot receives the image of the object to which the eye is actually directed, while the images of surrounding objects fall upon zones which surround the yellow spot; and the result of this arrangement is that generally speaking, the distinctness of vision diminishes in proportion to the distance of the image of the object from the retinal center. The consequent effect has been well described by saying that what we see resembles a picture, the central part of which is exquisitely finished, while the parts around the center are only roughly sketched in. We are conscious that these outer parts are there; but if we desire to see them accurately, they must be made the objects of direct vision in their turn.

The indistinctness with which we see lateral objects is so completely neutralized by the quick mobility of the eyes, and by the manner in which they range almost unconsciously over the whole field of vision, that it seldom or never forces itself upon the attention. It may be conveniently displayed by means of an instrument called a perimeter, which enables the observer to look steadily at a central spot, while a second spot, or other object, is moved along an arc, in any meridian, from the circumference of the field of view towards the center, or *vice versa*. Slight differences will be found between individuals; but, speaking generally, a capital letter one-third of an inch high, which is legible by direct vision at a distance of 16 feet, and is recognizable as a dark object at 40° or 50° from the fixing point, will not become legible at a distance of 1 foot, until it arrives within about 10°.

The image formed upon the retina is rendered visible by two different conditions,—that is to say, by differences in the amount of light which enters into the formation of its different parts, and by differences in the quality of this light, that is, in its color. The former conditions are fulfilled by an engraving, the latter by a painting. It is with the latter conditions only, and with the power of perceiving them, that we are concerned this evening.

Before such an audience as that which I have the honor to address, it is unnecessary to say more about color than that it depends upon the power possessed by the objects which we describe as colored, to absorb and retain certain portions of white or other mixed light, and to reflect or transmit other portions. The resulting effect of color is the impression produced upon the eye or upon the brain by the waves of light which are left, after the process of selective absorption has been accomplished. Some substances absorb two of the three fundamental colors

of the solar spectrum, others absorb one only, others absorb portions of one or more. Whatever remains is transmitted through the media of the eye, and in the great majority of the human race, suffices to excite the retina to a characteristic kind of activity. Few things are more curious than the multitude of different color sensations which may be produced by the varying combinations of the three simple elements, red, green, and violet; but this is a part of the subject into which it would be impossible for me now to enter, and with which most of those who hear me must already be perfectly familiar.

Apart from the effect of color as one of the chief sources of beauty in the world, it is manifest that the power of distinguishing it adds greatly to the acuteness of vision. Objects which differ from their surroundings by differences of color are far more conspicuous than those which differ only by differences of light and shade. Flowers are much indebted to their brilliant coloring for the visits of the insects by which they are fertilized; and creatures which are the prey of others find their best protection in a resemblance to the colors of their environment. It is probably a universal truth that the organs of color perception are more highly specialized and that the sense of color is more developed in all animals in precise proportion to the general acuteness of vision of each.

From a variety of considerations, into which time will not allow me to enter, it has been concluded that the sense of color is an endowment of the retinal cones, and that the rods are sensitive only to differences in the quantity of the incident light, without regard to its quality. Nocturnal mammals, such as mice, bats, and hedgehogs, have no cones; and cones are less developed in nocturnal birds than in diurnal ones. Certain limitations of the human color sense may almost be inferred from the anatomy of the retina. It is found, as that anatomy would lead us to suppose, that complete color sense exists only in the retinal center, or in and immediately around the yellow spot region, and that it diminishes as we pass away from this center towards the periphery. The precise facts are more difficult to ascertain than might be supposed; for although it is easy to bring colored objects from the circumference to the center of the field of vision on the perimeter, it is by no means easy to be quite sure of the point at which the true color of the advancing object can first be said to be distinctly seen. Much depends, moreover, on the size of this advancing object, because the larger it is the sooner will its image fall upon some of the more sparsely distributed cones of the peripheral portion of the retina. Testing the matter upon myself with colored cards of the size of a man's visiting card I find that I am conscious of red or blue at about 40° from the fixing point, but not of green until it comes within about 30°; while, if I take three spots, respectively of bright red, bright green, and bright blue, each half a centimetre in diameter and separated from its neighbor on either side by an interval of half a centimetre, spots which would be

visible as distinct and separate objects at 8 metres, I can not fairly and distinctly see all three colors until they come within 10° of the center. Beyond 40°, albeit with slight differences between individuals and on different meridians for the same individual, colors are only seen by the degree of their luminosity; that is, they appear as light spots if upon a dark ground and as dark spots if upon a light ground. Speaking generally therefore, it may be said that human vision is only tri-chromatic, or complete for the three fundamental colors of the solar spectrum, over a small central area, which certainly does not cover more than 30° of the field; that it is bi-chromatic, or limited to red and violet, over an annulus outside this central area; and that it is limited to light and shade from thence to the outermost limits of the field.

The nature and imitations of the color sense in man long ago suggested to Thomas Young that the retina might contain three sets of fibers, each set capable of responding to only one of the fundamental colors; or in other words, that there are special nerve fibers for red, special nerve fibers for green, and special nerve fibers for violet. It has also been assumed that the differences between these fibers might essentially consist in the ability of each set to respond only to light vibrations of a certain wave length, much as a tuned string will only respond to a note with which it is in unison. In the human subject, so far as has yet been ascertained, no optical differences between the cones are discoverable; but the analogy of the ear and the facts which have been supplied by comparative anatomy combine to render Young's hypothesis exceedingly probable, and it is generally accepted, at least provisionally, as the only one which furnishes an explanation of the facts. It implies that elements of all three varieties are present in the central portion of the retina; that elements sensitive to green are absent from an annulus around the center; and that the peripheral portions are destitute of any elements by which color sense can be called into activity.

According to the observation already made, that the highest degree of acuteness of vision is necessarily attended by a corresponding acuteness of color sense, we should naturally expect to find such a highly developed color sense in birds, many of which appear, as regards visual power, to surpass all other creatures. I need not dwell upon the often-described acuteness of vision of vultures or upon the vision of fishing birds, but may pass on to remark that the acuteness of their vision appears not only to be unquestionable, but also to be much more widely diffused over the retina than is the case with man. If we watch domestic poultry or pigeons feeding we shall frequently see a bird, when busily picking up food immediately in front of its beak, suddenly make a lateral dart to some grain lying sidewise to its line of sight, which would have been practically invisible to a human eye looking in the same direction as that of the fowl. When we examine the retina the explanation both of the acuteness of vision and of its distribution be-

comes at once apparent. In birds, in some reptiles, and in fishes not only are cones distributed over the retina much more abundantly and more evenly than in man, but the cones are provided with colored globules, droplets of colored oil, at their apices, through which the light entering them must pass before it can excite sensation and which are practically impervious to any color but their own. Each globule is so placed as to intervene between what is regarded as the collecting portion of the cone and what is regarded as its perceptive portion in such a way that the latter can only receive color which is capable of passing through the globule. The retinæ of many birds, especially of the finch, the pigeon, and the domestic fowl, have been carefully examined by Dr. Waelchli, who finds that near the center, green is the predominant color of the cones, while among the green cones, red and orange ones are somewhat sparingly interspersed and are nearly always arranged alternately, a red cone between two orange ones, and *vice versa*. In a surrounding portion, called by Dr. Waelchli the red zone, the red and orange cones are arranged in chains and are larger and more numerous than near the yellow spot. The green ones are of smaller size and fill up the inter-spaces. Near the periphery the cones are scattered, the three colors about equally numerous and of equal size, while a few colorless cones are also seen. Dr. Waelchli examined the optical properties of the colored cones by means of the micro-spectroscope and found, as the colors would lead us to suppose, that they transmitted only the corresponding portions of the spectrum, and it would almost seem, excepting for the few colorless cones at the peripheral part of the retina, that the birds examined must have been unable to see blue, the whole of which would be absorbed by their color globules. It would be necessary to be thoroughly acquainted with their food in order to understand any advantage which the birds in question may derive from the predominance of green, red, and orange globules over others, but it is impossible to consider the structure thus described without coming to the conclusion that the birds in which it exists must have a very acute sense of the colors corresponding to the globules with which they are so abundantly provided and that this color sense, instead of being localized in the center, as in the human eye, must be diffused over a very large portion of the retina. Dr. Waelchli points out that the coloration of the yellow spot in man must, to a certain extent, exclude blue from the central and most sensitive portion of his retina.

It is hardly necessary to mention how completely the high differentiation of the cones in the creatures referred to—tends to support the hypothesis of Young, that a similar differentiation, although not equally manifest, exists also in man. If this be so, we must conclude that the region of the yellow spot contains cones, some of which are capable of being called into activity by red, others by green, and others by violet; that a surrounding annulus contains no cones sensitive to green, but such as are sensitive to red or to violet only; and that, beyond and around

this latter region, such cones as may exist are not sensitive to any color; but, like the rods, only to differences in the amount of light. When cones of only one kind are called into activity the sensation produced is named red, green, or violet, and when all three varieties are stimulated in about an equal degree the sensation produced is called white. In the same way the innumerable intermediate color sensations, of which the normal eye is susceptible, must be ascribed to stimulation of the three varieties of cones in unequal degrees.

The conditions of color-sense which in the human race (or at least in civilized man) exist normally in outer zones of the retina, are found in a few individuals, to exist also in the center. There are persons in whom the region of the yellow spot is absolutely insensitive to color, and recognizes only differences in the amount or quantity of light. To such persons the term "color-blind" ought perhaps in strictness to be limited; but the individuals in question are so rare that they are hardly entitled to a monopoly of an appellation which is conveniently applied also to others. The totally color-blind would see a colored picture as if it were an engraving, or a drawing in black and white, and would perceive differences between its parts only in the degree in which they differed in brightness.

A more common condition is the existence, in the center of the retina, of a kind of vision like that which normally exists in the zone next surrounding it; that is, a blindness to green. Persons who are blind to green appear to see violet and yellow much as these are seen by the normal-sighted, and they can see red, but they can not distinguish it from green. Others, and this form is more common than the preceding, are blind to red, and a very small number of persons are blind to violet. Such blindness to one of the fundamental colors may be either complete or incomplete; that is to say, the power of the color in question to excite its proper sensation may be either absent or feeble. In some cases the defect is so moderate in degree as to be adequately described by the phrase "defective color-sense."

The experiments of Helmholtz upon color led him to supplement the original hypothesis of Young by the supposition that the special nerve elements excited by any one color are also excited in some degree by each of the other two, but that they respond by the sensation appropriate to themselves, and not by that appropriate to the color by which they are thus feebly excited. This, which is often called the Young-Helmholtz hypothesis, assumes that the pure red of the spectrum, while it mainly stimulates the fibers sensitive to red, stimulates in a less degree those which are sensitive to green, and in a still less degree those which are sensitive to violet, the resulting sensation being red. Pure green stimulates strongly the green-perceptive fibers, and stimulates slightly both the red-perceptive and the violet-perceptive—resulting sensation, green. Pure violet stimulates strongly the violet-perceptive fibers, less strongly the green-perceptive, least strongly the red-

perceptive—resulting sensation, violet. When all three sets of fibers are stimulated at once the resulting sensation is white, and when a normal eye is directed to the spectrum the region of greatest luminosity is in the middle of the yellow; because, while here both the green-perceptive and the red-perceptive fibers are stimulated in a high degree, the violet-perceptive are also stimulated in some degree.

According to this view of the case the person who is red-blind, or in whom the red-preceptive fibers are wanting or paralyzed, has only two fundamental colors in the spectrum instead of three. Spectral red nevertheless is not invisible to him, because it feebly excites his green-preceptive fibers, and hence appears as a saturated green of feeble luminosity; saturated, because it scarcely at all excites the violet-preceptive fibers. The brightest part of the spectrum instead of being in the yellow is in the blue-green, because here both sets of sensitive fibers are stimulated. In the case of the green-blind, in whom the fibers preceptive of green are supposed to be wanting or paralyzed, the only stimulation produced by spectral green is that of the red-preceptive and of the violet-perceptive fibers; and where these are equally stimulated we obtain the white of the green-blind, which, to ordinary eyes, is a sort of rose color, a mixture of red and violet. In like manner the white of the red-blind is a mixture of green and violet, and if we consider the facts we shall see that spectral red, which somewhat feebly stimulates the green-perceptive fibers of the normal eye, and spectral green, which somewhat feebly stimulates the red-perceptive fibers of the normal and also of the green-blind eye, must appear to the green-blind to be one and the same color, differing only in luminosity, and that in an opposite sense to the preception of the red-blind. In other words, red and green are undistinguishable from each other as colors alike to the red-blind and to the green-blind; but to the former the red and to the latter the green appears, as compared with the other, to be of feeble luminosity. In either case the two are only lighter and darker shades of the same color. The conditions of violet-blindness are analogous, but the defect itself is very rare; and as it is of small industrial importance it has attracted but a small degree of attention.

Very extensive investigations, conducted during the last few years both in Europe and in America, have shown that those which may be called the common forms of color-blindness, the blindness to red and to green, exist in about 4 per cent. of the male population and in perhaps 1 per thousand of females. Among the rest there are slight differences of color-sense, partly due to differences of habit and training, but of little or no practical importance. One such difference, to which Lord Rayleigh was the first to direct attention, has reference to yellow. The pure yellow of the spectrum may, as is generally known, be precisely matched by a mixture of spectral red with spectral green; but the proportions in which the mixture should be made differ within certain limits for different people. The difference must, I think, depend upon

differences in the pigmentation of the yellow spot rather than upon any defect in the nervous apparatus of the color-sense. There is a very ingenious instrument, invented by Mr. Lovibond and called by him the "tintometer," which allows the color of any object to be accurately matched by combinations of colored glass, and to be expressed in terms of the combination. In using this instrument we not only find slight differences in the combinations required by different people, but also in the combinations required by the two eyes of the same person. Here again, I think the differences must be due either to differences in the pigmentation of the yellow spot, or possibly also to differences in the color of the internal lenses of the several eyes, the lens, as it is well known, being usually somewhat yellow after middle age. The differences are plainly manifest in comparing persons all of whom possess tri-chromatic vision, and are not sufficient in degree to be of any practical importance.

Taking the ordinary case of a red-blind or of a green-blind person, it is interesting to speculate upon the appearance which the world must present to him. Being insensible to one of the fundamental colors of the spectrum, he must lose (roughly speaking) one-third of the luminosity of nature; unless, as is possible, the deficiency is made good to him by increased acuteness of perception to the colors which he sees. Whether he sees white as we see it, or as we see the mixtures of red and violet, or of green and violet, which they make to match with it, we can only conjecture, on account of the inadequacy of language to convey an accurate idea of sensation. We have all heard of the blind man who concluded, from the attempts made to describe scarlet to him, that it was like the sound of a trumpet. If we take a heap of colored wools, and look at them first through a glass of peacock blue, by which the red rays are filtered out, and next through a purple glass, by which a large proportion of the green will be filtered out, we may presume that, under the first condition, the wools will appear much as they would do to the red blind; and under the second, much as they would do to the green blind. It will be observed that the appearances differ in the two conditions, but that in both, red and green are practically undistinguishable from each other, and appear as the same color, but of different luminosity.

Prior to reflection, and still more, prior to experience, we should be apt to conjecture that the existence of color-blindness in any individual could not remain concealed, either from himself or from those around him; but such a conjecture would be directly at variance with the truth. Just as it was reserved for Mariotte, in the reign of Charles II, to discover that there is, in the field of vision of every eye, a lacuna or blind spot, corresponding with the entrance of the optic nerve, so it was reserved for a still later generation to discover the existence of so common a defect as color-blindness. The first recorded case was described to Dr. Priestley by Mr. Huddart, in 1777, and was that of a man

named Harris, a shoemaker at Maryport, Cumberland, who had also a color-blind brother, a mariner. Soon afterwards, the case of Dalton, the chemist, was fully described, and led to the discovery of other examples of a similar kind. The condition was still however looked upon as a very exceptional one; insomuch that the name of "Daltonism" was proposed for it, and is still generally used in France as a synonym for color-blindness. Such use is objectionable, not only because it is undesirable thus to perpetuate the memory of the physical infirmity of an eminent philosopher, but also because Dalton was red-blind, so that the name could only be correctly applied to his particular form of defect.

Color-blindness often escapes detection on account of the use of color names by the color-blind in the same manner as that in which they hear them used by other people. Children learn from the talk of those around them, that it is proper to describe grass as green, and bricks or cherries as red; and they follow this usage, although the difference may appear to them so slight that their interpretation of either color-name may be simply as a lighter or darker shade of the other. When they make mistakes, they are laughed at, and thought careless, or to be merely using color names incorrectly; and a common result is that they rather avoid such names, and shrink from committing themselves to statements about color. Dr. Joy Jefferies gives an interesting description of the almost unconscious devices practiced by the color-blind in this way. He says:

"The color-blind, who are quick-witted enough to discover early that something is wrong with their vision by the smiles of their listeners when they mention this or that object by color, are equally quick-witted in avoiding so doing. They have found that there are names of certain attributes they can not comprehend, and hence must let alone. They learn also what we forget, that so many objects of every-day life always have the same color, as red tiles or bricks, and the color names of these they use with freedom; whilst they often, even unconsciously, are cautious not to name the color of a new object till they have heard it applied, after which it is a mere matter of memory stimulated by a consciousness of defect. I have often recalled to the color-blind their own acts and words, and surprised them by an exposure of the mental jugglery they employed to escape detection, and of which they were almost unaware, so much had it become matter of habit. Another important point is, that as violet blindness is very rare, the vast majority of defective eyes are red or green blind. These persons see violet and yellow as the normal-eyed, and they naturally apply these color names correctly. When therefore they fail in red or green, a casual observer attributes it to simple carelessness,—hence a very ready avoidance of detection. It does not seem possible that any one who sees so much correctly, and whose ideas of color so correspond with our own, can not be equally correct throughout, if they will but take the pains to notice and learn."

When the color-blind are placed in positions which compel them to select colors for themselves and others, or when as sometimes happens, they are not sensitive with regard to their defect, but rather find amusement in the astonishment which it produces among the color-seeing, the results which occasionally follow are apt to be curious. They have often been rendered still more curious, by having been the unconscious work of members of the Society of Friends. Color-blindness is a structural peculiarity, constituting what may be called a variety of the human race; and like other varieties, it is liable to be handed down to posterity. Hence, if the variety occurs in a person belonging to a community which is small by comparison with the nation, and among whose members there is frequent inter-marriage, it has an increased probability of being reproduced; and thus, while many of the best known of the early examples of color bindness, including that of Dalton himself, were furnished by the Society of Friends, the examinations of large numbers of scholars and others, conducted during the last few years have shown that in this country, color blindness is more common among Jews than among the general population. The Jews have no peculiarities of costume; but the spectacle, which has more than once been witnessed, of a venerable Quaker who had clothed himself in bright green or vivid scarlet, could scarcely fail to excite the derision of the unreflecting. Time does not allow me to relate the many errors of the color-blind which have been recorded; but there is an instance of a clerk in a Government office, whose duty it was to check certain entries, in relation to their subject-matter, with ink of one or of another color, and whose accuracy was dependent upon the order in which his ink bottles were ranged in front of him. This order having been accidentally disturbed, great confusion was produced by his mistakes, and it was a long time before these were satisfactorily accounted for. An official of the Prussian post-office, again, who was accustomed to sell stamps of different values and colors, was frequently wrong in his cash, his errors being as often against himself as in his favor, so as to exclude any suspicion of dishonesty. His seeming carelessness was at last explained by the discovery of his color-blindness, and he was relieved of a duty which it was impossible for him to discharge without falling into error.

The color mistakes of former years were however of little moment when compared with those now liable to be committed by engine drivers and mariners. The avoidance of collisions at sea and on railways depends largely on the power promptly to recognize the colors of signals; and the colors most available for signaling purposes are red and green, or precisely those between which the sufferers from the two most common forms of color-blindness are unable with any certainty to discriminate. About 13 years ago there was a serious railway accident in Sweden, and in the investigation subsequent to this accident, there were some remarkable discrepancies in the evidence given with

regard to the color of the signals which had been displayed. Professor Holmgren, of the University of Upsala, had his attention called to this discrepancy, and he found, on further examination, that the witness whose assertions about the signals differed from those of other people was actually color-blind. From this incident arose Professor Holmgren's great interest in the subject, and he did not rest until he had obtained the enactment of a law under which no one can be taken into the employment of a Swedish railway until his color-vision has been tested, and has been found to be sufficient for the duties he will be called upon to perform. The example thus set by Sweden has been followed, more or less, by other countries, and especially, thanks to the untiring labors of Dr. Joy Jeffries, of Boston, by several of the United States; while at the same time much evidence has been collected to show the connection between railway and marine accidents and the defect.

It has been found, by very extensive and carefully conducted examinations of large bodies of men, soldiers, policemen, the workers in great industrial establishments, and so forth, as well as of children in many schools, that color-blindness exists in a noticeable degree, as I have already said, in about 4 per cent. of the male industrial population in civilized countries, and in about one per thousand of females. Among the males of the more highly educated classes, taking Eton boys as an example, the color-blind are only between 2 and 3 per cent., and perhaps nearer to 2 than to 3. Whether a similar difference exists between females of different classes, we have no statistics to establish. The condition of color-blindness is absolutely incurable, absolutely incapable of modification by training or exercise, in the case of the individual; although the comparative immunity of the female sex justifies the suggestion that it may possibly be due to training throughout successive generations, on account of the more habitual occupation of the female eyes about color in relation to costume. However this may be, in the individual, as I have said, the defect is unalterable; and if the difference between red and green is uncertain at 8 years of age, it will be equally uncertain at 80. Hence the existence of color-blindness among those who have to control the movements of ships or of railway trains constitutes the real danger to the public; and it is highly important that the color-blind, in their own interests as well as in those of others, should be excluded from employments the duties of which they are unfit to discharge.

The attempts hitherto made in this country to exclude the color-blind from railway and marine employment have not been by any means successful. As far as the merchant navy is concerned, so-called examinations have been conducted by the board of trade, with results which can only be described as ludicrous. Candidates have been "plucked" in color at one examination, and permitted to pass at a subsequent one; as if correct color-vision were something which could be acquired.

Such candidates were either improperly rejected on the first occasion, or improperly accepted on the second. On English railways there has been no uniformity in the methods of testing; except (in so far as I am acquainted with them) that they have been almost uniformly misleading, calculated to give rise to the imputation of color-blindness where it did not exist, and to leave it undiscovered where it did. In these circumstances it is not surprising that great discontent should have arisen among railway men in relation to the subject; and this discontent has led, indirectly, to the appointment of a committee by the Royal Society, with the sanction of the board of trade, for the purpose of investigating the whole question as completely as may be possible.

It is perhaps worth while, before proceeding to describe the manner in which the color sense of large bodies of men should be tested for industrial purposes, to say something as to the amount of danger which color-blindness produces. A locomotive, as we all know, is under the charge of two men, the driver and the fireman. In a staff of 1,000 of each, allotted to 1,000 locomotives, we should expect, in the absence of any efficient method of examination, to find 40 color-blind drivers and 40 color-blind firemen. The chances would be 1 in 25 that either the driver or the fireman on any particular engine would be color blind; they would be 1 in 625 that both would be color-blind. These figures appear to show a greater risk of accident than we find realized in actual working, and it is manifest that there are compensations to be taken into account. In the first place, the term "color-blind" is itself in some degree misleading; for it must be remembered that the signals to which the color-blind person is said to be "blind" are not invisible to him. To the red-blind, the red light is a less luminous green; to the green-blind, the green light is a less luminous red. The danger arises because the apparent differences are not sufficiently characteristic to lead to certain and prompt identification in all states of illumination and of atmosphere. It must be admitted therefore that a color-blind driver may be at work for a long time without mistakes; and it is probable, knowing, as he must, that the differences between different signal lights appear to him to be only trivial, that he will exercise extreme caution. Then it must be remembered that lights never appear to an engine driver in unexpected places. Before being intrusted with a train he is taken over the line, and is shown the precise position of every light. If a light did not appear where it was due, he would naturally ask his fireman to aid in the lookout. It must be also remembered that to over-run a danger signal does not of necessity imply a collision. A driver may over-run the signal, and after doing so may see a train or other obstruction on the line, and may stop in time to avoid an accident. In such a case he would probably be reported and fined for over-running the signal; and if the same thing occurred again, he would be dismissed for his assumed carelessness, probably with no suspicion of his defect. Color-blind firemen are unquestionably thus driven out of the service

by the complaints of their drivers; and none but railway officials know how many cases of over-running signals, followed by disputes as to what the signals actually were, occur in the course of a year's work. I have never heard of an instance in this country, in which, after a railway accident, the color vision of the driver concerned or of his fireman has been tested by an expert on the part either of the board of trade or of the company, but a fireman in the United States has recently recovered heavy damages from the company for the loss of one of his legs in a collision which was proved to have been occasioned by the color-blindness of the driver. Looking at the whole question, I feel that the danger on railways is a real one, but that it is minimized by the several considerations to which I have referred, and that it is much smaller than the frequency of the defect might lead us to think likely.

At sea, the danger is much more formidable. The lights appear at all sorts of times and places, and there may be only one responsible person on the lookout. Mr. Bickerton, of Liverpool, has lately published accounts of three cases in which the color-blindness of officers of the mercantile marine, all of whom had passed the board of trade examination, was accidentally discovered by the captain being on deck when the officers in question gave wrong orders consequent upon mistaking the light shown by an approaching vessel. The loss of the *Ville du Havre* was almost certainly due to color-blindness; and a very fatal collision in American waters, some years ago, between the *Isaac Bell* and the *Lumberman*, was traced, long after the event, to the color-blindness of a pilot, who had been unjustly accused of being drunk at the time of the occurrence. In how many instances color-blindness has been the unsuspected cause of wrecks and other calamities at sea, it is impossible to do more than conjecture.

It is necessary then, alike in the public interest and in the interest of the color-blind, who have doubtless often suffered in the misfortunes which their defects have produced, to detect them in time to prevent them from entering into the marine and railway services; and the next question is, how this detection should be accomplished. We have to distinguish the color-blind from the color-sighted; but we must be careful not to confound color-blindness with the much more common condition of color-ignorance.

It would surprise many people, more especially many ladies, to discover the extent to which sheer ignorance of color prevails among boys and men of the laboring classes. Many who can see colors perfectly, and who would never be in the least danger of mistaking a railway signal, are quite unable to name colors or to describe them, and they are sometimes unable to perceive for want of education of a faculty which they notwithstanding possess, anything like fine shades of difference. Mr. Gladstone once published a paper on the scanty and uncertain color-nomenclature of the Homeric poems, and he might have found very similar examples among his own contemporaries and in his own

country. I have lately seen a pattern card of colored silks issued by a Lyons manufacturer, which contains samples of two thousand different colors, each with its more or less appropriate name. There is here a larger color vocabulary than the entire vocabulary for the expression of all his knowledge and of all his ideas, which is possessed by an average engine driver or fireman, and just as most of us would be ignorant of the names of the immense majority of the colors displayed on that card, so hundreds of men and boys among the laboring classes, especially in large towns where the opportunities of education by the colors of flowers and insects are very limited, are ignorant of the names of colors which persons of ordinary cultivation mention constantly in their daily talk and expect their children to pick up and to understand unconsciously. It is among people thus ignorant that the officials of the board of trade and of railways have been most successful in finding their supposed color-blind persons, and these persons who would never have been pronounced color-blind by an expert have been able, as soon as they have paid a little attention to the observation and naming of color, to pass an official examination triumphantly. The sense of color presents many analogies to that of hearing. Some people can hear a higher or a lower note than others, the difference depending upon structure, and being incapable of alteration. No one who cannot hear a note of a certain pitch can ever be trained to do so: but within the original auditory limits of each individual the sense of hearing may be greatly improved by cultivation. In like manner a person who is blind to red or green must remain so, but one whose color sense is merely undeveloped by want of cultivation may have its acuteness for fine differences very considerably increased.

In order to test color-vision for railway and marine purposes, the first suggestion which would occur to many people would be to employ as objects the flags and signal lanterns which are used in actual working. I have heard apparently sensible people use, with reference to such a procedure, the phrase upon which Faraday was wont to pour ridicule, and to say that the fitness of the suggested method "stands to reason." To be effectual, such a test must be applied in different states of atmosphere, with colored glasses of various tints, with various degrees of illumination, and with the objects at various distances; so that much time would be required in order to exhaust all the conditions under which railway signals may present themselves. This being done, the examinee must be either right or wrong each time. He has always an even chance of being right; and it would be an insoluble problem to discover how many correct answers were due to accident, or how many incorrect ones might be attributed to nervousness or to confusion of names.

We must remember that what is required is to detect a color-blind person against his will; and to ascertain, not whether he describes a given signal rightly or wrongly on a particular occasion, but whether

he can safely be trusted to distinguish correctly between signals on all occasions. We want, in short, to ascertain the state of his color-vision generally; and hence to infer his fitness or unfitness to discharge the duties of a particular occupation.

For the accomplishment of this object, we do not in the least want to know what the examinee calls colors, but only how he sees them, what colors appear to him to be alike and what appear to be unlike; and the only way of attaining this knowledge with certainty is to cause him to make matches between colored objects, to put those together which appear to him to be essentially the same, and to separate those which appear to him to be essentially different. This principle of testing was first laid down by Seebeck, who required from examinees a complete arrangement of a large number of colored objects; but it has been greatly simplified and improved by Professor Holmgren, who pointed out that such a complete arrangement was superfluous, and that the only thing required was to cause the examinee to make matches to certain test colors, and, for this purpose, to select from a heap which contained not only such matches but also the colors which the color-blind were liable to confuse with them.

After many trials, Holmgren finally selected skeins of Berlin wool as the material best suited for this purpose; and his set of wools comprises about 150 skeins. The advantages of his method over every other are that the wool is very cheap, very portable, and always to be obtained in every conceivable color and shade. The skeins are not lustrous, so that light reflected from the surfaces does not interfere with the accuracy of the observation, and they are very easily picked up and manipulated, much more easily than colored paper or colored glass. The person to be tested is placed before a table in good daylight, the table is covered by a white coth, and the skeins are thrown upon it in a loosely arranged heap. The examiner then selects a skein of pale green, much diluted with white, and throws it down by itself to the left of the heap. The examinee is directed to look at this pattern skein and at the heap, and to pick out from the latter and to place beside the pattern as many skeins as he can find which are of the same color. He is not to be particular about lighter or darker shades, and is not to compare narrowly, or to rummage much amongst the heap, but to select by his eyes, and to use his hands chiefly to change the position of the selected material.

In such circumstances a person with normal color sight will select the greens rapidly and without hesitation, will select nothing else, and will select with a certain readiness and confidence easily recognized by an experienced examiner, and which may even be carried to the extent of neglecting the minute accuracy which a person who distrusts his own color sight will frequently endeavor to display. Some normal sighted people will complete their selections by taking greens which incline to yellow, and greens which incline to blue, while others will

reject both; but this is a difference depending sometimes upon imperfect color education, sometimes upon the interpretation placed upon the directions of the examiner, but the person who so selects sees the green element in the yellow greens and in the blue greens, and is not color-blind. The completely color-blind, whether to red or to green, will proceed with almost as much speed and confidence as the color sighted; and will rapidly pick out a number of drabs, fawns, stone colors, pinks, or yellows. Between the foregoing classes we meet with a few people who declare the imperfection of their color sense by the extreme care with which they select, by their slowness, by their hesitation, and by their desire to compare this or that skein with the pattern more narrowly than the conditions of the trial permit. They may or may not ultimately add one or two more of the confusion colors to the green, but they have a manifest tendency to do so, and a general uncertainty in their choice. One of the great advantages of Holmgren's method over every other is the way in which the examiner is able to judge, not only by the final choice of matches, but also by the manner in which the choice is made, by the action of the hands, and by the gestures and general deportment of the examinee.

When confusion colors have been selected, or when an unnatural slowness and hesitation have been shown in selecting, the examinee must be regarded as either completely or incompletely color-blind. In order to determine which, and also to which color he is defective, he is subjected to the second test. For this, the wool is mixed again, and the pattern this time is a skein of light purple—that is, of a mixture of red and violet, much diluted with white. To match this, the color-blind always selects deeper colors. If he puts only deeper purples, he is incompletely color-blind. If he takes blue or violet, either with or without purple, he is completely red blind. If he takes green or gray, or one alone, with or without purple, he is completely green blind. If he takes red or orange, with or without purple, he is violet blind. If there be any doubt, the examinee may be subjected to a third test, which is not necessary for the satisfaction of an expert, but which sometimes strengthens the proof in the eyes of a bystander. The pattern for this third test is a skein of bright red, to be used in the same way as the green and the purple. The red blind selects for this dark greens and browns, which are much darker than the pattern; while the green blind selects greens and browns which are lighter than the pattern.

The method of examination thus described is, I believe, absolutely trust-worthy. It requires no apparatus beyond the bundle of skeins of wool, no arrangements beyond a room with a good window, and a table with a white cloth. In examining large numbers of men, they may be admitted into the room fifty or so at a time, may all receive their instructions together, and may then make their selections one by one, all not yet examined watching the actions of those who come up in their turn, and thus learning how to proceed. The time required for large numbers

averages about a minute a person. I have heard and read of instances of color-blind people who had passed the wool test satisfactorily, and had afterwards been detected by other methods, but I confess that I do not believe in them. I do not believe that in such cases the wool test was applied properly, or in accordance with Holmgren's very precise instructions; and I know that it is often applied in a way which can lead to nothing but erroneous results. Railway foremen, for example, receive out of a store a small collection of colored wools selected on no principle, and they use it by pulling out a single thread, and by asking the examinee, "What color do you call that?" Men of greater scientific pretensions than railway foremen have not always selected their pattern colors accurately, and have allowed those whom they examined and passed to make narrow comparisons between the skeins in all sorts of lights in a way which should of itself have afforded sufficient evidence of defect.

Although however the expert may be fully satisfied by the wool test that the examinee is not capable of distinguishing with certainty between red and green flags or lights in all the circumstances in which they can be displayed, it may still remain for him to satisfy the employer who is not an expert, the railway manager, or the shipowner, and to convince him that the color-blind person is unfit for certain kinds of employment. It may be equally necessary to convince other workmen that the examinee has been fairly and rightly dealt with. Both these objects may be easily attained by the use of slight modifications of the lights which are employed. Lanterns for this special purpose were contrived some years ago by Holmgren himself and by the late Professor Donders, of Utrecht, and what are substantially their contrivances have been brought forward within the last few months as novelties by gentlemen in this country who have re-invented them. The principle of all is the same, namely, that light of varying intensity may be displayed through apertures of varying magnitude and through colored glass of varying tints, so as to imitate the appearances of signal lamps at different distances and under different conditions of illumination, of weather, and of atmosphere. To the color-blind the difference between a red light and a green one is not a difference of color, but of luminosity, the color to which he is blind appearing the less luminous of the two. He may therefore be correct in his guess as to which of the two is exhibited on any given occasion, and he is by no means certain to mistake one for the other when they are exhibited in immediate succession. His liability to error is chiefly conspicuous when he sees one light only and when the conditions which govern its luminosity depart in any degree from those to which he is most accustomed. With the lanterns of which I have spoken it is always possible to deceive a color-blind person by altering the luminosity of a light without altering its color. This may be done by diminishing the light behind the glass, by ncreasing the thickness of the red or green glass, or by placing a piece

of neutral tint, more or less dark, in front of either. The most incredulous employer may be convinced by expedients of this kind that the color-blind are not to be relied upon for the safe control of ships or of locomotives. With regard to the whole question there are many points of great interest, both physical and physiological, which are still more or less uncertain, but the practical elements have, I think, been well-nigh exhausted, and the means of securing safety are fully in the hands of those who choose to master and to employ them. The lanterns in their various forms are useful for the purpose of thoroughly exposing the color-blind and for bringing home the character of their incapacity to unskilled spectators; but they are both cumbrous and superfluous for the detection of the defect, which may be accomplished with far greater ease and with equal certainty by the wool test alone.

I have already mentioned that the examinations which have been conducted in the United States, thanks to the indefatigable labors of Dr. Joy Jeffries, have led to the discovery of an enormous and previously quite unsuspected amount of color ignorance, the condition which is frequently mistaken for color-blindness by the methods of examination which are in favor with railway companies and with the board of trade; and this color ignorance has been justly regarded as a blot on the American system of national education. It has therefore, in some of the States, led to the adoption of systematic color-teaching in the schools; and for this purpose Dr. Joy Jeffries has introduced a wall chart and colored cards. The children are taught, in the first instance, to match the colors in the chart with those of the cards distributed to them, and when they are tolerably expert at matching they are further taught the names of the colors. It must nevertheless always be remembered that a knowledge of names does not necessarily imply a knowledge of the things designated, and that color vision stands in no definite relation to color nomenclature. Even this system of teaching may leave a color-blind pupil undetected.

TECHNOLOGY AND CIVILIZATION.*

By F. REULEAUX.

From the present status of the world's culture, one can not fail to discern the significant influence of our scientific technology in qualifying us for greater achievements than the past centuries have yet witnessed, whether in connection with rapid transit by land or sea, tunneling mountains, piercing the air, making the lightning our message-bearer from pole to pole or sending our voices across the land; or whether, indeed, from another point of view we bring into our service the mighty mechanical powers, or adapt and make use of those intangible contrivances usually unnoticed by the world at large.

Everywhere in modern life, about us, in us, with us, beside us, is felt the influence of scientific art acting as an agent and as companion, whose ceaseless service we never realize until for a moment it fails us.

Commonplace though this be, still it seems to me that in the cultured world and perhaps in the narrower circles of scientific men, this truth is too slightly valued. The value of scientific technology in its true character as producer and promoter of civilization, is too little recognized.

This may result from a confusion of the so-called technical with the unscientific; or on the other hand, from concealment of its results under a preponderating mass of idealism, its development being cramped by ambition for gain and trammeled by social evils, which go hand in hand with industrial labor. But I will not here consider this side of the question. I would attempt a nearer approach to the inner sanctuary of technology to certain weighty questions, which appear especially deserving of present notice, as:

What place, particularly in associate working, the technology of our day takes in civilization? A place not so well defined, it appears to me, as is that we assign to less important social, political, and scientific events.

Again, a question occurs as to the chief features of the method followed by technology to attain its ends, and concerning the plan which

* Translated from *Prometheus* (Berlin), 1890, vol. I, pp. 625, 641, and 666.

must more or less underlie device and invention; a question which (especially for patent legislation) has long employed and must long continue to employ the scientist as well as the administrative practitioner.

If we will compare our civilization with that of other nations we must understandingly glance at the people and their pursuits, which we find upon the lowest stratum; for example, those who, lacking a knowledge of writing, that wondrous thought transmitter, have, of course, no care for science. In this comparison one will soon encounter peoples whom a high culture has for centuries, yes, thousands of centuries, been a part. These are the peoples of eastern and southern Asia, the Chinese, Japanese, people of India, the Persians, and Arabians. Noting without prejudice their culture, we must concede them to be in a state of high development, indeed to have been highly developed, when middle Europe still remained deep in barbarism. Even then science and art flourished among them, and is still advancing.

For 3,000 years the Indian Vedas have devoutly proclaimed the Deity; 2,000 years ago the Indian poets produced their odyssey the "Mahabharata"—the great Bharata, the forerunner of many dramas, among them the tender "Sakuntala," the charm of which is still potent since its sentiments found their origin in the heart of man. Philosophy flourished likewise, and the science of language in so great degree that the Indian grammarians of to-day can look back upon an unbroken line of predecessors, the vista terminating in Panini, whom they reverence like a god. Mathematics, too, were fostered, and to-day we write our numbers in Indian characters. In parts of India and in eastern Asia the commercial arts progressed then as now. Persia, too, was laurel-crowned among the world's poets. Following the great Firdousi came the "Horaz" of Schiras, and in his footsteps Hafis sung his immortal songs, all of which have become a part of our literary treasure through the sesame of translation. And the Arabian literature, to which we have not yet had access in its entirety, how has it laid under tribute the Grecian inheritance, and so perfected astronomy that at the present time we name half the heavens after them. How, under the patient and studious princes of the time of Charles, did they foster the growth of arithmetical and still deeper science! How too have they surpassed our knowledge of chemistry in various substances and essences!

What is then the spiritual difference which sunders their path from ours? Are we in certain arts still behind them? They are brave soldiers, gentle and industrious citizens, wise statesmen and scholars; honor and justice hold high rank among them. Where then, considered as men, lie the points of difference?

Or, on the other hand, do we question whether the spiritual boundaries lead to the good, and would we fain know whence springs our superiority over them?

How is it possible, for example, that England with a few thousand of

her own troops, rules the two hundred millions of India; how was it possible for her to remain victor in opposition to their terrific and fanatical revolt in 1857? How does it happen that we, Europeans or (not particularly to mention the European-settled America,) that the Atlantic nations alone compass the earth with railroads, surround it with telegraph lines, traverse its water girdle with mighty ships, and that to all this the other five-sixths of the earth's inhabitants have not added a span—the same five-sixths which still, for the greater part, are grandly organized and highly cultivated?

There are different ways of explaining this astonishing fact, or rather, of at least attempting to determine it comprehensively. Klemm, the industrious Leipsic collector, who was a pre-historian long before the discovery of pile habitations, has propounded the distinction between "active" and "passive" peoples; and many to-day follow him therein. To him the Atlantic nations are the active; all others, down to the utterly uncultivated, the passive. According to this theory we make history, they suffer it. Although this discrimination appears to have so much in its favor it does not hold. Nations can (as history teaches) be a long time active, then passive, and later again active. Activity and passivity are not to nations indwelling characteristics, but circumstances into which and out of which they can fall without changing their spiritual, essential position. One proof of reality the Klemm theory does not stand. Europe could, to-morrow, unyoked from Asia, be made passive without losing the character which makes railroads, steamships and telegraphs belong to her as her spiritual possession. The Arabian, on the contrary, could destroy the products of scientific technology as the pretended Omar the books, but would not be able to re-produce them, as has many times been done in case of the books.

Others have supposed, and still believe, that it is Christianity that establishes the distinction.

This however does not stand the test. Of course a considerable part ot the thinking which resulted in metamorphosed inventions and discoveries was done in the Christian empire, but by no means all. What an innovation was made by the art of printing, and yet we know that 1,000 years earlier the Chinese had found a way to this art. Gunpowder, too, that marks so decisive a step in the progress of our civilization, was used by the Arabians long before the time of the Freiburg monks. Then in mechanics we find those important power machines, the water wheels, are very old and of Asiatic origin.

But passing from these examples to a genuine offspring of Europe, the steam-engine, watching its gradual development up to its actual use—the time of the Renaissance—in Italy, Germany, France, and England, but never outside of Christendom, even this, we find, does not encounter progress, but on the contrary, its adherents often oppose it up to the last.

We look further and do we not find to-day Christians living in the

East, for example, in Armenia and in Abyssinia, entirely outside the contemplation of our victorious modern technology? In the past they have added naught thereto and to-day they are not its contributors.

It can not be the things themselves, the inventions, but the engendering thought which must have produced the change, the innovation. In fact we can but ascribe this to a peculiar progress in thought precedence, a difficult, dangerous ascent to a higher, freer comprehension of nature.

The spell which bound us was broken by our understanding when we found the forces of nature following in their operations no capricious will—a Godly will—but working according to steadfast, unchangeable laws—the laws of nature; never otherwise.

According to laws mighty, fixed, eternal,
Must we complete our being's circle

breathe Goethe's words from out the terrors of nature's inexorable power. But according to "laws mighty, fixed, eternal" roll the worlds, the stars pursue their course, a tile falls from the roof or a drop from its cloud height.

Suns wander up and down,
Worlds go and come again,
And this no wish can alter.

In this grand poetical form is seized the same uplifting knowledge that not the bodily but the spiritual force incloses within itself the presentiment of God, that even the world's creation consists in the immutability of its laws. That it might win the knowledge, thought broke through the old barriers, but immediately drew from real life conclusions such as these, if we may utter them quite free from secondary considerations.

If we bring lifeless bodies into such circumstances that their working of natural laws answers our purposes, we may permit them instead of this labor to work for living beings.

This began to be carried out with intelligence, and thereby was created our present technology. Scientific technology I must name it. When the spirit entertained the idea which sought to make natural laws a conscious power, scarcely anything was known of these laws and they must first be wooed. Through hard battle indeed must they be won, for the learned world believed itself to have them in its possession. The reformer had therefore not simply to make the discovery, but to accomplish the gigantic task of overcoming antagonistic convictions and at the same time to support a spiritual campaign up to the heights of freer knowledge, for this march found weighty opposition in the decrees of the church, which had demanded its sacrifice. The victory was won, and therewith our present technology gained the command. The opposing current of the time had spent itself, comprehending, perhaps, its injustice, for do not its first representatives travel as gaily upon the railroad, telephone, and telegraph as do others? Only small

skirmishers exist as a reserve, and this more from stubbornness than conviction. At all events they do not in the least retard the chief movement.

What had happened had the reaction of that time prevailed—for it was a reaction begun in Germany more than 100 years before, Copernicus having lain more than 90 years in the grave when Galileo was unwillingly compelled to witness against him—what had happened in such an event is difficult to conceive; and yet not so, for we may see it exemplified in the great Arabian nation. Among this people the reaction had, in truth, conquered. Their Galileos, their Averrhoës, and numberless others, were defeated, together with their free convictions; with them their entire sect, and therewith the Arabian culture, which already had lifted the hand to grasp the palm of victory of free knowledge, was paralyzed by the fanatical victors, and paralyzed they still lie low, already half a thousand years. Allah aalam! "God alone knows," therefore shalt thou not desire to know! So sounds it since then for the pure Mohammedan; all investigation is cut off from him, forbidden and declared sinful. A noble and refined disciple of the Prophet has given expression to the hope that the Moslem may yet be called to take up the lost leadership. Who may believe him? However, it appears certain that the overthrow of free thought in the Arabian language has become decisive for the remaining Asiatic culture. Like a dam lies the spiritual-slain mass between them and us, and so has it come that we alone have entered into the development to which the pictured progress of thought led the way. The powers of nature which she has taught us to make useful are the mechanical, physical, and chemical; to permit them to work for us requires a great outfit of mathematical and natural science. From this entire equipment we exercise a portion as a privilege.

It seems necessary, in order to briefly distinguish the two directions of development, to call them by particular names. The Greeks named an artistic mechanism, an arrangement through which the unusual could be conducted, a *manganon*, which word goes back, according to some, to the name of the eminent race of magicians. All kinds of definite tangible things which were considered skillfully and wisely thought out were so titled; among others, a catapult for projectiles for purposes of war. With this the word comes into the Middle Ages. Then, early in the seventeenth century, a great machine was invented for rolling and smoothing the washing, and since this contrivance bore a remarkable outward resemblance to the catapult, it was also given its name, whereupon the word wandered further into the remaining European tongues, as every house-wife knows, or perhaps does not know, if she send her washing to a "mangle."

Again, for our purpose, I would generalize that old word and name, on the one hand, that something by means of which the forces of nature are known in her laws, *manganism*, and on the other, that which

seems to stand as nature's defender, mysteriously guarding her ways—*naturism.*

Employing for the present these terms, we shall see the peoples of the earth divided into *manganistical* and *naturistical*, and shall notice that, on account of their full understanding of their material equipment, the former have a powerful advantage over the latter. Indeed, we dare go much further and hesitate not to assert that to the manganistical nations belongs the domination of the earth, although now, as ever, it must be battled for. Still the observer may confidently predict the victory of the manganists and that resistance can but mean either gradual overthrow or destruction.

That unyielding determination makes possible the unprecedented step from naturism to manganism is shown in our time, a time so rich in culture, by the example of Japan.

The chief men of this nation, having recognized the necessity, have also gained the political power for the purpose, and so transpires before our eyes the intelligent effort, towards which all their strength is directed, of systematically changing their scheme of instruction. Difficult as is the attempt its beginning promises success, consisting as it does in nothing else than *learning*, *learning*, *learning.*

Very gently in India the English have commenced to work towards manganistical education, and although all is yet in the beginning, great results are possible.

It is unnecessary however to stray into distant lands to find naturism; in Europe it is at hand, and indeed in every human being lurks a portion of naturism. The first touch with manganism must be through education, the surrender of the uncultured mass of intellect to kind nature, but subject to a firm control which shall so hold her in check as to prevent the ruin which would otherwise threaten in the full contact with fate.

In Spain manganism has developed but slightly. The Iberian Peninsula has not contributed to the great metamorphosing inventions; naturally the repression of thought advancement would occur more readily there, as at that period the new-discovered world held attention. The loss to Spain is, however, incalculable.

Greece, once leading the world in arts and sciences, was at the time of the blossoming of scientific technology, so entangled in the result of her fall that the movement did not seize her. Now as a nation she seeks to raise herself out of naturism in order to resume the transmission of the old spiritual activity, and we may watch with interest the experiment made upon the classical soil of this beautiful land. Without manganism the effort must fail.

Italy furnishes us with a striking illustration. For a long time devoted thoroughly to naturism, and also desiring her share in the great scientific discoveries of the Renaissance, this highly-gifted people more or less neglected manganism, but preserved her flowers of art, and has

therein sought and found her glory; this neglect her new form of government has caused her to recognize, as well as the necessity for its avoidance; consequently we see the Italians exerting themselves with astonishing energy to spread among themselves manganistical industries and qualities. That their rapid and significant progress in useful industries weakens their achievements in art industry can not be doubted.

Like a shadow this fact flits over us, until it seems as if between the two directions must exist an opposition to which one will fall a sacrifice. But not so; art and scientific technology are not at variance; it only requires great effort for both to be developed; great firmness and spiritual insight into æsthetical laws to counterbalance the disturbing grasp of the machine.

That both may develop side by side is shown by the present movement in Austria and Germany.

Turning now to the consideration of the inner method of manganism, I pass over an entire line of preparatory grades, but desire to note that which is common to different actions, but which seems to the outside world contradictory. Such generalizing shortens, but is necessary in order to make clearer the influx of new appearances in the technical kingdom. For the purpose of making these certain, efficient and intelligent, it may be permitted to employ a few simple examples:

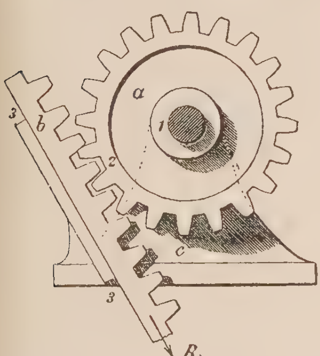

Fig. 1.

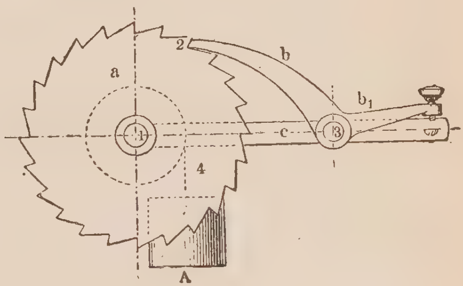

Fig. 2.

The cog-wheel *a*, Fig. 1, catching in the usual manner in the cogs of the bar at 2, is rotated at 1 in the stationary frame *c*, in which also at 3, the cog-bar *b* slides, this bar, a very long one, being pulled down by a weight *B*.

Imagining the wheel *a* so turned as to raise the weight *B*, or in such manner as to lower it, we have before us an efficient machine of a definite kind, viz, one of continuous direction of motion whether forward or backward. We will call it, because of this continuous motion, a *running* work (Laufwerke). As is well known, there are many running works; among them friction wheels, cog-wheels, beltings, turbines,

etc., in many different combinations. Opposed to this mechanism is another of a different motion; of this Fig. 2 furnishes an example. The wheel *a* turns 1, in a fixed frame and has saw or similar shaped teeth in which, at 2, a ratchet catches. This ratchet hinders the wheel from following the pulling of the weight A at the margin of the wheel *a*. But if the wheel be turned as we wind a cord, 4, on which hangs the weight, the ratchet permits the wheel to go forward but retards it again as soon as the compelling force subsides.

This arrangement is known as "*obstruction*" (Gesperre.)

In the use just described we would call it *obstructing work* (Sperrwerk): its backward and forward motion varying, thus requiring it to be completely discriminated from *running work* (Laufwerke).

From the given groups of mechanisms, five others are possible.

If we next imagine the ratchet to be raised, through pressure upon the button at 5, the obstruction being released, the weight A falls down, taking or drawing with it the wheel *a*. The resulting motion can be utilized in many ways: quickly, as through a push with a ram, slowly, gradually, as by a clock; also in the running work of the telegraph, changing always according to supply.

Through winding on spokes, the mechanical labor can always be usefully changed. Instead of lifting a weight A, one can also place an elastic body, *i. e.*, a spring in a condition of tension. We will therefore name the produced mechanism *tension work* (Spannwerk). The crossbow was a spring tension work; there are millions of spring tension works in practical use in flint-locks.

We procure a third mechanism through a slight change of the management, namely, by allowing the ratchet that was previously released to be again caught. This then catches up the wheel *a* and with it the fallen weight A. A sufficiently strong structure pre-supposed, one can also make the mechanism serve for catching up heavy masses, and we name it accordingly *catch work* (Fangwerke). The mechanisms used in mines and also in elevators for the catching of the propellers in case of rope-break, are such *catch work*. If one considers that the wheel teeth can be made so fine as to be invisible, whereby the circumference of the wheel *a* will be smooth and the obstructing ratchet simply a friction body, the obstruction changes into a friction obstruction, as one perceives in the brake of the railroad train. The applications of *catch work* are also very useful and numerous.

A fourth mechanism one would secure out of the groups in question, if one attached, but on a moving arm, perhaps a second similar ratchet to the nearer one, fastened to it, the last having a swinging motion. Through this motion one can then, intermittingly, move the wheel with the intention of lifting the weight, since the first ratchet always catches the wheel when it begins to let the weight sink. The thus formed and driven mechanism is called *leap work* (Schaltwerk). Applications of the same are known and many. A fifth manner of conversion of the

groups results, if one uses perhaps a narrow, corner-shaped segment of a wheel and forms with it an obstruction for the passage between the points 1 and 2, in the fashion, I will say, of a door. Then through closing the obstruction at 2, the passage can be retarded or stopped; through loosening, it will be opened. We will name the mechanism in this form, *closing work* (Schliesswerk). It exists in closing doors, windows, closets, chests, in the form of locks, and so on in known and numerous changes. We see here the wide domains of the lock, which offers millions, yes millionfold variations of closing work.

The sixth, and perhaps from the standpoint of the mechanic the most remarkable change of the obstruction, is the checking or *check work* (Hemmwerk), as we will say. It exists if we set free the obstruction by light touches upon the button at 5 and immediately closing it again. If this occur regularly the progress of the wheel *a* may serve, among other purposes, for measuring time. In clocks this *check work* is largely used. The regular release of the obstruction takes place by means of an even-timed body, the pendulum. Variations of check work exist in many other machines.

Thus we see there are many examples of *obstruction works* (Gesperrwerken), as we may call them collectively, *i. e.*, works in which the obstructing ratchet plays a part. But let us look still farther. It often occurs that obstruction works are combined and the action of one transmitted to the other. A fine example is furnished by the set-trigger of target rifles. This trigger is nothing else than a little tension work, very easily loosened, in consequence of which the firmer held tension work of the cock is loosened, one thus working upon the other. Such a combination we may call a tension work of a higher order, or, in case of a similar combining of obstruction works we speak of an obstruction work of a higher order. An illustration is furnished by the motive work of a clock, where the weight and spring tension work (Gewichts-und Fedderspannwerk) drives the obstruction work (das Hemmwerk), thus working in the second order. Clearly, we have here a principle, for the transmission of motion can occur between obstruction work and wheel work, and so on. For example, there is attached to the check work of the clock a cog-wheel work which moves the hands. Naming motive works in general, several examples of which we have noticed *drive works* (Treibwerken), the wheel work of a clock must be a drive work of the third order, consisting of tension work, check work, and wheel-running work arranged the one over the other.

Having taken so broad a view in this field of observation, we turn to another quite different in aspect.

If we notice our machines in practical use we find among them a number in which fluidity serves as force and motion transmitter, as the hydraulic press, the pump, spouting machines, water wheels, the turbines, etc. But not only liquid but gaseous fluids we similarly convert into gas motors, air machines, and especially into steam-engines. Close

observation shows that we have subjected all these cases, in consequence of the suitable inclosing of the liquids in channels, pipes, and vessels, to such a forced way of motion—I at one time proposed to name it "forced-running"—that they are able to work in mechanisms as do firm bodies, but have this advancage of conforming themselves always to their surroundings.

If we introduce something of this kind in our running work (Fig. 1), replacing the cog bar by a stream of water, then our running work becomes a water wheel, mediocre indeed, if the water is taken as the driving force. It becomes a dipping wheel or spray wheel when the wheel *a* is propelling and the water *b* is the propelled body.

The practice in machines leads to the same thought concerning obstruction work. The obstructing ratchets are named valves when either the wheel *a* or its substitute—a section of the wheel, cog bar, etc.—have been converted to liquids. The valves are in reality in every way, try them or examine them as we will, the obstructing ratchets of the liquid. One observes immediately what a new, great, yes, even grand, enlargement has been gained by the putting into use of these drive works. Examples surround us, I should say crowd around us. Our common water-pump, with the butt of the valves and the sucking valve, is a water leap work prepared exactly in accordance with the scheme mentioned before, viz, of that leap work found in Fig. 2. Also in check work we find fluids, liquids, and gates taking the place of an ascending wheel or its substitute, as in water throwing machines and not less in steam-engines.

In fact, regarding these machines as drive works, they correspond to clocks which I have taken as illustrations of obstruction works, the difference being solely that in clocks a harmful resistance, in the other machines a useful resistance, is overcome. Had I more time I would prove their similarity in all points.

The valves, for instance, often single, but sometimes a combination of two or more in one machine, correspond to the so-called anchor of the clock check work, to the eccentric (muschelschieber) of the steam engine, the pendulum of the clock being represented by the vibrating butt, etc.

Thus the great and powerful steam-engine legitimately and with perfect ease falls in the line, taking there its rightful place. And so must it be with scientific perception which will have to do with true, logical connection only (not with sensational), performing wonders. But in dealing with this principle we must gain one more ascent in order to attain the full theoretical horizon. Let us not regret the trifling exertion which must bring abundant reward.

Noting, from the common standpoint, the source of power in our steam-engine, we find within the collected mass of stored-up steam an active, communicating atom force, which is an expansive power or tension work. The boiler, too, with its valves and contrivance for letting

off steam, is but a tension work, differing from that previously noticed in that it lodges in a physical manner the called forth tension, making it, in truth, a *physical tension work*. This observation carries us further, draws us on, as it were, to the casual connection by which heat is communicated to the boiler water. This connecting link is the fire, the glowing, flaming coal which gave up chemically, in combustion, the energy stored therein. Thus fire is a *chemical tension work* made active through kindling, but holding latent, if we consider it in the form of coal, a heat energy stored within by nature's slow process during millions of years and now eagerly yielded to our simple expedient.

Thus we have our steam-engine complete; in the boiler fire a liberated chemical tension work; in the boiler itself a physical tension work made active by the fire; in the engine proper, consisting of stop-cocks, cylinder, and piston-work, a mechanical check work, with motive power previously supplied; consequently, as a whole, a general drive work of the third order whereby we slight all secondary mechanisms of permitted masses.

But if instead of the simple steam engine with its alternate motion we consider a crank-engine, we have attached to the check work, in the form of the crank-motor, a running work, which we can and do use, in thousands of forms; but the machine thus becomes in this, its most-used variety, a general drive-work of the fourth order.

Permit me to call attention to still another example taken from steam industry upon the railroad.

In the locomotive just developed we have before us a drive work of the fourth order. Next come the drive wheels of the engine as running work, friction-wheel work (Reibräder werk), and joining this locomotive the train gliding over the rails, a self-moving second running work, making, as a whole, a drive work of the sixth order.

But let our train be of modern form and it will have a Westinghouse brake. The reason of the great favor in which this brake is held and of its great importance our theory explains as follows:

The brake itself is a catch work formed from a friction obstruction work which we formerly set in motion with the hand.

Now we manage otherwise. We have with Westinghouse in the form of the air battery on the train, indeed on every car, a strong, readily-placed tension work which we can at all times easily release through a stop cock in the form of an obstructing ratchet, which the brake contracts. Beginning from above, if we follow the brake apparatus, we have before us: The little steam-engine, a check work; the air pressure pump, a leap work; the mentioned crank mechanism, a check work; and the side brake itself, a catch work; together a drive work and indeed a mechanism of the fifth order; and if we add thereto, as we must, steam-boiler and fire, the whole results as a general drive work of the seventh order. Higher numbers of orders certainly do not belong to usual contrivances.

We may now turn, without anxiety lest we sacrifice clearness, to the side of the most modern of all technical novelties, the electro-mechanical. Here we recognize in the Galvanic battery, or chain, a *chemical runningwork*, which expression can well be conceded, as it depends upon motion excitement, although it be atomical; the induced physical-electrical stream, the valves of which are the obstruction ratchet, the contact, polishing springs, etc., is used in various arts; in telegraphy it works in leap work of the second order, provides by relay for release and making fast again, and a mechanical running arrangement of writing work; it results, according to circumstances, from the third to the fourth order.

The usual sound-contrivances of the railroad work in the fifth order, chemically in current producers, physically leaping in the anchor pulling through which a mechanical tension work, that is one bent by the hand, is released; the same drives a check work which again the little hammer tension work (Hammerspannwerke) springs, makes taut, and then releases.

Among chemical drive work, we notice that the tension works take a prominent place. Those placed here will be of the number so artistically prepared by chemists that they give up their tension, or expansive force, slowly or rapidly. Gunpowder is the most powerful tension work, which the naturistical groping Middle Age set in the place of the mechanical tension work stretched by the hand of man out of netting, bows, and sinews in large and small throwing machines. The purpose remaining exactly the same, the kind of tension work was changed. The fuse releasing the new tension work was in itself a slow running, chemical tension work, entirely separated from the larger. Later we got so far as to take the two together in a single contrivance, at first in flint-locks, then in percussion locks. There one entered the third order. The percussion cap, a chemical tension work rather easily liberated, is set free by a mechanical tension work attached to the guncock. The ball is thrown by a tension work of the third order, as occurs in the set-trigger in the fourth order.

Allow me to say a word concerning a petty example, the match. Not two generations have we possessed it, and previous to this brief period we manganists, in point of fire kindling, were very nearly on a par with the lowest naturists.

In a natural state, as we know, people, through laboriously acquired skill, kindle a fire by rubbing together two pieces of wood; in other words, they set free that tension work, heat. The old Greeks used for the purpose the *pyreion*, the under piece of which, called the eschara, contained a bore, in which the rubbing piece, the trypanon or borer, was inserted and then turned by twisting the hands.

Ought not in some hidden corner of the Grecian mountains the pyreion still to be found? It would be very serviceable to bring it to light.

The little fire-chests containing flint, steel, tinder, and threads dipped

in sulphur, which in my earliest childhood I saw used in my home, are examples which have kept their places in spite of the all-conquering match; it would be well to have specimens of these preserved in ethnographical museums.

Later came steel and flint, a physical tension work used for itself. With their help one kindled—and many still do it to-day—the tinder, an easily freed tension work, especially prepared for the purpose and consisting at that time of burnt linen.

On the tinder as soon as it glimmered, was set free a chemical tension work rather difficult to release, the thread dipped in sulphur, and finally with this, a thin piece of wood, but not for a time a coal. For the kindling of the wood alone one used, in succession, four distinct tension works, one physical, stone and steel, and three chemical, tinder, sulphur, and wood.

We now see the match fully in the domain of the former developed principle. The little important fire tool was made by combining three, but soon after four tension works, and is a chemical tension work of the fourth order, formed from the tension works phosphorous, chloracid kali, sulphur, and wood. For the sulphur, as is known, was later substituted in many ways wax or paraffine. But the principle is very plainly recognized; each one of the tension works following one upon another, is more difficult to set free than its predecessor, but was freed definitely, and then through a very easy mechanical action upon the little tension work most highly sensitive, the hair-trigger, brought about the deliverance, as it were, of each of the four obstructions which had caused such trouble, demanding the entire force of one and frequently of two men. That the combination of the four tension works was so recently attained proves that the fundamental principle of the train of thought must have been quite difficult.

We have now, at last, the manganistical principle fully before us, in a common form as well as in the greatest, the examples embracing the most powerful forces, down to the finest and smallest, and we can declare that the method consists: *In the cultivation depending upon a scientific knowledge of the laws of nature, and the resulting higher orders, and those standing side by side, of mechanical, physical, and chemical drive work.*

If the foregoing is developed essentially with a consideration of mechanical technical aims, it permits itself to turn without any compulsion upon the precedency of chemical technology and may, therefore, be found to embrace in itself the entire problem. One has only, for example, to think of a chemical manufactory, etc., and how sulphuric acid enters as a physical and mechanical medium in the colors. As in the above both of the others are side by side with the mechanical.

From the standpoint now gained, if we again consider scientific technology, we shall see how its results are closely bound with our life habits, indeed, with our entire culture. We may overlook the fact that

we are directly surrounded in our dwellings by thousands of obstruction works which have made our rooms safe, comfortable, and convenient for light, air, and warmth. We may overlook this because naturistical labor is able to produce similar, although less perfect, results. But let us notice other things whereby our dwellings have received their character. There is the gas-light in the house, on the street, in the public building. We may thank for it a chemical tension work of the fourth order—fire, retort, gasometer, conduction by stop-cocks, passing by all intermediate works—all of them important, all ramifying through the city pipes. The water for house and street necessity, when taken from a river-water conduit, furnishes a drive work of at least the sixth order. Upon the railroad we move by drive work of a higher order, regulate the powerful service with another, by means of drive works permit freight to be carried on the rails from place to place, from land to land, from one part of the earth to another, a thousand-fold more than a person could carry. Throughout the earth by means of physical drive work we have the messenger service, both written and spoken.

How fare we in war? In millions of chemical tension works, large and small, generally of higher order, we carry the driving force to the distant battlefield and there set it free by means of a high order of drive works.

Upon the ocean we are carried hundreds of miles from land, for weeks and months, by means of tension-work activity.

Rich productions, such as coal, we have gained from nature. The naturistical man early found upon the high mountain range the water course, that running work subordinated to tension work, and very likely the future will bring to light other products, such as petroleum, which we may say was discovered three decades ago. This product is a highly elastic chemical tension work fitted to play its part under a clear flame. In reality it is a combination of two or more chemical tension works, each under such slight restraint as to free itself invisibly.

We had, therefore, to submit this product of nature to a process of separation, according to the manganistical principle, into groups of small parts easily liberated and on which the tension work was first transmissible and generally applicable. Police directions required that if the product were made an article of trade the obstruction (sperrung) should be a safe one; but how favorable has been the result. This fluid tension work discovered, as it were, "ready made" in nature for purposes of illumination, has displaced those products which, by the aid of noticeable manganistical implements had previously been obtained from the seeds of plants. Let us turn to another phase of tension work. The conflagration is but an invisible liberating of a chemical tension work, as is well proved. The obstruction ratchet is raised in opposition to our will with ever-increasing rapidity and the powerful, liberated tension work often overleaps our control, but we bring to bear upon it for the purpose of its capture, another drive work, formerly operated by main strength only, but now usually by chemical tension-

work under the application of drive works of a still higher order. We also turn a chemical tension work, the gas or chemical engine, as the Americans call it, which acts instantaneously upon the water being used. In the last case the drive work connected with the water is of a very low order; this furnishes an example of the manner in which drive works contest for the same intended motion and seek each to gain for itself the palm in lessening the number of drive works, that is, the height of the order number. Everywhere it is the manganistical thought, the manganistical principle whereby we in part preserve, in part make easier, in part defend our life, and whereby we also advance annihilatingly against others.

Our industries, finally, which produce as well the necessities as the manganistical mechanisms, what have they not brought about for culture advancement by means of this same manganistical principle? Here let us venture a little nearer by attempting to apply a measure.

Coal serves us as an essential assistant in manganistical labor. This is now obtained in an abundance of over 400,000,000 tons, the greater part annually converted to industrial purposes. The surplus above 400,000,000 tons suffices to cover heating necessities. So we have for each of the 300 working days of the year one and one-third million tons of coal, which are used for chemical, mechanical, and physical-technical purposes. If we sum up the entire labor arrived at therewith for the sake of the survey of dynamical execution, the results under this acceptation of uses of coal show $1\frac{1}{4}$ kilograms for horse-power in a working day of 12 hours, *i. e.*, $4\frac{1}{2}$ tons per hour during the year, together with the horse. A horse-power, in round numbers, of 90,000,000, statistical numbers and taxes, in fact, would in dynamics yield 20,000,000. For every horse-power must be reckoned the working force of six strong men, which results in 540,000,000 active man-power during a day of 12 hours. It is this powerful executive force which the 250,000,000 of Atlantic nations entirely alone (since the other 1,250,000,000 of naturists have added nothing to it) have accomplished by man through the manganistical principle! When we consider that every tenth one of the 1,250,000,000 men exerts daily such labor as before contemplated, probably a much too high estimate, there results an execution of 125,000,000 man-power. We Atlantic peoples, a sixth of the earth's inhabitants, perform by our manganistic labor more than four times as much as those can execute. The superiority of the manganist over the naturist is attained and reimbursed through useful labor, and thereby also reaches, taken only humanly, its right. This so much the more as our labor execution is transmitted to each of them. I speak of the great, entire development, and not perhaps of its still existing deficiencies, to the extension and under the extension of culture and civilization.

So, then, has scientific technology become the bearer of culture, the powerful, tireless laborer in the service of civilization and cultivation of the races of men, and promises for a long future to add a line of greater results than is at present attained.

THE RAMSDEN DIVIDING ENGINE.*

By J. ELFRETH WATKINS, Curator, Section of Transportation and Engineering, U. S. National Museum.

The circle is a figure that has always been found in nature.

Although this simple geometrical figure has been used in inscriptions and for decoration from time immemorial, I have been able to discover only one very early reference to a pair of compasses, or dividers.

In referring to the graven images, the worship of which was forbidden by the Jewish law, the Prophet Isaiah, in chapter 44, verses 12, 13, old version, describes the manner in which these idols were constructed, as follows:

"The smith with the tongs both worketh in the coals and fashioneth it with hammers - - -." "The carpenter stretcheth out his rule; he marketh it out with a line; he fitteth it with planes and he marketh it out with a compass and maketh it after the figure of man."

In the revised version the phrase is translated—

"The carpenter stretcheth out a line; he marketh it out with a pencil; he shapeth it with planes and he marketh it out with the compasses and shapeth it after the figure of a man."

The Hebrew word which is here translated "compass" or "compassess," is *mehugah*, from *hug*, a circle—*mehug* something to make a circle.

There can, therefore, be little doubt that an instrument for drawing circles and probably similar to what is now known as the "compasses" was used by the Hebrew mechanics. Even if we accept the theory of a deutero Isaiah, this instrument can certainly claim the respectable antiquity of the sixth century B. C.

The circle was associated with the measurement of time and the observation of the positions of the heavenly bodies many centuries before the Christian era.

THE SUN-DIAL AND GNOMON.

The sun-dial of Ahaz, is thus alluded to in Isaiah, chapter 38, verse 8, old version, "Behold, I will bring again the shadow of the degrees, which is gone down in the sun-dial of Ahaz, ten degrees backward. So the sun returned ten degrees by which degrees it was gone down."

* Deposited in the U. S. National Museum by Dr. Henry Morton, president, Stevens Institute of Technology, Hoboken, New Jersey.

By recent Biblical critics* this dial is supposed to have been an obelisk whose shadow fell upon the steps of the palace of Ahaz, each step bein called a degree.

It is by no means improbable however that these degrees wer marked on a plane of stone or metal.

The simple records made by the Chaldean shepherds and herdsme of the observations by which they determined the seasons and by whic they were governed in the different operations of husbandry, led th early cultivators of science to devise instruments (doubtless crude in th beginning) by which they could obtain data for more accurately ascer taining the lengths of the solar and lunar periods.

Astrology and astronomy bore the closest relationship to each othe at that remote period.

"In the valley of the Euphrates there were in those days observa tories† in most of the large cities, and professional astronomers regu larly took observations of the heavens, copies of which were sent t the king, as each movement or appearance in the heaven was suppose to portend some evil or good to the kingdom."

Among the first instruments of which there is record is the gnomon,‡ with which the Babylonians were familiar, and from whom Herodotu states (II, 109) "the Greeks learned the use of it, together with th pole." The comparison of the perpendicular height of the gnomon, wi the length of its meridian shadow projected on a horizontal plane o the days of the summer and winter solstices, afforded the early astro omer an opportunity to calculate the difference of the sun's meridia altitudes on those days.

ANCIENT ASTROLABES.

Ptolemy, in his "Almagest," written 145 A. D., describes an astrolab or circular instrument for making celestial observations (which he cal *αστρολαζιπον οργανον*) which consisted of a heavy circle of metal arrange so that when it was suspended the divisions which we now call 0° an 180° would come to rest in the same horizontal plane.

A diametrical bar suspended in the center of the circle and turnin on a pin was furnished with disks containing slits through which an heavenly body could be seen and its altitude determined in degrees o parts thereof.

Other astrolabes were constructed in early times, consisting of tw graduated circles set exactly at right angles.

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*Compare Isaiah 38: 8, revised version: "I will cause the shadow on the steps which is gone down on the dial of Ahaz with the sun, to return backward ten steps so the sun returned ten steps on the dial whereon it was gone down."

† George Smith, "Assyrian Discoveries," p. 408.

‡ Vitruvius, who wrote in the first century B. C., gives in Book I, chap. 6, direc tions for using the gnomon to ascertain the north and south line in laying out th streets of a city, thus indicating that the Romans were not familiar with the magneti needle.

BABYLONIAN SYSTEM OF DIVIDING THE CIRCLE.

In a paper upon "Babylonian Astronomy," by Sayce and Bosanquet (*Monthly Notices Royal Astronomical Society*, 1880, vol. XL, No. 3,), relating to the tablets of the millennial period, from 2,000 B. C. to 1,000 B. C., I find this statement: "The divisions which we find employed are 8, 12, 120, 240, 480 parts. It has been assumed that the division of the circle into 360 parts was made by this ancient people. There is however no authority in the inscriptions for this assumption. It seems to have been derived originally from Achilles Tatius, and the pre-conceived idea thus introduced appears to have caused even those most conversant with the inscriptions to see the divisions of the circle into 360 in matters which do not involve it.

THE MODERN DIVISION AN OUTGROWTH OF THE SEXAGESIMAL SYSTEM.

"It is hardly doubtful that the division of the circle as practiced by Ptolemy and in modern times was an outgrowth of the sexagesimal system, but the latter does not contain the former. The numeration of the inscriptions is by two methods, the sexagesimal and the decimal.

"The decimal method is in all respects comparable with our own and was used by preference in the Assyrian period.

"In it words and signs were used which were precisely equivalent to our "hundreds" and "thousands."

"In the sexagesimal method the reckoning was the same as in the decimal up to 60; 60 was 1 soss. The counting went on by multiples of 60 + number over, up to 1 ner = 600. Then by ners + sosses + number over, up to 1 saru = 3,000.

"The numbers used are always taken in this way. There is no instance of counting by 60, 360, 3,600. The foundation of the number 360 was not, therefore, a natural step in the sexagesimal arithmetic of the inscriptions.

TABLET FROM THE PALACE OF SENNACHERIB.

"The division of the circle into 480 parts is illustrated by a tablet from the palace of Sennacherib (668–626 B. C.) in the British Museum, written in Accadian, which treats of the moon's position during a month. The numbers of them or many of them are unintelligible or corrupt. This is partly due to the fact that the tablet is a copy of an ancient one, probably the date before 2,000 B. C.; but there is amply sufficient left to show that there was a real division of 480 parts, the moon's mean daily motion being 16°, as it should be roughly, throughout the intelligible portions."

The numbers of the tablet are as follows:

[The word "degrees" is used to represent the units of the division of 480.]

Day of the moon.	Moon advances.		Day of the moon.	Moon advances.	
	Degrees.	Degrees.		Degrees.	Degrees.
1	5	....	16*	....	1[illegible]
2	10	....	17	208	3[illegible]
3	20	....	18	192	4[illegible]
4	40	....	19	176	6[illegible]
5	80	....	20	160	8[illegible]
6	96	....	21	144	9[illegible]
7	112	....	22	128	11[illegible]
8	128	....	23	112	14[illegible]
9	144	....	24	96	3[illegible]
10	160	....	25	80	5[illegible]
11	176	....	26	32	1[illegible]
12	192	....	27	23	2[illegible]
13	208	....	28	15	[illegible]
14	224	....	29	$5\frac{1}{20}$	....
15	240	....	30 †	....	....

* The sixteenth day, for 224° of advance, it becomes obscure and retrogrades.
† The thirtieth day the moon is the god Anu.

S. 162.

OBVERSE

Fig. 1.—Babylonian Planisphere.

FRAGMENT OF PLANISPHERE IN THE BRITISH MUSEUM.

Figure 1 represents a small fragment of a planisphere in the Britis[h] Museum (S. 162). It contains two compartments, each of which is char[...]

acterized by the name of a month. The month Marchesvan is the eighth and Cislev the ninth. The arcs have at their left-hand corners the numbers shown. (This is a "sky aspect." A "globe aspect" would be the reverse.)

"This remarkable fragment is sufficient to determine the following table, in which the year is supposed to be divided into 12 mean months:

No. of month.	Outer circle.	Inner circle.
	40	20
1		
	20	10
2		
	240	120
3		
	220	110
4		
	200	100
5		
	180	90
6		
	160	80
7		
	140	70
8		
	120	60
9		
	100	50
10		
	80	40
11		
	60	30
12		

"To compare the longitudes of the planisphere with our own we have the following table, taking the numbers of the inner circle, *i. e.*, the division of 120:

Name of month.	No. of month.	Degrees.	Degrees. Longitude from same zero with 360°, reckoned positively.
		20	300
Nisan	1		
		10	330
Iyyar	2		
		120	360
Sivan	3		
		110	30
Tammuz	4		
		100	60
Ab	5		
		90	90
Elul	6		
		80	120
Tisri	7		
		70	150
Marchesvan	8		
		60	180
Cislev	9		
		50	210
Tebet	10		
		40	240
Sebat	11		
		30	270
Adar	12		
		20	300

The late George Smith proposed to read 150 for 140, and 75 for 70.* Sayce and Bosanquet assert that "There is no foundation for this, except the pre-conceived idea that the circle ought to be divided into 360°. The numbers are imprinted on the clay with great clearness according to the sexagesimal notation." (*Monthly Notices*, *R. A. S.*, 1880.)

REASONS FOR DIVIDING THE CIRCLE INTO 360 DEGREES.

On the other hand, we have the generally accepted statement that the Egyptians divided the circle into 360° from the sun's annual course or according to the number of days, dividing the year into 12 months, and each month into 30 days and allotting 1° to each day with an intercalary month every 6 years.

The Greeks divided each month into three periods of ten days each.

It will be remembered that the Jewish year contained only 354 days.

It is not definitely known how the astrolabes of Hipparchus (second century B. C.) and Ptolemy (second century A. D.) were divided; probably these graduated circles contained 360°. It is stated that the parallactical instrument used by Copernicus (1473–1543), and by which he measured altitudes, had its limb divided by equal divisions that were the subtenses of 3′ 49″.137 each. If an error of only 4″.1 was made in measuring this instrument, and if 3′.45″ was the correct reading it would indicate that each quarter of the circle was divided into 1,440, or each sixth of the circle into 960 equal parts.

Many writers believe that the number 360 was selected from the fact that it admits of a great many aliquot parts, such as 2, 3, 4, 5, 6, 8, and 9.

It has occurred to me as not an unreasonable conjecture that the origin of the sexagesimal system may have resulted in some way from the fact that the circumference of the circle is divided into six equal parts by chords exactly equal to the radius in length. I do not remember to have seen this theory advanced by any previous writer.

The earliest records indicate that each day was divided into six parts.

In a recent paper on "Chaldean Astronomy," by Dr. Christopher Johnson, of Johns Hopkins University (p. 141), he asserts that "in the earliest tablets the day is divided (at least for astronomical purposes) into six watches—three day watches and three night watches." "In the later tablets, however, we find a division of the day into 12 kaspu or double hours, each kaspu being divided into 60 degrees or minutes."

There is mention of an inscription on a tablet in "Western Asiatic Inscriptions" (published by the British Museum, III 51, 1), a translation of which reads: "The sixth day of Nisan, day and the night were balanced there were 6 kaspu of day and 6 kaspu of night."

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* "I am of opinion that the numbers under the month of Marchesvan, 140° and 70° are errors in the Assyrian copy and should be 150° and 75°." (George Smith's "Assyrian Discoveries," p. 407.)

DECIMAL DIVISION OF THE CIRCLE ADVOCATED IN THE SIXTEENTH CENTURY.

Whatever may have been the origin of the division of the circle into 360° the system has been condemned from time to time by many eminent mathematicians, among them Stevinus (1548–1620), who, in his "Cosmography" (lib. I, def. 6), states that "the decimal division of the circle (which he contends for) prevailed in *Sæculo sapienti.*"*

Henry Briggs (1556–1630), Oughtred (1574–1660), and Sir Isaac Newton (1642–1727) constructed large tables of sines, the plan being to divide each degree into 100 minutes of 100 seconds each.

Dr. Charles Hutton, in the early part of this century, published extensive tables giving real lengths of arcs of various decimal degrees in terms of the radius. Some of the French mathematicians divided the quadrant into 100 degrees and then into decimals of degrees. William Crabtree, Gascoigne,† and Jeremiah Horrocks ‡ (1619–1641) projected tables with complete decimal divisions, the whole arc of the circle being divided into 1,000,000 parts. (Philosophical Transactions, vol. XXVII, p. 230.)

DECIMAL SYSTEM FREQUENTLY USED BY THE HEBREWS.

I have taken some pains to find, if possible, some trace of the employment of a sexagesimal numerical system by the Hebrews in the measurement of straight lines.

In the description of the city and temple seen by Ezekiel in his vision and described in the fortieth and forty-second chapters, the measuring reed (qana) § of 6 great cubits, corresponding somewhat with our 10-foot rod, is mentioned in ten places.

The decimal system however was more frequently used than the sexagesimal in noting the dimensions of the walls and courts described in these chapters. Thus the number 500 is found three times, 100 eleven times, 90 one time, 70 one time, 60 one time, 50 nine times, 30 two times, 25 five times, 20 six times, 10 three times, 5 seven times. It would seem reasonable to assume that in describing an imaginary structure the

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* The decimal method is in all respects comparable with our own and was used by preference in the Assyrian period. In it words and signs were used which were precisely equivalent to our "hundreds" and "thousands." (Sayce and Bosanquet, vol. 40, *Monthly Notices, Royal Astronomical Society.*)

† Gascoigne is said to have invented a micrometer about 1640.

‡ Horrocks observed the first transit of Venus that was carefully noticed November 24, O. S. 1639, that predicted by Kepler in 1631 being invisible in Europe.

§ Ezekiel 40 : 3, revised version : "And he brought me thither, and behold there was a man, whose appearance was like the appearance of brass, with a line of flax in his hand, and a measuring reed." Same chapter, verse 5: "And behold, a wall on the outside of the house round about, and in the man's hand a measuring reed of 6 cubits long of a cubit and an handbreadth each ; so he measured the thickness of the building one reed ; and the height one reed."

dimensions given would be according to the method of enumeration in general use.

In noting measurements of length in other portions of the Scriptures three-score is used three times:

1 Kings, 6:2: "And the house which King Solomon built for the Lord, the length thereof was three-score cubits, and the breadth thereof twenty cubits, and the height thereof thirty cubits."

Ezra, 6:3: "Let the house be builded, the place where they offer sacrifices, and let the foundations thereof be strongly laid; the height thereof three-score cubits, and the breadth thereof three-score cubits."

Daniel, 3:1: "Nebuchadnezzar the king made an image of gold, whose height was three-score cubits, and the breadth thereof six cubits."

The numbers 6 and 12 are used elsewhere as follows:

Ezekiel, 41:1, revised version: "And he brought me to the temple, and measured the posts, six cubits broad on the one side and six cubits broad on the other side, which was the breadth of the tabernacle."

Ezekiel, 43:16, revised version: "And the altar hearth shall be twelve cubits long by twelve broad, square in the four sides thereof."

METHODS OF DIVIDING THE CIRLE BY HAND.

The most ancient figure with graduated divisions of a circle discovered in England, was a quadrant, marked with Roman characters, which was found on a chimney piece at Helmdon, in Northampton shire, with the date M°133 (meaning A. D. 1133) marked upon it.

Different methods of dividing a metallic or wooden circle into degrees and their subdivisons were successfully practiced by the early astronomers, notably by Tycho Brahe* (1546–1601), of Sweden; Johann Hevelius† (1611–1687), of Dantzic, in Poland; Dr. Robert Hooke (1635–1703), while curator of experiments of the Royal Society; Ole Roemer (1644–1710), the Danish astronomer, of whom it is said that he may be considered "the inventor of nearly all our modern instruments of precision," and many of whose ideas were adopted by astronomers a century later.

In attempting to engrave and divide correctly the circles used for mathematical purposes, all of these early laborers in the field of science were compelled to depend entirely upon manual skill.

The first notable example of the division of circular arcs of which I have found record is the mural arc, of 8 feet radius, which George Graham graduated for the English National Observatory in 1725. The

* An electro replica of Tycho Brahe's quadrant, from the original in the British Museum, is deposited in the Smithsonian Institution. Triangular diagonals are not found in this instrument. Tycho Brahe's instruments had the advantage of long radii, which rendered any inequalities that might occur in his divisions of less value than instruments of short radii; the smallest subdivisions into which he professed to mark his spaces were 10′ each.

† The errors of Hevelius' large sextant for 6′ radius used about 1650, amounted to 15″ or 20″.

manner in which it was accomplished is described substantially as follows (see p. 332, Smith's Optics, 1738):

"Two concentric arcs of radii 96.85″ and 95.8″ respectively were first described by the beam compass. On the inner of these arcs 90° was to be divided into degrees and twelfth parts of a degree, while the same on the outer was to be divided into 96 equal parts, and these again into sixteenth parts. The reason for adopting the latter was that 96 and 16 both being powers of 2, the divisions will be got at by continual bisection alone, which, in Graham's opinion, who first employed it, is the only accurate method, and would thus serve as a check upon the accuracy of the divisions of the outer arc. With the same distance on the beam compass as was used to describe the inner arc, laid off from 0°, the point 60° was at once determined.

"With the points 0° and 60° as centers successively, and a distance on the beam compass very nearly bisecting the arc of 60°, two slight marks were made on the arc; the distance between these marks was carefully divided by the hand, aided by a lens, and this gives the point 30°. The chord of 60° laid off from the point 30° gave the point 90°, and the quadrant was now divided into three equal parts. Each of these parts was similarly bisected, and the resulting divisions again trisected, giving 18 parts of 5° each. Each of the quinquesected gave degrees, the twelfth parts of which were were arrived at by bisecting and trisecting as before. The outer arc was divided by continual bisection alone, and a table was constructed by which the readings of the one arc could be converted into those of the other. After the dots indicating the required divisions were obtained, either straight strokes, all directed towards the center, were drawn through them by the dividing knife, or sometimes small arcs were drawn through them by the beam compass having its fixed point somewhere on the line which was a tangent to the quadrantal arc at the point where a division was to be marked."

In 1767 John Bird, an English mathematical-instrument maker, graduated a quadrant of 8 feet radius. His method was that of continual bisection, and is described in a pamphlet published by order of the commissioners of longitude, 1767, entitled "The Method of Dividing Astronomical Instruments," by John Bird, mathematical-instrument maker in the Strand.

The exact radius which he used was $95\frac{938}{1000}$ inches. The radius laid off from the point 0° gave the point 60°. This arc of 60° was carefully bisected, giving the point 30°, from which the radius, that had remained undisturbed on the original beam compasses, was laid off, giving the point 90°.

The chords of 30°, 15°, 10° 20′ 4° 40′, and 42° 40′ were computed and carefully laid off, each on a separate pair of beam compasses. Bird used an exact scale of equal parts, which by the aid of a magnifying glass he was able to read to one one-thousandth of an inch.

Having marked the four points 0°, 30°, 60°, and 90°, the mode of procedure was as follows: The chord of 15° laid off backward from 90° gave 75°. From 75° the chord of 10° 20′ was laid off forward, giving 85° 20′, and from 90° the chord of 4° 40′ laid off backward gave the same point.

85° 20′ = 5,120′ or 1,024 chords of 5′ each, and $1,024 = 2^{10}$ (2 carried to the tenth power), so that by continual bisections the arcs of 5′ were accurately marked.

In order to divide the circle beyond the 85° 20′ into arcs of 5′ each, an arc of 40′ (or eight 5′ divisions) was laid off backwards to 84° 40′, thus leaving an arc of 320′ or 64 arcs of 5′ each between these two points. These 5′ arcs were laid off by continual bisections. Thus Bird was able to check accurately the original arcs of 15°, 30°, 60°, 75°, and 90°.

ORIGIN OF THE DIVIDING ENGINE—CUTTING ENGINES FOR CLOCK WHEELS.

To the clock-maker, more than any other mechanic, we are indebted for the origin of the dividing engine.

"While the art of clock-making was in its rude state the dividing of a wheel into a number of parts and cutting away notches of spaces was done by manual operation with a file. This was not only a tedious but a very imperfect way of obtaining a desired result, since the unequal lines in the size and shape of the tools prevented it from transmitting applied force in an equable manner.

"To facilitate the manual operation of cutting wheels by a file the sample platform was invented (described by Father Alexander in his book on clock-making), which was a circular plate of brass from 10 inches to a foot or more in diameter, with as many concentric circles thereon as the usual number of teeth in the wheels and pinions of clockwork required to be divided into corresponding parts of a circle. In the center of this platform was fixed a stem or fast arbor, around which an alidade, ruler, or index, with a straight edge pointing to the center, turned freely into any given point of a required circle, by means of which the divisions of any given circle were transferred to a wheel placed on the side stem under the side index by a marking point. At length a little frame was mounted on the index, which was contrived to direct and confine the file in such a way as to cut the notches of a wheel placed over the index with less deviation from the truth than could be managed by mere manual dexterity. This addition, no doubt, led to the adoption of a circular file, or cutter, and of such other appendages as completed the construction of the simple cutting machine."*

It is asserted in "*Etrennes Chronometriques*" par M. Julian le Roy, "that Dr. Hooke was the first person who contrived, about 1675, such an arrangement as could merit the name of a cutting engine (machine á

* See Rees' Encyclopædia, vol. II: "Cutting engines."

fendre). The doctor's invention, which, like many of his inventions, has proved to be of permanent and great utility in mechanics, consists of an entire transmutation of the old stationary platform, with its movable appendages, into a movable platform inserted into a strong metallic frame, with stationary and additional appendages; the machine thus converted into an engine or self-acting piece of mechanism consisted of a strong frame; the sliding supporting bars of the platform or plate with a horizontal screw of adjustment for distance from the circular file; the dividing plate with a revolving arbor to receive the wheel to be cut; and the alidade fixed to the great frame in the position of a tangent line to any of the dividing circles and applying its bent and rounded point to the punched marks of division on the circle successively as the plate revolving in the act of cutting the successive teeth of a wheel."

In the year 1716, Henry Sully brought to England from France a cutting engine, made by M. de la Faudriere, which has been mentioned by Julien le Roy and described by Thiout in his "*Traite d'Horlogerie.*"

In 1730, M. Taillemard made further improvements in the cutting engine, particularly by introducing a tubed arbor instead of an arbor with a square hole, which had been used before.

After Taillemard, his apprentice Hulot continued to construct engines in a superior way in France, and was succeeded by his son, whose execution was deemed equal to that of his father's.

EARLY DIVIDING ENGINES.

Smeaton, in a paper entitled "The graduation of astronomical instruments," read before the Royal Society at London, November 17, 1785, mentions an engine made in 1741, by Henry Hindley, of York, England, which indented the edge of any circle in such a way that a screw with fifteen threads acting at once would, by means of a micrometer, read off any given number of divisions, so as to answer the purpose of subdividing the circle.

It would appear that this engine was better adapted for cutting toothed wheels for clock-work than for graduating circles with exactness.

The Duc de Chaulnes, in a memoir to the Royal Academy of Science, at Paris, published 1765, referred to the difficulties in obtaining perfection of the screw and notches of the rack "so that they be rendered perfectly equal, notwithstanding the unequal density and hardness of different portions of the metal so racked." He calls his method "the explication of the new way of dividing."

It is said that he constructed an engine which he claimed to be his original invention, but unfortunately the want of "a perfect screw with intervals exactly proportioned to the effective radius of his quadrant, was a source of error that posterior contrivances were required to remedy."

Ramsden's machine for cutting the screws of his dividing engine accurately (which will be referred to below), reduced these errors to a minimum.

JESSE RAMSDEN'S DIVIDING ENGINES.

Jesse Ramsden was the son of an innkeeper, and was born near Halifax, in Yorkshire, in 1735. While at school in his native county his fondness for mathematics was observed. Although he served as an apprentice to a cloth maker in Halifax for some time, yet at the age of twenty-four he had become skillful in making mathematical and philosophical instruments, and his success was so great that he was soon able to open an extensive establishment in London.

It is stated that Ramsden first had his attention called to the subject of dividing engines in 1760, by the reward which was offered by the English board of longitude to John Bird for his method of dividing.

Ramsden was doubtless acquainted with what Hooke, the Duc de Chaulnes, Hindley, and others had previously done, and before the spring of 1768 he completed his first engine, having in 1760 constructed a very superior sextant.

This first engine had an indented plate 30 inches in diameter, and was used to divide theodolites and other common instruments, and did so with sufficient accuracy, but it was not satisfactory to Mr. Ramsden, who, in 1774–'75, constructed the engine, with a plate 45 inches in diameter, which is now in the U. S. National Museum. (See Plate I, from a recent photograph.)

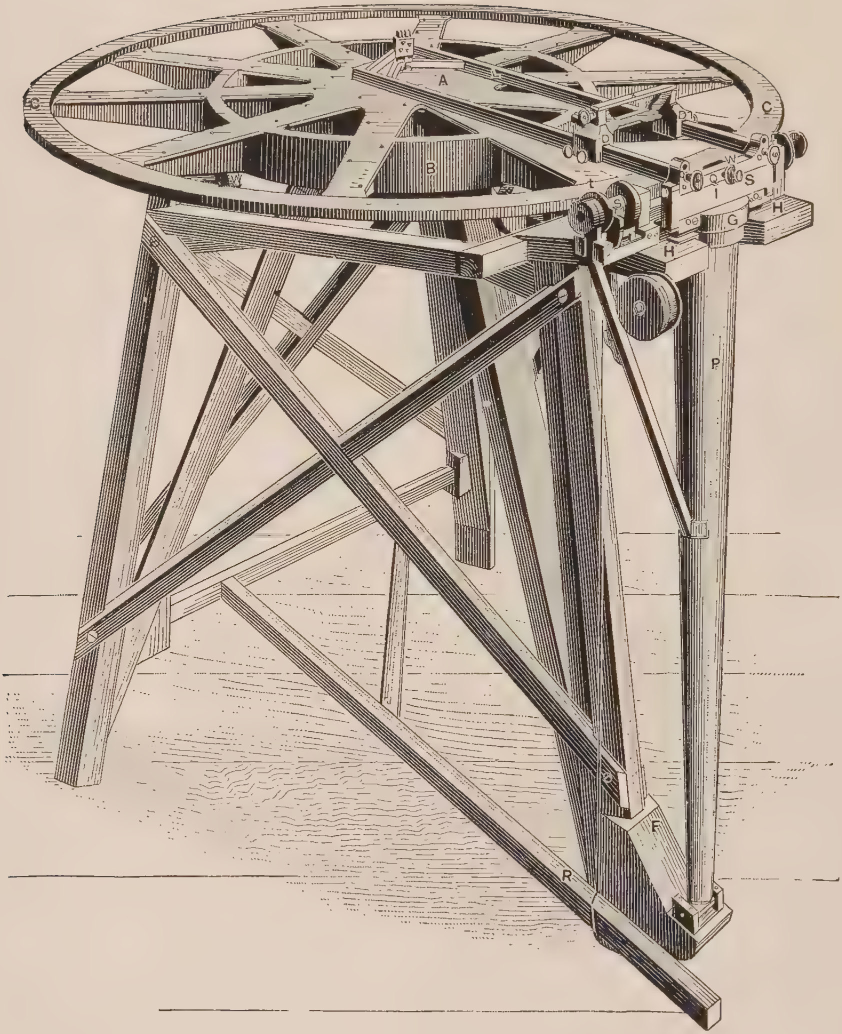
This dividing engine, together with the cutting gear with which the screws of this machine was made, were sold by the heirs of Ramsden to Messrs. Knox and Shain, of Philadelphia, Pennsylvania, from whom Prof. Henry Morton, president of the Stevens Institute of Technology, Hoboken, New Jersey, purchased them about 10 years ago. Dr. Morton has recently deposited these machines in the U. S. National Museum.

The test of this, Ramsden's second engine, which divided a sextant for Mr. Bird's examination accurately, was so satisfactory "that the board of longitude, ever ready to remunerate any successful endeavor, and to promote the lunar method of determining longitude at sea," conferred a handsome reward on the inventor on condition that the engine should be at the service of instrument makers, and that Mr. Ramsden would publish an explanation of his method of making and using it. This he did in a quarto pamphlet in 1777, the preface to which was prepared by Nevil Maskeline, astronomer royal, dated Greenwich, November 28, 1776. In the following extract from it the reasons for publishing the pamphlet are given:

"Mr. Ramsden, mathematical instrument maker in Piccadilly, was paid the sum of £615, by certificate from the commissioners of longitude, upon delivering to them, upon oath, a full and complete written explana-

RAMSDEN DIVIDING ENGINE.

Deposited in the National Museum by Dr. Henry Morton. (From photograph.)



RAMSDEN DIVIDING ENGINE.

(From original lithograph in Ramsden's publication.)

tion and description of his engine for dividing mathematical instruments (accompanied with proper drawings) and of the manner of using the same, and also of the engine by which the endless screw, being a principal part of the said dividing engine, was made, and upon agreeing and entering into articles with them for assigning over the right and property of the said engine to them for the use of the public, and engaging himself to give to the said commissioners and such other persons, being mathematical instrument makers, not exceeding ten, as shall be appointed by them during the space of 2 years, from the 28th of October, 1775, to the 28th of October, 1777, such instruction and information with regard to the making and using of the said engine, as may be fully sufficient to enable any intelligent workman to construct and use other engines of the same kind, and also binding himself to divide all octants and sextants by the said engine which shall be brought to him by any mathematical instrument makers for that purpose at the rate of 3 shillings for each octant and at the rate of 6 shillings for each brass sextant, with nonius divisions to half minutes, for so long a time as the said commissioners shall think proper to permit the said engine to remain in his possession. Of which sum of £615 paid to the said Mr. Ramsden, £300 was given him as a reward for the improvement made by him in the art of dividing instruments by means of the said dividing engine and for discovering the same, and the remaing £315 in consideration of his making over the property in the said engine to the commissioners of longitude, for the use of the public, and for the other considerations before mentioned.

"In order to render this instrument more extensively useful, the commissioners of longitude ordered the written explanations, with drawings, of the dividing engine to be prepared for publication, and it is now published accordingly."

Plate II is from a lithograph in Ramsden's publication, and illustrates the machine as originally constructed.

Mr. Ramsden states in his pamphlet that "the teeth on the circumference of the wheel were cut by the following method:

"Having considered what number of teeth on the circumference would be the most convenient, which in this engine is 2,160, or 360 multiplied by 6, I made two screws of the same dimensions of tempered steel, in the manner hereafter described, the interval between the threads being such as I knew by calculation would come within the limits of what might be turned off the circumference of the wheel. One of these screws, which was intended for ratching or cutting the teeth, was notched across the threads, so that the screw, when pressed against the edge of the wheel and turned round, cut in the manner of a saw. Then, having a segment of a circle a little greater than 60 degrees, of about the same radius with the wheel, and the circumference made true, from a very fine center, I described an arch near the edge, and set off the chord of 60 degrees on this arch. This segment was put in the place of the

wheel, the edge of it was ratched, and the number of revolutions and parts of the screw contained between the interval of the 60 degrees were counted. The radius was corrected in the proportion of 360 revolutions, which ought to have been in 60 degrees, to the number actually found, and the radius, so corrected, was taken in a pair of beam compasses while the wheel was on the lath, one foot of the compasses was put in the center and with the other a circle was described on the ring; then half the depth of the threads of the screw being taken in dividers was set from this circle outwards and another circle was described, cutting this point; a hollow was then turned on the edge of the wheel of the same curvature as that of the screw at the bottom of the threads; the bottom of this hollow was turned to the same radius or distance from the center of the wheel as the outward of the two circles before mentioned.

"The wheel was now taken off the lathe, the bell-metal piece (*D*) was screwed on as before directed, which after this ought not to be removed.

"From a very exact center a circle was described on the ring *C*, about four-tenths of an inch within where the bottom of the teeth would come. This circle was divided with the greatest exactness I was capable of, first into 5 parts and each of these into 3. These parts were then bisected 4 times, that is to say, supposing the whole circumference of the wheel to contain 2,160 teeth, this being divided into 3 parts, each of them would contain 144, and this space bisected 4 times would give 72, 36, 18, and 9; therefore each of the last divisions would contain 9 teeth. But, as I was apprehensive some error might arise from quinquesection and trisection, in order to examine the accuracy of the divisions I described another circle on the ring *C*, one-tenth inch within the former, and divided it by continual bisections, as 2,160, 1,080, 540, 270, 135, $67\frac{1}{2}$, and $33\frac{3}{4}$; and, as the fixed wire (to be described presently) crossed both the circles, I could examine their agreement at every 135 revolutions (after ratching could examine it at every $33\frac{3}{4}$); but not finding any sensible difference between the two sets of divisions, I, for ratching, made choice of the former; and, as the coincidence of the fixed wire with an intersection could be more exactly determined than with a dot or division, I therefore made use of intersections in both circles before described.

"The arms of the frame were connected by a thin piece of brass of three-fourths of an inch broad, having a hole in the middle of four-tenths of an inch in diameter; across this hole a silver wire was fixed exactly in a line to the center of the wheel; the coincidence of this wire with the intersections was examined by a lens seven-tenths inch focus, fixed in a tube which was attached to one of the arms.

"Now a handle or winch being fixed on the end of the screw, the division marked 10 on the circle was set to its index, and, by means of a clamp and adjusting screw for that purpose, the intersection was set exactly to coincide with the fixed wire; the screw was then

carefully pressed against the circumference of the wheel by turning the finger-screw; then, removing the clamp, I turned the screw by its handle 9 revolutions, till the intersection marked 240 came nearly to the wire; then, unturning the finger-screw, I released the screw from the wheel and turned the wheel back till the intersection marked 2 exactly coincided with the wire, and by means of the clamp before mentioned, the division 10 on the circle being set to its index, the screw was pressed against the edge of the wheel by the finger-screw; the clamps were removed, and the screw turned 9 revolutions till the intersection marked 1 nearly coincided with the fixed wire; the screw was pressed, as before, the wheel was turned back till the intersection 3 coincided with the fixed wire; the division 10 on the circle being set to its index, the screw was pressed against the wheel as before, and the screw turned 9 revolutions till the intersection 2 nearly coincided with the fixed wire, and the screw was released; and I proceeded in this manner till the teeth were marked round the whole circumference of the wheel. This was repeated three times round, to make the impression of the screw deeper. I then ratched the wheel round continually in the same direction without ever disengaging the screw, and in ratching the wheel about 300 times round the teeth were finished.

"Now it is evident, if the circumference of the wheel was even one tooth or ten minutes greater than the screw would require, this error would in the first instance be reduced to one-two-hundred-and-fortieth part of a revolution or two seconds and a half; and these errors or inequalities of the teeth were equally distributed round the wheel at the distance of 9 teeth from each other. Now, as the screw in ratching had continually hold of several teeth at the same time, and these constantly changing, the above-mentioned inequalities soon corrected themselves and the teeth were reduced to a perfect equality.

"The piece of brass which carries the wire was now taken away and the cutting screw was also removed and a plain one (hereafter described) put in its place. On one end of the screw is a small brass circle, having its edge divided into 60 equal parts and numbered at every sixth division, as before mentioned. On the other end of the screw is a ratchet-wheel having 60 teeth, covered by the hollowed circle, which carries two clicks that catch upon the opposite sides of the ratchet when the screw is to be moved forward.

"The cylinder turns on a strong steel arbor, which passes through and is firmly screwed to the piece *Y*. This piece, for greater firmness, is attached to the screw-frame by braces; a spiral groove or thread is cut on the outside of the cylinder, which serves both for holding the string and also giving motion to the lever on its center by means of a steel tooth that works between the threads of the spiral. To the lever is attached a strong steel pin on which a brass socket turns. This socket passes through a slit in the piece, and may be tightened in any part of the slit by the finger-nut. This piece serves to regulate the number of revolutions of the screw for each tread of the treadle,

"Several different arbors of tempered steel are truly ground into the socket in the center of the wheel. The upper parts of the arbors that stand above the plane are turned of various sizes, to suit the centers of different pieces of work to be divided.

"When any instrument is to be divided, the center of it is very exactly fitted on one of these arbors, and the instrument is fixed down to the plane of the dividing wheel by means of screws, which fit into holes made in the radii of the wheel for that purpose.

"The instruments being thus fitted on the plane of the wheel, the frame which carries the dividing point is connected at one end by finger screws with the frame which carries the endless screw; while the other end embraces that part of the steel arbor which stands above the instrument to be divided by an angular notch in a piece of hardened steel; by this means both ends of the frame are kept perfectly steady and free from any shake.

"The frame carrying the dividing point or tracer is made to slide on the frame which carries the endless screw to any distance from the center of the wheel as the radius of the instrument to be divided may require, and may be there fastened by tightening two clamps, and the dividing point or tracer being connected with the clamps by the double-jointed frame admits a free and easy motion towards or from the center for cutting the divisions without any lateral shake."

ENGINE BY WHICH THE ENDLESS SCREW OF THE DIVIDING ENGINE WAS CUT.

The machine constructed by Ramsden for cutting the screw, and used to cut the 2,160 teeth in the circumference of the circle of his dividing engine, is of the greatest interest, for it is one of the earliest applications of the principle of changing the lateral speed of the tool in cutting a screw by differential wheels;—the method now used in the slide rest of a lathe.

Plate III is from a photograph of this machine deposited in the U. S. National Museum by Dr. Morton.

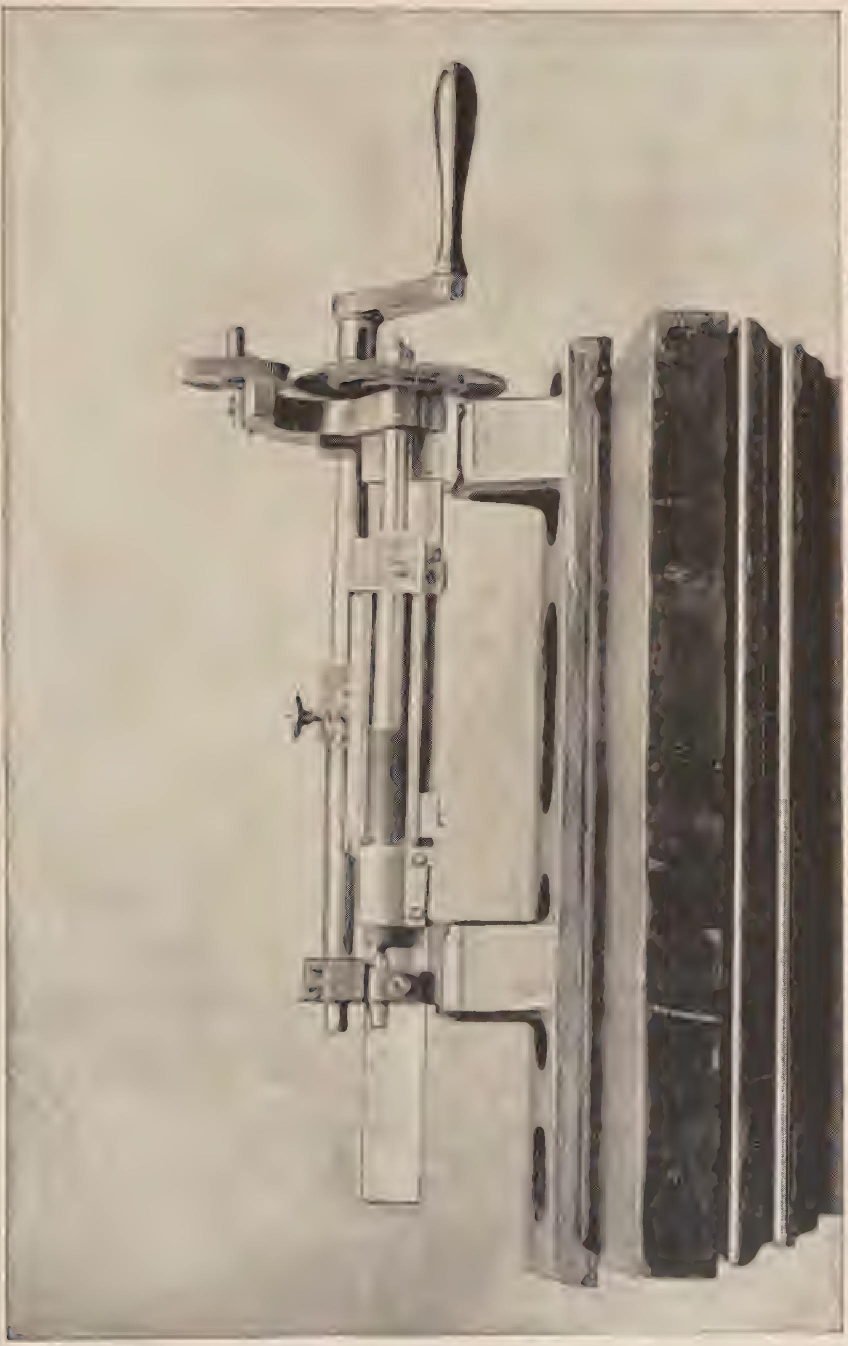
It has not been found practicable to letter the various parts of this machine to correspond with those referred to in Ramsden's description.

It is believed however that the reader will find more interest in following the original description in the words of the celebrated mechanician than in reading an explanation of the construction of the machine couched in modern terms.

Ramsden describes his machine thus:

A represents a triangular bar of steel, to which the triangular holes in the piece *B* and *C* are accurately fitted, and may be fixed on any part of the bar by the screws *D*.

E is a piece of steel whereon the screw is intended to be cut, which, after being hardened and tempered, has its pivots turned in the form of two frustrums of cones, as represented in the drawings of the dividing engine (foot-note Fig. 5). These pivots were very exactly



MACHINE BY WHICH THE ENDLESS SCREW AND THE TEETH ON THE PLATE OF THE RAMSDEN DIVIDING ENGINE WERE CUT.

Deposited in the U. S. National Museum by Dr. Henry Morton. (From photograph.)

fitted to the half holes *F* and *T*, which were kept together by the screws *Z*.

H represents a screw of untempered steel, having a pivot, *I*, which runs in the hole *K*. At the other end of the screw is a hollow center, which receives the hardened conical point of the steel pin *M*. When this point is sufficiently pressed against the screw to prevent its shaking the steel pin may be fixed by tightening the screws *Y*.

N is a cylindric nut, movable on the screw *H*, which, to prevent any shake, may be tightened by the screws *O*. This nut is connected with the saddle piece *P* by means of the intermediate universal joint *W*, through which the arbor of the screw *H* passes. A front view of this piece, with a section across the screw arbor is represented at *X*. This joint is connected with the nut by means of two steel slips, *S*, which turn on pins between the cheeks *T* on the nut *N*. The other ends of these slips, S, turn in like manner on pins (*a*). One axis of this joint turns in a hole in the cock (*b*), which is fixed to the saddle piece, and the other turns in a hole, (*d*), made for that purpose in the same piece on which the cock (*b*) is fixed. By this means, when the screw is turned round, the saddle piece will slide uniformly along the triangular bar *A*.

Having measured the circumference of the dividing wheel, I found it would require a screw about one thread in a hundred coarser than the guide screw *H*. The wheels on the guide-screw arbor *H*, and that on the steel *E*, on which the screw was to be cut, were proportioned to each other to produce that effect by giving the wheel (*L*) 198 teeth and the wheel (*Q*) 200. These wheels communicated with each other by means of the intermediate cogwheel *R*, which also served to give the threads on the two screws the same direction.

K is a small triangular bar of well-tempered steel, which slides in a groove of the same form on the saddle piece *P*. The point of this bar or cutter is formed to the shape of the thread intended to be cut on the endless screw. When the cutter is set to take proper hold of the intended screw it may be fixed by tightening the screws (*e*), which press the two pieces of brass, *G*, upon it.

The saddle piece *P* is confined on the bar *A* by means of the pieces (*g*), and may be made to slide with a proper degree of rightness by the screws (*n*).

RAMSDEN GRADUATES THE GREAT THEODOLITE NOW AT GREENWICH.

In 1785 Mr. Ramsden was requested "to make an instrument for measuring horizontal angles with more precision than the ordinary theodolite." It was with this dividing engine that Ramsden graduated this instrument known as "the great theodolite," still preserved at Greenwich, for the trigonometrical survey of Great Britain, described in Vol. 80, Philosophical Transactions.

One of the first projects of the trigonometrical survey of Great Britain

was to measure the exact linear distance between the observatory at Greenwich and the observatory at Paris, which was satisfactorily accomplished under the direction of General Roy.

In January, 1788, Jesse Ramsden, who had twice before undertaken the task of constructing an astronomical circle, began the one which he completed August, 1789.

His death occurred in 1800, at which time he was a member of the Royal Society, Fellow of the Imperial Academy of St. Petersburg, and wore a Copley medal.

THE DIVIDING ENGINES OF TROUGHTON, SIMS AND OTHERS.

Eighteen years after the completion of Ramsden's engine (1793), Edward Troughton completed a circular dividing engine, somewhat similar in detail, with a plate smaller than Ramsden's. And in 1843, William Sims, Troughton's successor, completed his engine, which has for nearly 50 years been in constant use at Charlton, near London.

Sims claimed that the merit of this engine consisted in making the axis of the plate a hollow tube into which the axis of the circle to be divided could be slipped, not making it necessary to detach the plate while it was being graduated, and obviating the necessity of re-setting the circle on the axle, which is always liable to create error.

Reichenbach, in Germany, and Gambey, in Paris, and Adie, in Edinburgh, also constructed dividing engines of merit. Reichenbach's was for a long time unsurpassed in accuracy. Gambey's is now at Hotel Cluny, Paris.

The German method which admits of great accuracy under skillful management, is performed by copying from a large circle, originally divided with extreme precision; over this circle the copy to be made is fixed concentrically; the degrees and minutes are cut into the copy by the aid of the micrometer microscope fixed independently over the divided circle.

In 1818, Repsold erected a circle at Göttengen and in 1819 Reichenbach erected one at Königsberg. Pistor and Martins of Berlin, constructed circles for Copenhagen, Albany, Leyden, Leipsic, Berlin, Washington Naval Observatory, and Dublin. Since the death of Martins, Repsold constructed circles for Strasburg, Bonn, Williamstown, Massachusetts, and Madison, Wisconsin; Troughton and Sims doing the work for Greenwich, Harvard, and Cambridge.

The Altazimuth, 8 feet in diameter, now (1890) at Palermo Observatory, was divided by hand by Ramsden

In 1806, Troughton constructed the first modern circle for the observatory at Blackheath.

In the Philosophical Transactions, for 1809, in a paper by Troughton on dividing instruments, p. 140, he states:

"I now subjoin a re-statement of the greatest errors of each of the

instruments that are brought into comparison by Sir George Shuckburg, after having reduced them all by one rule, viz :

"Allowing each of the two points which bound the most erroneous extent to divide the apparent error equally between them.

"They are expressed in parts of an inch and follow each other in the order of their accuracy.

Sir George Shuckburg's 5-feet standard	.000165
General Roy's scale of 42-inch standard	.000240
Sir George's equatorial, 24-inch standard	.000273
The Greenwich quadrant, 8-feet standard	.000465
Mr. Aubert's standard, 5-feet standard	.000700
The Royal Society's standard,* 92-inch standard	.000795

"For the justness of the above statement I consider my name pledged."

I am informed by recent travellers in China and Japan that the circles for astronomical and other instruments are still divided by hand, unaided by machinery.

The dividing engine at the Coast Survey, Washington, made by Troughton, was made automatic by Joseph Saxton about 1855; it was re-constructed about 10 years ago by Fauth & Co., of Washington, who have at their establishment a dividing engine for which they claim great accuracy.

Thus have the mechanicians for a century kept pace with the demands for accurate instruments.

* This is the same which Mr. Bird used in dividing his 8-feet mural quadrant and was presented to the Royal Society by Mr. Bird's executors.

A MEMOIR OF ELIAS LOOMIS.*

By H. A. Newton.

The President and Fellows of Yale College have requested that in this public place and manner, I should give an account of the life, scientific activity, and public services of our late colleague, Prof. Elias Loomis. It is a pleasure to perform the duty thus laid upon me. The hours of intercourse I have had with him, and his generous confidences, are precious treasures of my life. And I hope you will find it worth your while to have turned away from other thoughts for a single hour, to listen to the account of what, during near three score years of mature life, our colleague was doing for science, and through science for man.

Elias Loomis was born in the little hamlet of Willington, Connecticut, August 7, 1811. His father, the Rev. Hubbell Loomis, was pastor in that country parish from 1804 to 1828. He was a man possessed of considerable scholarship, of positive convictions, and of a willingness to follow at all hazards wherever truth and duty, as he conceived them, might lead. He had studied at Union College, in the class of 1799, though apparently he did not finish the college course with his class. He is enrolled with that class in Union College, and he also received, in 1812, the honorary degree of Master of Arts from Yale College. At a later date he went to Illinois, and there was instrumental in founding the institution which afterwards became Shurtleff College.

Although the boy inherited from his father a mathematical taste, yet his love for the languages also was shown at a very early age. At an age at which many bright boys are still struggling with the reading of English, he is reported to have been reading with ease the New Testament in the original Greek. He prepared for college almost entirely under the instruction of his father. He was, for a single winter only, at the Academy at Monson, Massachusetts. Owing in part to feeble health he was more disposed, in those early years, to keep to his books than to roam with other boys over the Willington hills. In later life

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* A memorial address, delivered in Osborn Hall (Yale College, New Haven, Connecticut), April 11, 1890. (From the *American Journal of Science*, June, 1890, vol. XXXIX, pp. 427–455.)

he frequently said that in his early days he never had a thought of asking what subjects he was most fond of, but studied what he was told to study.

At the age of 14 he was examined and was admitted to Yale College, but owing to feeble health he waited another year before actually entering a class. In college he appears to have been about equally proficient in all of the studies, taking a good rank as a scholar, and maintaining it through his college course. President Porter remembers well the retiring demeanor of the young student, and his concise and often monosyllabic expressions, peculiarities which he retained through life. During his junior and senior years he roomed with Alfred E. Perkins, whose bequest was the first large endowment of the college library. He graduated in 1830.

A few weeks before graduation he left New Haven and entered a school, Mount Hope Institute, near Baltimore, to teach mathematics, and he remained there for a year and a term. One of his classmates, the late Mr. Cone of Hartford, said that Mr. Loomis had intended to spend his life in teaching, and that it surprised him when he heard that his purpose was abandoned, and that Mr. Loomis had gone, in the autumn of 1831, to the Andover theological seminary with the distinct expectation of becoming a preacher. This new purpose was however again changed, when a year later, he was appointed tutor in Yale College. A vacancy in the tutorship in the May following (1833), and while not yet 22 years of age he returned to New Haven and entered upon the duties of the office. Here he remained for 3 years and one term. In the spring of 1836 he received the appointment to the chair of mathematics and natural philosophy in Western Reserve College, at Hudson, Ohio. He was allowed to spend the first year in Europe. He was therefore during the larger part of the year 1836–'37 in Paris attending the lectures of Biot, Poisson, Arago, Dulong, Pouillet, and others. He did not visit Germany because of want of money. A long series of letters written by him at this time appeared in the Ohio *Observer*, and the contrast between England and France as he saw them, and the same places as seen by the tourist to day is decidedly interesting.

He purchased in London and Paris apparatus for his professorship and the outfit for a small observatory, and in the autumn of 1837 began his labors at Hudson. Here he remained for 7 years, maintaining with unflagging perseverance both his work in teaching and his scientific labors. In judging of this work at Hudson we must remember that he was not with perfect surroundings. He was without an assistant and without the counsel and encouragement of associates in his own branches of science. The financial troubles which culminated in this country in 1837 were peculiarly severe upon the young and struggling college. Money was almost unknown in business circles in Ohio, trade being almost entirely in barter. In this way principally was paid so much of the promised salary of $600 per annum as was not in arrears. In one

of his letters he congratulates himself that all of his bills that were more than 2 years old had been paid. In another he says that there was not enough money in the college treasury to take him out of the state. When he left Hudson the college offered to pay at once the arrears of his salary by deeding to him some of its unimproved lands.

In 1844 he was offered, and he accepted, the office of professor of mathematics and natural philosophy in the university of New York. In this new position he undertook the preparation of a series of text books in the mathematics, and for some years a large part of the time which he could spare from his regular college work was given to the preparation of these books.

When Professor Henry resigned his professorship at Princeton in order to accept the office of Secretary of the Smithsonian Institution Professor Loomis was offered the vacant chair. He went to Princeton and remained there during 1 year, at the end of which he was induced to return again to his old place in the university of New York. Here he continued until 1860, when he was elected to the professorship in Yale College made vacant by the death of Professor Olmsted. For the last 29 years of his life he here labored for the college and for science, passing away on the 15th of August, 1889.

Let us look now in succession at the different lines of his activity during these 56 years,—4 here in the tutorship and in Europe, 7 at Hudson, Ohio, 16 in New York City and Princeton, and 29 in New Haven.

For the first year on returning from Andover to New Haven he was tutor in Latin, although it seems that he might, had he chosen it, have been tutor of mathematics. I believe that at the beginning his mind was not yet definitely turned toward the exact sciences. In his childhood he had taken specially to Greek. In college he was equally proficient in all of his studies. He is represented to have led his class at Andover in Hebrew, and now on entering the tutorship he chose to teach the Latin language and literature. During the second year he taught mathematics and the third year natural philosophy. His later success in scientific work was, I believe, in no small measure due to his earlier broad and thorough study of language.

I have made some inquiry in order to learn what it was that turned his attention and tastes toward science. One of his colleagues in the tutorship, the Rev. Dr. Davenport, says that he recollects very distinctly the first indication to his own mind that Tutor Loomis was turning his thoughts in this direction. The great meteoric shower of 1833 came early in the period of his tutorship, and the views of Professor Twining and Professor Olmsted about the astronomical character and origin of these interesting and mysterious bodies were a common topic of conversation among scientific men in the college, especially whenever Professor Olmsted was present. The tutors were accustomed to meet as a club from time to time in the tutors' rooms in turn, and Dr. Davenport well recollects the occasion when Tutor Loomis brought in

a globe and discussed before the club the new theories about these bodies. Up to this time Tutor Loomis had seemed to him to have given his thoughts and study to language rather than to science.

In January, 1834, there were constituted in the Connecticut Academy of Arts and Sciences twelve committees representing the several departments of knowledge, and Tutor Loomis was put on the committee on mathematics and natural philosophy. These are the only signs of scientific taste or activity which I have detected earlier than the autumn of 1834, after he had been a year and a term in the tutorship. From this time on to the end of his life he gave his time and energies to several subjects that are enough distinct one from the other to make it convenient to disregard a strictly chronological account of his labors and consider his work in each subject by itself.

A subject of which he early undertook the investigation was terrestrial magnetism. We often use the rhetorical phrase "True as the needle to the pole," but looked at carefully, the magnetic needle is anything but constant in direction. Like the weather vane on the steeple it is ever in motion, swinging back and forth, in motions minute and slow it is true, but still always swinging. It has fitfully irregular motions; it has motions with a daily period; motions with an annual period; and motions whose oscillations require centuries for completion.

The *daily* motions of the magnetic needle were those which Tutor Loomis first studied. At the beginning of the second year of his tutorship he set up by the north window of his room in North College a heavy wooden block, and on it the variation compass that belongs to the college. Here for over thirteen months he observed the position of the needle at hourly intervals in the daytime, his observations usually being for seventeen successive hours of each day.

The results of these observations, together with a special discussion of the extraordinary cases of disturbance, were published in the *American Journal of Science* in 1836. No similar observations of the kind made in this country had at that time been published. So far as I am aware, none made before 1834 have since been published, except ten days' observations made by Professor Bache in 1832. In fact I know of only one or two like series of hourly observations made in Europe earlier than these by Tutor Loomis. He also at this time formed the purpose of collecting all the observations of magnetic declination that had been hitherto made in the United States and of constructing from them a magnetic chart of the country. He appealed successfully to the Connecticut Academy of Arts and Sciences for its sympathy and aid. The work of collecting facts was so far advanced before leaving New Haven that when he had been a few months professor at Hudson he forwarded to the *American Journal of Science* a discussion of the observations thus far obtained, and with them a map of the United States, with the lines of equal deviation of the needle drawn upon it. Two

years later he published additional observations and a revised edition of this map.

These were the first published magnetic charts of the United States, and though the materials for their construction were not numerous, and in many cases those obtainable were not entirely trustworthy, yet 16 years later, when a map was made by the United States Coast Survey from later and more numerous data, Professor Bache declared that between his own new map and that of Professor Loomis, when proper allowance had been made for the secular changes, the "*agreement was remarkable.*"

The northern end of a perfectly balanced magnetic needle turns downward, and the angle it makes with the horizon is called the magnetic *dip*. This angle is an important one, and is observed with accuracy only by using an expensive instrument, and taking unusual pains in observing. Hence only a few observations of this element were found by Professor Loomis. From these however he ventured to put on his first magnetic map a few lines that exhibited the amount of the *dip*.

While he was in Europe he purchased a first-class dipping needle for Western Reserve College, and at Hudson and the neighborhood in term time, and at other places in vacation, he made observations with this needle. Some of these observations were made before his second magnetic chart was published, and upon this map were now given tolerably good positions of the lines of equal magnetic dip. But he continued his observations for several years, determining the dip at over seventy stations, spread over thirteen States, each determination being the mean of from 160 to over 4,000 readings. These observations were published in several successive papers in the transactions of the American Philosophical Society at Philadelphia.

Various papers on terrestrial magnetism, in continuation of his earlier investigations, appeared in 1842, in 1844, in 1847, and in 1859, but movements in Germany, England, and Russia had meanwhile been inaugurated, which led to the establishment by governments of a score of well-equipped magnetic observatories, and this subject passed largely out of private hands.

Closely connected with terrestrial magnetism, and to be considered with it, is the *aurora borealis*. In the week that covered the end of August and the beginning of September, 1859, there occurred an exceedingly brilliant display of the northern lights. Believing that an exhaustive discussion of a single aurora promised to do more for the promotion of science than an imperfect study of an indefinite number of them, Professor Loomis undertook at once to collect and to collate accounts of this display. A large number of such accounts were secured from North America, from Europe, from Asia, and from places in the Southern Hemisphere; especially all the reports from the Smithsonian observers and correspondents were placed in his hands by the secretary, Professor Henry.

These observations and the discussions of them were given to the public during the following 2 years in a series of nine papers in the *American Journal of Science.*

Few (if any) displays on record were so remarkable as was this one for brilliancy and for geographical extent. Certainly about no aurora have there been collected so many facts. The display continued for a week. The luminous region entirely encircled the north pole of the earth. It extended on this continent on the 2d of September as far south as Cuba and to an unknown distance to the north. In altitude the bases of the columns of light were about 50 miles above the earth's surface, and the streamers shot up at times to a height of 500 miles. Thus over a broad belt on both continents this large region above the lower atmosphere was filled with masses of luminous material. A display similar to this, and possibly of equal brilliancy, was at the same time witnessed in the Southern Hemisphere.

The nine papers were mainly devoted to the statements of observers. Professor Loomis however went on to collect facts about other auroras, and to make inductions from the whole of the material thus brought together. He showed that there was good reason for believing that not only was this display represented by a corresponding one in the Southern Hemisphere, but that all remarkable displays in either hemisphere are accompanied by corresponding ones in the other.

He showed also that all the principal phenomena of electricity were developed during the auroral display of 1859; that light was developed in passing from one conductor to another, that heat in poor conductors, that the peculiar electric shock to the animal system, the excitement of magnetism in irons, the deflection of the magnetic needle, the decomposition of chemical solutions, each and all were produced during the auroral storm, and evidently by its agency. There were also in America effects upon the telegraph that were entirely consistent with the assumption previously made by Walker for England, that currents of electricity moved from northeast to southwest across the country. From the observations of the motion of auroral beams, he showed that they also moved from north-northeast to south-southeast, there being thus a general correspondence in motion between the electrical currents and the motion of the beams.

When there is a special magnetic disturbance at any place, there is usually a similar one at all other neighboring places. But these disturbances do not occur at the several places at the same instant of time. Professor Loomis showed that in the United States they take place in succession as we go from northeast to southeast, the velocity of the wave of disturbance being over 100 miles per minute. The waves of magnetic irregularities were thus connected with the electrical current and with the drifting motions of the streamers in the auroral display.

As incident to this discussion, he collected all available observations of auroras, and he deduced from them the annual number of auroras

visible at each place of observation. These numbers, when written upon a chart of the Northern Hemisphere, showed that auroras were by no means equally distributed over the earth's surface. It was found that the region in which they occurred most frequently was a belt or zone of moderate breadth and of oval form, inclosing the North Pole of the earth, and also the North Magnetic Pole. It was therefore much farther south in the Western Hemisphere than in the Eastern. Along the central line of this belt there are more than eighty auroras annually, but on going either north or south from the central line of that belt the number diminishes.

In 1870, Professor Loomis published a paper of importance relating to terrestrial magnetism, in which he showed its connection and that of the aurora with spots on the sun. That the spots on the sun had periods of maximum and minimum development had long been known. Lamont had noticed a periodicity in the magnetic diurnal variations. Sabine and Wolf and Gauthier had noticed that the two periodicities were allied. The connection of the period of solar spots with conjunction and opposition of certain planets had been shown by De La Rue and Stewart. Professor Loomis undertook an exhaustive examination of the facts that tended to confirm or refute the propositions that had been advanced. He confirmed and added to the conclusions of Messrs. De La Rue and Stewart. He also brought together such facts as were relevant to the question, and he showed that the regular diurnal variation of the magnetic needle was entirely independent of the solar spots, but that those disturbances that were excessive in amount were almost exactly proportional to the spotted surface of the sun. He also showed that great disturbances of the earth's magnetism are accompanied by unusual disturbances on the sun's surface on the very day of the storm.

Various forms of periodicity in the aurora have frequently been suggested. Professor Loomis, from all available accounts of the aurora, was able to show that while in the center of the zone of greatest auroral frequency auroras might be visible nearly every night, and hence that periodicity could not easily be shown by means of numbers of auroras recorded in such places, yet that such periodicity was distinctly traceable at places where the average number seen was about twenty or twenty-five a year. The times of maxima and minima of the solar spots were seen to correspond in a remarkable manner with the maxima and minima in the frequency of auroral displays in these middle latitudes. Also from the daily observations made by Messrs. Herrick and Bradley at New Haven during 17 years, he concluded that auroral displays in the middle latitudes of America are generally accompanied by an unusual disturbance of the sun's surface on the very day of the aurora. The magnetism of the earth, the aurora borealis, and the spots on the sun, have thus all three a casual connection, and apparently that connection is closely related to the conjunctions and oppositions of certain planets.

Shortly after the publication of this memoir, Professor Lovering published his extensive catalogue of auroras. A further discussion of the periodicity of the auroras was undertaken by Professor Loomis and published in 1873. In this he made use of all the auroras recorded in Professor Lovering's catalogue. They confirmed his previous conclusions, only slight modifications being required by the new facts presented, and by their more systematic collation.

In these papers, as in most of his papers upon other subjects, Professor Loomis was ever intent upon answering the questions: What are the laws of nature? What do the phenomena teach us? To establish laws which had been already formulated by others, but which still needed confirmation, was to him equally important with the formulation and proof of laws entirely new.

Let us now turn to another important line of Professor Loomis's work—astronomy. As I have said, he was early interested in the shooting stars. In October, 1834, he read a paper before the Connecticut Academy of Arts and Sciences upon this subject, probably in substance that which was shortly afterward published in the *American Journal of Science.* The published paper is principally a re-statement of the observations made in Germany in 1823, by Brandes in concert with his pupils for determining the paths of the stars through the atmosphere, together with methods of computation. From the results of Brandes's observations, however, he deduces an argument for the cosmic character of the shooting stars. One month after reading this paper to the Connecticut Academy he engaged in similar concerted observations with Professor Twining, who was then residing near West Point, New York. These were only moderately successful, but they were the first observations of the kind undertaken in America.

During the senior year of his college course there arrived at New Haven the 5-inch telescope, given to the college by Mr. Sheldon Clark, constructed by Dolland. This instrument was much larger than any telescope then in the country. It was temporarily placed in the Atheneum tower, where it was mounted on castors and wheeled to the windows for use. This temporary abode it occupied however for over 30 years. In spite of its miserable location it was, in the decade following its installment, a power in the development of the study of astronomy in the college. The lives and works of Barnard, and Loomis, and Mason, and Herrick, and Lyman, and Chauvenet, and Hubbard, and of other graduates of the college prove this. What rich returns for Mr. Sheldon Clark's $1,200 investment!

In 1835, the return of Halley's comet had been predicted, and its appearance was eagerly expected by astronomers and the public; Professor Olmsted and Tutor Loomis first in this country caught sight of the stranger, and throughout its course they noted its physical appearances. With such means as he had at command, Mr. Loomis observed the body's place, and computed from his observations the orbit.

The latitude and longitude of an observatory are constants to be early determined. These were measured by President Day for Yale College in 1811. In the summer of 1835, Tutor Loomis, with such instruments as the college possessed, a sextant and a small portable transit, made numerous observations of Polaris for latitude, and several moon culminations for longitude. From these he computed the latitude and longitude of the Atheneum tower. The longitude from Greenwich, though obtained from a small number of observations, differs less than 2 seconds of time from our best determinations to-day.

While in Europe in 1836–'37, Professor Loomis, as I have said, bought for Western Reserve College the instruments for an observatory. These were a 4-inch equatorial, a transit instrument, and an astronomical clock. On his return he erected, in 1837, a small observatory at Hudson, and in September, 1838, began to use the instruments. He had no assistant, and by day had a full allotment of college work. Two hundred and sixty moon culminations and sixteen occultations observed for longitude, sixty-nine culminations of Polaris for latitude, along with observations on five comets sufficiently extended for a computation of their orbits; these attested his activity outside of his required duties. Some years later, when the corresponding European observations were made public, he prepared an elaborate discussion of these longitude observations, and published in it *Gould's Astronomical Journal.* A sixth comet was observed by him at Hudson in 1850.

It may not seem a very large output of work in six years' time to have determined the location of the observatory, and to have observed five comets. But we must recollect that the telegraph had not then been invented, that the exact determination of the longitude of a single point in the western country had a higher value then than it can have now, and that it could be obtained only by slow and tedious methods. These were moreover days of small things in astronomy in this country. At Yale College we had a telescope but not an observatory. At Williamstown an observatory had been constructed, but it was used for instruction, not for original work. At Washington Lieutenant Gilliss, and at Dorchester Mr. Bond, were commissioned by the Government in 1838 to observe moon culminations in correspondence with the observers in the Wilkes exploring expedition for determining their longitude. These two prospective sets of observations, both of them under Government auspices and pay, were the only signs of systematic astronomical activity in the United States outside of Hudson, when in 1838 Professor Loomis began his observing there. In his inaugural address he asks: "Where now is our American observatory? Where throughout this rich and powerful nation do you find a single spot where astronomical observations are regularly and systematically made? There is no such spot." When he left Hudson in 1844, the situation was not largely changed. Mr. Bond had removed his instruments and work to Cambridge. The High School Observatory at Philadelphia had been erected

and Messrs. Walker and Kendall were using its instruments. Professor Bartlett had built the observatory at West Point, and had begun to observe there. Lieutenant Gilliss after years of excellent work in the little establishment on Capitol Hill had just finished the present Naval Observatory building at Washington, Professor Mitchell had begun to build the Cincinnati Observatory, and the Georgetown observatory building had been erected. Professor Loomis's work at Hudson should be measured by what others were doing at the time, rather than by the larger performance of to-day.

In the summer of 1844, the year in which Professor Loomis came to New York, a new method in astronomy had its first beginnings. The telegraph line had just been built between Baltimore and Washington, and Captain Wilkes at Baltimore compared his chronometer by telegraph with one at Washington, and so determined the difference of longitude of the two places.

Professor Bach was now Superintendent of the Coast Survey, and he determined at once to use the new method for the purposes of the survey. To Mr. Sears C. Walker was committed the direction of the work, but scarcely less important were the services of Professor Loomis, who for three campaigns had charge of the end of the lines in Jersey City and New York. Their first partially successful efforts were made in 1846, but the practical difficulties were overcome and entire success was obtained by them in 1847 and 1848. In these years the differences of longitude of Washington, Philadelphia, New York, and Cambridge were thus determined with an accuracy far greater than any previous similar determination whatsoever.

The next summer, that of 1849, Professor Loomis assisted in a like work to connect Hudson, Ohio, with the eastern stations. His observations of moon culminations at Hudson were thus available equally with those made at Philadelphia, Washington, Dorchester, and Cambridge for determining the absolute longitudes of Atlantic stations from Greenwich. It was not until 1852, that Europeon astronomers began to use these telegraphic methods in measuring longitudes.

In 1850, Professor Loomis published a volume on the "Recent progress of astronomy, especially in the United States." A first and a second edition was soon exhausted, and in 1856, the volume was entirely re-written and very much enlarged. Some of the topics in these volumes were the subjects of articles communicated from time to time to the public in the *American Journal of Science*, *Harper's Magazine* and other periodicals. Another important contribution to astronomy appeared in 1865, that is, his "Introduction to practical astronomy." Eminent astronomers in England and America have expressed in the highest terms their praise of this book. Though it is now 35 years since its first appearance, and many treatises on the same subject, some elaborate and some elementary, have since been published, yet for an introduction to practical work I believe that a student will find this volume better than any other for his uses at the beginning of his course.

The increase of our knowledge in astronomy was, from first to last, an object of special interest to Professor Loomis. Before he left New York, the income from his text books enabled him to make to Yale College the generous offer of coming to New Haven and working in an observatory at his own charges, provided a suitable observatory should be constructed and equipped for him. Unfortunately, the college was not able, although it was greatly desirous of doing it, to avail itself of his generous offer. Near the same time he joined with public spirited citizens of New York in an effort to establish an astronomical observatory in or near that city, and for that purpose an act of incorporation was obtained from the New York State legislature. After coming to New Haven, he always took the warmest interest in the plans of Mr. Winchester for the establishment of an observatory in connection with Yale University. His counsel and assistance have been instrumental, more than the public could know, in producing and preserving whatever of value has been developed in that observatory.

The science of meteorology has however been that in which Professor Loomis has made the most important contributions to human knowledge.

Shortly after his graduation in 1830, and before he entered upon the tutorship, there appeared the first of a long series of papers by Mr. Redfield, of New York City, upon the theory of storms. In the last year of his tutorship there appeared also the first of a like remarkable series of papers on the same subject by Professor Espy, of Philadelphia. Two rival theories were advocated by these two men, and these theories became the subject of no little discussion in scientific meetings, and in scientific journals, for a long period of years. Professor Loomis had, from their very inception, taken a warm interest in these discussions, and the subject of meteorology, and in particular its central problem the theory of storms, held in his thought and work the first place from that time to the day of his death.

In his visit to Europe (the year before he went to Hudson), he purchased a set of meteorological instruments, and for several years in Hudson he steadily performed the naturally irksome task of making twice each day a complete set of meteorological observations. A few weeks after he entered upon his professorship in Hudson a tornado passed 5 miles from that place, and he went out immediately to examine the track and learn what facts he could that should bear upon the theory of the tornado. The results were valuable, but he was not altogether satisfied with them. They led him however to undertake the discussion of one of the large storms that covered the whole United States.

For this purpose he selected the storm which had occurred near the 20th of December, 1836. Sir John Herschel had recommended that hourly observations be taken by all meteorological observers on four term days in the year, that is, observations for thirty-six successive

hours at each equinox and each solstice. This storm fell partly upon one of these term days. Professor Loomis set to work to collect all the meteorological observations made during the week of the storm that he could obtain from all parts of the United States, and from some stations in Canada. The discussion resulting therefrom was read in March, 1840, before the American Philosophical Society at Philadelphia.

Let us for a little while consider the amount of knowledge of the facts about storms in our possession in 1840, the date when this memoir was read and an abstract of it published in Philadelphia. Franklin had noted the motion of storms from southwest to northeast. He said:* "Our northeast storms in North America begin first in point of time in the southwest parts, that is to say, the air in Georgia, the farthest of our colonies to the southwest, begins to move southwesterly before the air of Carolina, which is the next colony northeastward; the air of Carolina has the same motion before the air of Virginia, which lies still more northeastward; and so on northeasterly through Pennsylvania, New York, New England, etc., quite to Newfoundland." Redfield had traced several storms along the West India Islands northwesterly until about in the latitude of 30° their course was turned quite abruptly and they swept off northeasterly along the Atlantic coast toward and even past Newfoundland. Espy found some storms moving easterly or south of east from the Mississippi to the Atlantic.

Brandes had announced as a law that the wind in storms blows inward toward a center, but his law was an induction from a small number of observations. Dove had contended for a whirling motion. Redfield advanced facts to show that the winds blew in circles anti-clockwise around a center that advanced in the direction of the prevalent winds, and with him agreed Reid, Piddington, and others. Espy, agreeing with Brandes, claimed that the observations in the various storms showed a centripetal motion of the winds toward a center if the region covered by the storm was round, and toward a central line if the storm region was longer in one direction than in another. Espy's conclusions were intimately connected with his theory that in the center of the storm there was an upward motion of the air, and that the condensation of vapor into rain furnished the energy needed for the continuation of the storm. The rival theories of Redfield and Espy were in sharp contest on several points, but the main contention was around this central question: Do the winds blow in circular whirls or do they blow in toward a center? New York State was collecting observations from the academies. The American Philosophical Society and the Franklin Institute, aided by an appropriation from the State of Pennsylvania, had united in an effort to learn the facts and the true theory of storms.

Under such circumstances the thorough discussion of a single violent storm was likely to add materially to our knowledge. The treatment

*Letter to Alexander Small, May 12, 1760.

of this storm by Professor Loomis was probably more complete than that of any previous one, and the methods which he employed were better fitted to elicit the truth than any earlier methods. But the storm was a very large one, extending from the Gulf of Mexico to an unknown distance north, and having its center apparently to the north of all the observers. The results which he was able to secure did not sustain either of the two rival theories, but rather tended to prove some features in each of them. Professor Loomis was not himself satisfied with them, and he therefore waited for another storm that should be better fitted for examination.

In the month of February, 1842, a second tornado passed over northeastern Ohio, and Professor Loomis with one of his colleagues again started out for the examination of the track. The tornado passed over a piece of woods, and hence the positions of the prostrate trees showed clearly the motion of the wind in the passing tornado and threw much light upon the character of this kind of storm. But the tornado was a single feature of a large storm that covered the whole country, and a second storm of great intensity was also experienced in the same month.

The discussion of these two storms was now undertaken by him. The paper giving the results of that discussion was sent to Professor Bache and read by him at the centennial meeting of the American Philosophical Society in May, 1843, and created, as Professor Bache wrote, a great sensation. It was at the time important for the light which it threw upon the rival contending theories of Espy and of Redfield, but it was more important by far by reason of the new method of investigation then for the first time employed.

In the paper upon the storm of 1836, Professor Loomis had made some advance upon previous methods of representing the facts about storms. But even the method he then used was entirely unfitted to give answers to the questions which meteorologists were asking. Some of those questions were stated in circulars issued by the joint committee of the American Philosophical Society and the Franklin Institute: What are the phases of the great storms of rain and snow that traverse our continent; what their shape and size; in what direction and with what velocity do their centers move along the surface of the earth; are they round or oblong or irregular in shape; do they move in different directions in different seasons of the year?

The graphic representation by Professor Loomis on the map of the United States of the storm of 1836, had been a series of lines drawn joining the places where at a given hour the barometer was at its lowest point. That line would, so far as the barometer was concerned, mark for that hour the central line of the storm. The progress of the line from hour to hour on the map showed, quite imperfectly, how the storm had traveled. Some arrows added showed to the eye also certain facts about the movements of the air.

Professor Espy adopted—and thereafter adhered to—a modification of this method of representing storm phenomena, and I think meteorologists will agree with me in my opinion that Professor Espy's four reports from 1842 to 1854, though they contained an immense accumulation of facts, were because of this radical defect of presentation almost useless to meteorological science.

In the discussion of the storms of 1842, instead of the line of minimum depression of the barometer, Professor Loomis drew on the map a series of lines of equal barometric pressure, or rather of equal deviations from the normal average pressure for each place. A series of maps representing the storm at successive intervals of twelve hours were thus constructed, upon each of which was drawn a line through all places where the barometer stood at its normal or average height. A second line was drawn through all places where the barometer stood 0.2 of an inch below the normal, and other lines through points where the barometer was 0.4 below, 0.6 below, 0.8 below, etc.; also lines were drawn through those points where the barometer stood 0.2, 0.4, 0.6, etc., above its normal height. The deviations of the barometric pressure from the normal were thus made prominent, and all other phenomena of the storm were regarded as related to those barometric lines. A series of colors represented respectively the places where the sky was clear, where the sky was overcast, and where rain or snow was falling. A series of lines represented the places at which the temperature was at the normal, or was 10 or 20 or 30 degrees above the normal, or below the normal. Arrows of proper direction and length represented the direction and the intensity of the winds at the different stations. These successive maps for the three or four days of the storm furnished to the eye all its phenomena in a simple and most effective manner.

You have no doubt, most of you, already recognized in this description the charts, which to-day are so common, issued by the United States Signal Service, and by weather-service bureaus in other countries. The method seems so natural, that it should occur to any person who has the subject of a storm under consideration. But the greatest inventions are oft-times the simplest, and I am inclined to believe that the introduction of this single method of representing and discussing the phenomena of a storm was the greatest of the services which our colleague rendered to science. This method is at the foundation of what is sometimes called "the new meteorology," and the paper which contains its first presentation stands forth, I am convinced, as the most important paper in the history of that science. I regret that I can not aid my memory by quoting the exact words, but I remember distinctly what seemed to me an almost despairing expression made many years ago by one who had high responsibility in the matter of meteorological work, as he looked out upon the confused mass of observations already made, and felt unable to say in what direction progress was to be expected. With this I contrast the buoyant expressions of another officer

charged with like responsibility, as he showed me, one or two decades later (in 1869), charts constructed like those of Professor Loomis, and said, "I care not for the mass of observations made in the usual form. What I want is the power and the material for making such charts as these." These two expressions of Sir George Airy and of LeVerrier mark the progress and the direction of progress in meteorology developed by Professor Loomis's memoir.

What was his own judgment of the method at the time of its publication and its value in meteorology can be seen from his words at the close of the memoir, which I beg permission to quote:

"It appears to me that if the course of investigations adopted with respect to the two storms of February, 1842, was systematically pursued we should soon have some settled principles in meteorology. If we could be furnished with two meteorological charts of the United States daily for one year, charts showing the state of the barometer, thermometer, winds, sky, etc., for every part of the country it would settle forever the laws of storms. No false theory could stand against such an array of testimony. Such a set of maps would be worth more than all which has been hitherto done in meteorology. Moreover the subject would be well-nigh exhausted. But one year's observation would be needed. The storms of one year are probably but a repetition of those of the preceding. Instead then of the guerrilla warfare, which has been maintained for centuries with indifferent success, although at the expense of great self-devotion on the part of individual chiefs, is it not time to embark in a general meteorological crusade? A well-arranged system of observations spread over the country would accomplish more in one year than observations at a few isolated posts, however accurate and complete, continued to the end of time. The United States are favorably situated for such an enterprise. Observations spread over a smaller territory would be inadequate, as they would not show the extent of any large storm. If we take a survey of the entire globe we shall search in vain for more than one equal area which could be occupied by the same number of trusty observers. In Europe there is opportunity for a like organization, but with this incumbrance, that it must needs embrace several nations of different languages and governments. The United States then afford decidedly the most hopeful field for such an enterprise. Shall we hesitate to embark in it; or shall we grope timidly along as in former years? There are but few questions of science which can be prosecuted in this country to the same advantage as in Europe. Here is one where the advantage is in our favor. Would it not be wise to devote our main strength to the reduction of this fortress? We need observers spread over the entire country at distances from each other not more than 50 miles. This would require five or six hundred observers for the United States. About half this number of registers are now kept in one shape or another, and the number by suitable efforts might probably be doubled. Supervision is needed to in-

troduce uniformity throughout and to render some of the registers more complete. Is not such an enterprise worthy of the American Philosophical Society? The General Government has for more than 20 years done something and has lately manifested a disposition to do more for this object. If private zeal could be more generally enlisted the war might soon be ended and men would cease to ridicule the idea of our being able to predict an approaching storm."

This plan of a systematic meteorological campaign was cordially seconded by Professors Bache and Peirce. At a somewhat later date the American Academy of Sciences, of Boston, appointed a committee, of which Professor Loomis was chairman, to urge upon the proper authorities the execution of the plan. The American Philosophical Society, of Philadelphia, united its voice with that of the Academy. About this time Professor Henry was made Secretary of the Smithsonian Institution. He determined to make American meteorology one of the leading subjects of investigation to be aided by the Institution. At Professor Henry's request, Professor Loomis prepared a report upon the meteorology of the United States, in which he showed what advantages society might expect from the study of the pheuomena of storms; what had been done in this country toward making the necessary observations and toward deducing from them general laws; and finally, what encouragement there was to a further prosecution of the same researches. He then presented in detail a practicable plan for securing the hoped-for advantages in their fullest extent.

This plan looked to a unifying of all the work done by existing observers, a systematic supervision, a supplementing of it by new observers at needed points, a securing of the coöperation of the British Government and the Hudson's Bay Company in the regions to the north of us, and finally a thorough discussion of the observations collected. A siege of 3 years was contemplated. In the history of the several steps that finally led to the establishment of the United States Signal Service this report has an important place.

The scheme laid down by Professor Loomis was in part followed out by the Institution, but the fragmentary character of the observations, the want of systematic distribution of the places of the observers, and the imperfections of the barometers made the material collected difficult of discussion. Professor Loomis waited in hopes of some better system.

In 1854, Professor Loomis undertook a re-discussion of the storm of 1836, using the new methods introduced for treating the storms of 1842. A visit to Europe shortly after enabled him to collect a large number of observations upon a storm or series of storms that occurred in Europe about a week later than that American storm. He had long been anxious to connect, if possible, these two storms, as he said, "stepping across the Atlantic." The European and the American storms however not only proved to be distinct one from the other, but the discussion showed clearly that many of the laws of American storms were

radically different from those of the European storms. The results of the whole discussion were published in 1859, by the Smithsonian Institution.

Upon coming to New Haven, in 1860, he commenced the collection of all the meteorological observations that had been made in New Haven and the immediate vicinity, and succeeded in finding sets which, when brought together, made up a nearly continuous record through 86 years. The results of these observations formed the subject of a memoir published by the Connecticut Academy of Arts and Sciences in 1866.

It became part of his duties in college to deliver a course of lectures upon the subject of meteorology. In preparation for these he caused to be printed in very limited numbers the outlines of a treatise upon meteorology, to be used as the basis of his series of lectures. In 1868 he developed this outline into a treatise suited to use in college classes and in private study. This treatise, notwithstanding the rapid advances of the science during more than 20 years, is still indispensable to the student of meteorology.

The better system of observing for which Professor Loomis had been long waiting came when the United States Signal Service was established in 1871. The daily maps of the weather published by the Bureau were constructed essentially after the plan which Professor Loomis had, 30 years before, invented for the treatment of the storms of 1842. As soon as these maps had been published for the two years 1872 and 1873, Professor Loomis commenced in earnest to deduce from them the lessons which they taught us respecting the nature and the phenomena of United States storms. To this investigation he gave nearly all his energies during the remaining 15 years of his life.

For several years he employed and paid for the services of assistants whose time was given to the preparation of material for use in his studies. The aggregate cost of this assistance was of itself a very large contribution to science. Beginning in April, 1874, he presented regularly at eighteen successive meetings of the National Academy of Sciences in April and in October of each year, a paper entitled "Contributions to Meteorology." These were at first based upon the publications of the Signal Service alone, but as years went by, like publications appeared in Europe that were useful for his work. These papers were published in July and January following the Academy meeting, and they regularly formed the first and leading article in eighteen successive volumes of the *American Journal of Science*. Gradually one after another of his college duties were committed to others that he might give his whole strength to these investigations.

An attack of malaria interrupted the regularity of the series. His advancing years and diminishing strength warned him that the end of his investigations could not be far distant. The number of hours in which he could work each day was slowly diminishing. Five more papers followed at somewhat less regular intervals.

In 1884, he began a revision of the whole series of papers. They had been presented without much regard to systematic order in the subjects investigated, and new material had accumulated from time to time, so that a thorough, systematic revison seemed absolutely necessary.

In 1885, he presented to the Academy of Sciences the first chapter of this revision, in which he discussed the areas of low pressure—their form, their size, their motions, and the phenomena attending them. Two years later, in 1887, the second chapter of the revision appeared, in which he discussed the areas of high pressure, their form, magnitude, direction, and velocity of movement, and their relation to areas of low pressure. Gradually his physical strength was failing, though his mind was as bright and clear as ever. To this work, the only work which he was now doing, he was able to give 2 or 3 hours a day. Anxiously he husbanded his strength, slowly and painfully preparing the diagrams and the table for the third chapter upon rain areas, the phenomena of rain-fall in its connection with areas of low pressure, and the varied phenomena of unusual rain-fall. "I see," he said to a friend, "not the end of this subject, but where I must stop. I hope I shall have strength to finish this work, and then I shall be ready to die."

This third and finishing chapter was finally passed through the printer's hands, and some advance copies distributed to correspondents abroad in the summer months of 1889. His work upon the theory of storms he felt was finished. As he paid the bill of the printer, he said to him: "When I return at the close of the vacation I expect to put into your hands for printing a new edition of the *Loomis Genealogy*." Before the close of the vacation he died.

These three chapters of his revised edition of "Contributions to Meteorology," constitute the full and ripe fruitage of his work in his favorite science. They will for a long time to come be the basis of facts by which writers in theoretical meteorology must test their formulas. They cover all the important points taken up in the twenty-three earlier memoirs with one important exception,—the relation of mountain observations to those made on the plains below. The laws connecting these two are not yet clearly indicated; much remains to be learned about them, and they are of the utmost importance in theoretical meteorology. He felt most deeply the backward steps taken by the United States Signal Service when mountain observations and the publication of the *International Bulletin* were discontinued. "The National Academy of Sciences," he said, "ought at once to take up the subject and use all its influence to secure the restoration of these two services.

Professor Loomis at various times studied certain other questions in physics and astronomy that were more or less allied with the subjects to which he gave the principal part of his time, and he published the results of his studies. He made a series of experiments on currents of electricity generated by a plate of zinc buried in the earth. He examined the electrical phenomena in certain houses in

New York; the curious phenomena of optical moving figures; the vibrations sent out from waterfalls as the water flows over certain dams; the orbits of the satellites of Uranus; the temperature of the planets; the variations of light of the stars η Argus and Algol; and the comet of 1861.

The subject of family *genealogy* has a peculiar fascination to many minds. It would be an interesting study to determine practically by a collection of facts what are the elements in a man's character which lead him to engage in this peculiar study. Certain it is that men of most diverse disposition are led into it. I should not have thought it likely that Professor Loomis would have taken up the subject very seriously. Others have expressed to me the same thought, and he himself says that he did not think it strange that others should be surprised at his devoting so much time to this subject, for he was surprised at it himself. He became interested in the subject early in life, and that interest remained unbroken to his last days. For nearly forty years before his first publication he collected from time time to materials for a list of the descendants of his ancestor, Joseph Loomis, who came from Braintree, England, in the year 1638, and settled in Windsor, Connecticut, in 1639. In each of his four visits to Europe he extended his inquiries to his ancestor's earlier history in England. The materials thus collected were put in type in 1870. He published a list containing 4,340 descendants of Joseph Loomis bearing the Loomis name. He regarded it as entirely provisional, printed to help himself in making further researches, and to excite interest in others of the name, who would thus be led to give additional information, or correction of errors.

Finding that to a limited extent only could he hope by correspondence to gain the information desired, he now undertook in his vacations to canvass the country by personal visits. He collected lists of names from every available source, from catalogues of every description, from city directories, county directories, county maps, and county tax lists, and he compiled from these sources lists of all the Loomis names he could find. Arranging these names by counties, he undertook to visit each family personally. In this way he made a pretty thorough canvass of every part of New England and New York State, of nearly every part of New Jersey and Pennsylvania, of the northern part of Ohio, and of some of the western cities.

After five years of these researches he published the second edition of the "Loomis Genealogy," in which were given 8,686 names of persons that bore the Loomis name, descendants of Joseph Loomis in the male branches.

Five year later, in 1880, Professor Loomis printed in two additional volumes a provisional list of 19,000 descendants of Joseph Loomis in the female branches. Large as was this list, he did not regard it as more than a first outline of a census of the descendants of the original

emigrant, and he hoped in the near future to publish an additional volume. For this he has left in manuscript many corrections and large additions that will be of use to the future Loomis genealogist.

Am I tarrying too long upon the vacation work of Professor Loomis? If so, I plead on this occasion that among these direct descendants of Joseph Loomis there were enrolled more than two hundred graduates of Yale College, and nearly one hundred more of our graduates have married members of this numerous family.

Professor Loomis was doubtless more widely known as the author of mathematical text-books than as a worker in new fields in science. Shortly after coming to New York, he prepared a text-book in algebra. The market was ready for a good book of this kind, and the work prepared for it was a good one. It was followed the next year by a Geometry. This was an attempt, and if judged by its reception and sale it was a successful attempt, to combine in a school book the rigid demonstrations of Euclid with the courses of thought in Legendre and in modern science. The task is one of peculiar difficulty, as the existence and activities of the English Society for the Improvement of Geometric Teaching now for near twenty years illustrates. Other books followed the Geometry from year to year, the whole forming a connected series from arithmetic upward, so that the list of his works finally numbered near twenty volumes. His experience in teaching, his rare skill in language, his clear conception of what was important, and his unwearied painstaking, combined to produce text-books which met the wants of teachers. About 600,000 volumes have been sold, benefiting the schools and colleges, and bringing to the author a liberal and well-merited pecuniary return.

We ought not to omit—on this academic occasion—to speak of the teacher. College graduates who have been under his instruction will probably retain a more positive impression of the personal traits and the character of Professor Loomis than of most of their other teachers. His crisp sentences, lucid thought, exactness of language, and steadiness of requirement, more than made up for any apparent coldness and real reserve. These characteristics of his riper years were peculiar to him from the beginning of his life as a teacher. During his tutorship he was thought to be strict as a disciplinarian, and this may have unfavorably affected his influence with some members of the class of 1837, of which he was tutor. It was not so with all of them. Some of you will recall what was said by a member of that class as he came to commencement a few years since, occupying at the time the highest office which a lawyer in the line of his profession can in this country secure: "If I have been successful in life," said Chief-Justice Waite, "I owe that success to the influence of Tutor Loomis more than to any other cause whatever."

There was in Professor Loomis so much of reserve, that to many persons he seemed cold and without interest in the lives of others. But

this was mainly due to appearances only. The tear would at times come unbidden to his eye. His correspondence with his class-mates in the years immediately following graduation shows warm interest in all that concerned them. From Hudson he wrote often to Mr. Herrick, and complained much of isolation, but more especially of isolation from scientific companions and books.

In 1840, he married Miss Julia E. Upson, of Talmadge, Ohio, a lady about whom those who knew her have spoken to me only in terms of praise, and for whose memory Professor Loomis cherished a tender reverence. She died in 1854, leaving two sons. From this time Professor Loomis lived in apartments, surrounded by his books and devoted to his studies. His sons, after passing their school and college days, went to their own fields of work. During many years of his New Haven life he was unable to receive visitors in the evening. He made very few new friends, and one after another of his old ones passed away. To his work he was able to give undivided his time and his strength. His mind did not seem to require the excitement of social intercourse for its full and healthful activity. Isolated though he was there was in him no trace whatever of selfish or morbid feeling. In council his advice was always marked by his clear judgment of what was important, and at the same time what was practicable. Whatever he himself had the right to decide was promptly decided by a yes or no, and few persons cared to question the finality of his decision. But when his colleagues, or others, had the right to decide he accepted their decision without questioning or subsequent murmur. Upon being told that his letters to Mr. Herrick had come to the college library, and that he could, if he chose, examine them and see whether there were among them any which he would prefer not to leave in this quasi public place, he promptly replied: "No, I never wrote a letter which I should be ashamed to see published."

After coming to New York he had a generous income from his books, besides his salary as professor. The amount he saved from his income was carefully and prudently invested, and before his death the savings with their accumulations were a large estate; how large only he and his banker knew.

One of his college class-mates told me that Mr. Loomis left college with the definitely expressed purpose that the world should be better for his living in it. The central proposition in his inaugural address at Hudson in 1838 was: "That it is essential to the best interests of society that there should be a certain class of men devoted exclusively to the cultivation of abstract science without any regard to its practical applications; and consequently that such men instead of being thought a dead weight upon society, are to be ranked among the greatest benefactors of their race." He chose this for his principal work for man, and he steadily kept to the chosen work. To establish an astronomical observatory had been through life a cherished object. He entered

into and aided heartily the plans of Mr. Winchester, both before and after Mr. Winchester asked his trustees to transfer his magnificent endowment to the university. Professor Loomis looked forward to a large institution in the future on the observatory site. To endow this public service, after making liberal provision for his two sons, he bequeathed his estate. The income from more than $300,000 will eventually be available to continue the work of his life. With clear judgment of what was most important he limited the use of that income to the payment of salaries of persons whose time should be exclusively devoted to the making of observations for the promotion of the science of astronomy, or to the reduction of astronomical observations, and to defraying the expenses of publication. He knew that if he provided observers, other benefactors would furnish buildings and instruments, and the costs of supervision and maintenance.

A university has an organic life, with its past and its future. The wealth of a university consists mainly in its men; not so much in those men who are its active members now, as in those who have lived themselves into its life in the past, and have made it a home of scholarship, of truth, and of devotion to duty; a place fit for the development of the nobler elements of character. The life and work of Elias Loomis form no mean portion of the wealth of Yale University.

PUBLICATIONS OF ELIAS LOOMIS.

1. On shooting stars. Am. Jour. April, 1835. (1), vol. XXVIII, pp. 95–104.
2. Halley's comet (Olmsted and Loomis). New Haven Daily Herald, September 1, September 4, September 28, and December 31, 1835.
3. Halley's comet (Olmsted and Loomis). Am. Jour. October, 1835. (1), vol. XXIX, pp. 155, 156.
4. Observations on the comet of Halley, made at Yale College. Am. Jour. July, 1836. (1), vol. XXX, pp. 209–221.
5. Observations on the variation of the magnetic needle, made at Yale College in 1834 and 1835. Am. Jour. July, 1836. (1), vol. XXX, pp. 221–233. (Sturgeon's Ann. Electr., vol. 2, pp. 270–282.)
6. Letters from Europe. (Thirty-six letters.) Ohio Observer (1837).
7. Meteoric shower of November 13. Cleveland Observer, November, 1837.
8. Hourly meteorological observations for the December solstice of 1837, made at Western Reserve College. Cleveland Observer, December 23, 1837.
9. Hourly meteorological observations for the vernal equinox of 1838, made at Western Reserve College. Cleveland Observer, March, 1838.
10. Observations on a hurricane which passed over Stow, in Ohio, October 20, 1837. Am. Jour. January, 1838. (1), vol. XXXIII, pp. 368–376.
11. Splendid meteor (May 18, 1838). Cleveland Observer, May 22, 1838.
12. Hourly meteorological observations for the summer solstice of 1838, made at Western Reserve College. Cleveland Observer, June, 1838.
13. On the variation and dip of the magnetic needle in different parts of the United States. Am. Jour. July, 1838. (1), vol. XXXIV, pp. 290–309. (With a map.)
14. On the latitude and longitude of Yale College observatory. Am. Jour. July, 1838. (1), vol. XXXIV, pp. 309–313.
15. Meteors of August 9. Cleveland Observer, August 11, 1838.
16. An inaugural address, delivered August 21, 1838. 8vo., p. 38. New York, 1838.

17. Hourly meteorological observations for the autumnal equinox of 1838, made at Western Reserve College. Cleveland Observer, September, 1838. (Professor Loomis also observed on the term days in December, 1838, and in March and June, 1839.)
18. On the meteor of May 18, 1838, and on shooting stars in general. Am. Jour., January, 1839. (1), vol. III, pp. 223–232.
19. Meteorological table and register. Am. Jour., April, 1839: (1), vol. XXXVI, pp. 163–165.
20. Observations to determine the magnetic dip at various places in Ohio and Michigan. In a letter to Sears C. Walker, esq. Read June, 1839. Am. Phil. Soc. Trans., vol. 7, pp. 1–6. (Am. Phil Soc. Proc., vol. I, p. 116; Am. Jour. (1), vol. XXXVIII, p. 397.)
21. Astronomical observations made at Hudson Observatory, latitude 41° 14′ 42″.6 north, longitude 5^h 25^m 44^s.15 west, with some account of the building and instruments. (Latitude of observatory; moon culminations; occultations.) Read October, 1839. Am. Phil. Soc. Trans., vol. VII, pp. 43–51. (Am. Phil. Soc. Proc., vol. I, pp. 129, 130.)
22. Additional observation of the magnetic dip in the United States. Read October, 1839. Am. Phil. Soc. Trans., vol. VII, pp. 101–111. (Am. Phil. Soc. Proc., vol. I, pp. 144–145.)
23. On the storm which was experienced throughout the United States about the 20th of December, 1836. Read March, 1840. Am. Phil. Soc. Trans., vol. VII, pp. 125–163. (With three plates.) (Am. Phil. Soc. Proc., vol. I, pp. 195–198; Am. Jour. (1), vol. XL, pp. 34–37.)
24. Meteorological sketches. (Eight papers.) Ohio Observer, 1840.
25. On the variation and dip of the magnetic needle in the United States. Am. Jour., July, 1840: (1), vol. XXXIX, pp. 41–50. (With a map.)
26. Observations to determine the magnetic intensity at various places in the United States, with some additional observations of the magnetic dip. Read November, 1840. Am. Phil. Soc. Trans., vol. VIII, pp. 61–72. (Am. Phil. Soc. Proc., vol. II, pp. 308–310.)
27. On the magnetic dip in the United States. Am. Jour., January, 1841: (1), vol. XL. pp. 85–92. (Sturgeon's Ann. Electr., vol. VII, pp. 156–162.)
28. Meteorological observations made at Hudson, Ohio, latitude 41° 14′ 42″.6 north, longitude 5^h 25^m 44^s.15 west, during the years 1838, 1839, and 1840. (Barometer; thermometer and hygrometer; winds; rain.) Am. Jour., October, 1841: (1), vol. XLI, pp. 310–330.
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30. On the dip and variation of the magnetic needle in the United States. Am. Jour. April, 1842: (1), vol. XLIII, pp. 93–116.
31. Encke's comet. Ohio Observer, April, 1842.
32. On a tornado which passed over Mayfield, Ohio, February 4, 1842, with some notices of other tornadoes. Am. Jour., April, 1842: (1), vol. XLIII, pp. 278–300. (With a map.)
33. Supplementary observations on the storm which was experienced throughout the United States about the 20th of December, 1836. Read May, 1842. Am. Phil. Soc. Trans., vol. VIII, pp. 305, 306.
34. Observations on the magnetic dip in the United States. Read May, 1842. Am. Phil. Soc. Trans., vol. VIII, pp. 285–304. (Am. Phil. Soc. Proc., vol. II, pp. 176–178.)
35. Icebergs in the Atlantic. Ohio Observer, July 1842.
36. The comet. (Five papers, with orbit.) Ohio Observer, March, 1843.

37. On two storms which were experienced throughout the United States in the month of February, 1842. Read May, 1843. Am. Phil. Soc. Trans., vol. IX, pp. 161–184. (With 13 maps.) (Am. Phil. Soc. Proc., vol. III, pp. 50–56.)
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41. Comparison of Gauss's theory of terrestrial magnetism with observation. Am. Jour., October, 1844: (1), vol. XLVII, pp. 278–281.
42. Astronomical observations made at Hudson Observatory, latitude 41° 14′ 42″.6 north, and longitude $5^h\ 25^m\ 44^s.15$ west. Third series. Read November, 1844. Am. Phil. Soc. Trans., vol. X, pp. 1–15. (Astron. Nachr., No. 517, October, 1844, vol. XXII, pp. 203–210. Roy. Astr. Soc., Month Notices, December, 1844.) Latitude of observatory; moon culminations; occultations; longitude of observatory; Encke's comet; comet of 1843; Mauvais's comet; Faye's comet.
43. Meteorological observations made at Hudson, Ohio, latitude 41° 14′ 42″.6 north, longitude $5^h\ 25^m\ 44^s.15$ west, during the years 1841, 1842, 1843, and 1844, with a summary for 7 years. (Barometer; thermometer and hygrometer; winds; clouds; rain.) Am. Jour., October, 1845: (1), vol. XLIX, pp. 266–283. (Astr. Nachr., vol. XXII, pp. 203–210.)
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52. Report on the meteorology of the United States, submitted to the Secretary of the Smithsonian Institution. Sen. Doc. No. 23, 30th Congress, first session, pp. 193–207. Ordered printed January, 1848.
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129. Results derived from an examination of the United States weather maps for 1872 and 1873. Read in N. A. S., April, 1874. Am. Jour. (3), vol. VIII, pp. 1–15. (With two plates.) (Influence of rainfall upon the course of storms; influence of the wind's velocity upon the progress of storms; relation between the velocity of the wind and the velocity of a storm's progress; to determine whether a storm is increasing or diminishing in intensity; form of the isobaric curves; classification of storms; where do the storms which seem to come from the far West originate?)
130. Elements of the differential and integral calculus, revised edition. 8vo, pp. 309. New York, 1874.
131. Results derived from an examination of the United States weather maps for 1872 and 1873. Read in N. A. S., November, 1874. Am. Jour. (3), vol. IX, pp. 1–14. (With plate.) (Direction and velocity of the wind within areas of maximum pressure; consequences of the outward flow of air from an area of high barometer; monthly minimum of temperature; long continued periods of cold weather; storms of January 6–8, 1874; connection between the velocity of the wind and the distance between the isobars in the neighborhood of a storm center.)
132. Results derived from an examination of the United States weather maps for 1872, 1873, and 1874—third paper. Read in N. A. S., April, 1875. Amer. Jour. (3), vol. X, pp. 1–14. (With plate.) (Directions of storm paths; diurnal inequality in the progress of storms; influence of rainfall upon the course of storms; in-influence of a neighboring area of high barometer upon the progress of a storm; form of the isobaric curves; great and sudden changes of temperature; storm of January 15, 1875, at Denver, Colo.)

133. The descendants of Joseph Loomis, who came from Braintree, England, in the year 1638, and settled in Windsor, Conn., in 1639, second edition, revised and enlarged. 8vo, pp. 611. New Haven, 1875.
134. Key to treatise on algebra. 12mo, pp. 219. New York, 1875.
135. Contributions to meteorology, being results derived from an examination of the United States weather maps and from other sources—fourth paper. Read in N. A. S. November, 1875. (With plate.) Am. Jour. (3) vol. XI, pp. 1–17. (Movement of areas of high barometer; monthly minima of temperature; influence of winds on the temperature, moisture, and pressure of the atmosphere; diurnal inequality in the rainfall; comparison of storm paths in America and Europe; oscillations of the barometer in different latitudes; storms traced across the Atlantic Ocean; velocity of ocean storms; storms of January 29 to February 8, 1870, on the Atlantic Ocean; application of Ferrell's formula; stationary storms.)
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142. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—ninth paper. Read in N. A. S. April, 1878. Am. Jour. (3), vol. XVI, pp. 1–21. (With three plates.) (Low barometer at Portland, Oregon; low barometer at San Francisco; areas of high barometer; temperature of Iceland and Vienna compared.)
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146. Anthony D. Stanley, professor of mathematics. In Yale College, a sketch of its history, etc., vol. 1, pp. 254–256. 1879.

147. Connecticut Academy of Arts and Sciences. *Ibid.*, pp. 329–337.

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153. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—sixteenth paper. Read in N. A. S. November, 1881. Am. Jour. (3), vol. XXIII, pp. 1–25. (With a map.) (Mean annual rain-fall for different countries of the globe; cases of excessive rain-fall; cases of deficient rain-fall.)

154. Contributions to meteorology, being results derived from an examination of the observations of the United States Signal Service and from other sources—seventeenth paper. Read in N. A. S. April, 1882. Am. Jour. (3), vol. XXIV, pp. 1–22. (With three plates.) (Relation of rain areas to areas of low pressure.)

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161. Contributions to meteorology—twenty-second paper. Am. Jour., April, 18 (3), vol. XXXIII, pp. 247–262. (With a plate.) (Areas of high pressure, the magnitude, and direction of movement; relation of areas of high pressure areas of low pressure.)

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A MEMOIR OF WILLIAM KITCHEN PARKER, F. R. S.*

William Kitchen Parker was born at Dogsthorpe, near Peterborough, June 23, 1823, and died suddenly, of syncope of the heart, July 3, 1890. He was visiting his second son, Prof. W. N. Parker, of Cardiff, and whilst cheerfully talking of late discoveries and future work in his favorite biological pursuits, he ceased to breath. Accustomed to outdoor life, he was a true lover of nature from the first; the forms, habits, and songs of birds, especially, he knew at an early age. Village schooling at Dogsthorpe and Werrington, and a short period at Peterborough Grammar School prepared him for an apprenticeship at 15 years of age to Mr. Woodroffe, chemist and druggist at Stamford; and 3 years afterwards he was apprenticed to Mr. Costal, medical practitioner, at Market-Overton. At Stamford he studied botany earnestly, and used to persuade a fellow apprentice to leave his bed in early mornings to go afield in search of plants. Both when living at his father's farm and in his holidays afterwards he kept many pet animals and dissected whatever he could get, including a donkey and many birds. Of the latter he prepared skeletons, and of these he made many large drawings at Market-Overton, which of late years he had some thought of publishing as an atlas of the osteology of birds. In 1844–'46 he studied at King's College, London, and became student-demonstrator to Dr. Todd and Mr. (now Sir William) Bowman there. He also attended at Charing Cross Hospital in 1846 and 1847, and, having qualified as L. S. A., he commenced practice in 1849 at Tachbrook street, Pimlico, and soon afterwards married Miss Elizabeth Jeffery. His wife's patient calmness under all difficulties and trials was a true blessing to a man of Mr. Parker's excitable temperament, and her unselfish life and widespread influence for good are well known in and beyond the family circle. Unfortunately he was left a widower about four months ago. His family consists of three daughters and four sons. Of the latter, one is professor of zoölogy and comparative anatomy in the University of Otago, New Zealand; the second is professor of biology in the University College at Cardiff, South Wales; the third is an able draftsman and lithographer, and the fourth has lately taken his diplomas of L. R. C. P. and M. R. C. S.

Mr. Parker had a good father, courteous and gentle by nature, conscientious, and earnest in business, who had worked hard to be able to

* From *Nature*, July 24, 1890, vol. XLII, pp. 297–299.

give even his youngest son, Mr. W. K. Parker, "a start in life." Fro his placid and thoughtful mother he probably inherited much of hi love of reading and his talent for learning.

Always energetic, in spite of constant ill health, Mr. Parker enthu siastically carried on his medical work and his natural history studies especially in the microscopic structure of animal and vegetable tissue Polyzoa and Foraminifera, collected on a visit to Bognor, and fron among sponge sand and Indian sea-shells, especially attracted his at tention. Having sorted, mounted, and drawn numbers of these micro zoa, he was induced, about 1856, by his friends W. Crawford Williamso and T. Rupert Jones, to work at the Foraminifera systematically. Hi paper on the *Miliolitidæ* of the Indian Seas (Trans. Micros. Soc., 1858 and a joint paper (with T. R. Jones) on the Foraminifera of the Norwe gian coast (*Annals N. H.*, 1857) resulted; and the latter formed th basis of a memoir on the Arctic and North Atlantic Foraminifera (Phi Trans., 1865). With T. Rupert Jones, and afterwards with W. B. Ca penter and H. B. Brady, Mr. Parker, down to 1873, described and illus trated many groups and species of Foraminifera, recent and fossil (se C. D. Sherborn's "Biography of Foraminifera" for these papers an memoirs), thereby establishing more accurately a natural classificatio of these microzoa, determining their bathymetrical conditions, an therefore their value in geology. That he did not neglect anatomica research is shown by memoirs in the Proceedings and Transactions o the Zoölogical Society on the osteology (chiefly cranial) and systemat position of Balæniceps (1860), Pterocles (1862), Palamedea (1863), Galli naceous Birds and Tinamous (1862 and 1866), Kagu (1864 and 1869) Ostriches (1864), Microglossa (1865), Common Fowl (1869), Eel (Nature 1871), skull of Frog (1871), of Crow (1872), Salmon, Tit, Sparrow Hawk Thrushes, Sturgeon, and Pig (1873). In the mean time the Ray Societ had brought out his valuable "Monograph on the structure and devel opment of the Shoulder Girdle and Sternum in the Vertebrata" (1868) and his Presidential addresses to the Royal Microscopical Society (1872 1873), and notes on the Archæopteryx (1864), and the fossil Bird bone from the Zebbug Cave, Malta (1865 and 1869), had been published Subsequently the Royal Society's Transactions contained his abun dantly illustrated memoirs on the skull of the Batrachia (1878 an 1880), of the Urodelous Amphibia (1877), the Common Snake (1878) Sturgeon (1882), Lepidosteus (1882), Edentata (1886), Insectivora (1886) and his elaborate memoir on the development of the wing of the Com mon Fowl (1888). In the "Reports of the *Challenger*" is his memoir o the Green Turtle (1880); and those on Tarsipes (Dundee, 1889), and th Duck and the Auk (Dublin, 1890), are his last works.

In former times a skull was taken as little more than a dry, symme rical, bony structure; or if it were the cartilaginous brain case of shark, it was to most a mere dried museum specimen. When howeve the gradations of the elements of the skull, from embryonic beginning were traced until their mutual relations and their homologues in othe

vertebrates were established, light was thrown on the wonderful completeness of organic uniformity and singleness of design. How such studies can be carried on both by minute dissection and the modern art of parallel slicing, and not by one method alone, is to be gathered from his teaching.

Mr. Parker was elected a Fellow of the Royal Society in 1865, and in the year following he received a royal medal for his comprehensive, exact, and useful researches in the developmental osteology, or embryonal morphology, of vertebrates. Some few years afterwards the Royal Society gave him an annual grant to aid in the prosecution of his studies; and, when that was discontinued, a pension from the Crown was graciously and appropriately awarded to him. A generous friend, belonging to a well-known Wesleyan family, more than once presented £100 towards the cost of some of the numerous plates illustrating his grand memoirs in the philosophical transactions.

In 1873, he received the diploma as member of the Royal College of Surgeons, and was appointed Hunterian professor, Professor Flower being invalided for a time; and afterwards both held the professorship conjointly. His earnestness and wide views were well appreciated, opening up the modern aspect of comparative anatomy, and showing that both in man and the lower vertebrates the wonderful structural development of their bony framework should be studied in a strictly morphological rather than a teleological method, and that its stages and resultant forms could be regarded only in the Darwinian aspect.

These lectures, given in abstract in the medical journals, became the basis of his "Morphology of the Skull," in writing which, from his dictation and notes, Mr. G. T. Bettany kindly assisted him; and again, in a semi-popular book, "On Mammalian Descent," another friend (Miss Arabella Buckley, now Mrs. Fisher) similarly helped him. In the latter work his own usual style frequently predominates, full of metaphor and quaint allusions, originating in his imaginative and indeed poetic mind, fully impregnated with ideas and expressions frequent in his favorite and much-read books—Shakespeare, Bacon, Milton, some of the old divines, and above all the old English Bible.

Separating himself from the trammels of foregone conclusions and from the formulated but imperfect misleading conceptions of some of his predecessors in biology, whom he left for the teaching of Rathke, Gegenbaur, and Huxley, Prof. W. K. Parker earnestly inculcated the necessity of single-sighted research, and the following up of any unbiased elucidations, to whatever natural conclusion they may lead. Simple and firm in Christian faith, resolute in scientific research, he felt free from dread of any real collision between science and religion. He insisted that "our proper work is not that of straining our too feeble faculties at system building, but humble and patient attention to what nature herself teaches, comparing actual things with actual" (Proc. Zoöl. Soc., 1864); and in his "Shoulder girdle, etc.," page 2, he writes: "Then, in the times to come, when we have 'prepared our

work without, and made it fit for ourselves in the field,' we shall be able to build a 'system of anatomy' which shall truly represent nature and not be a mere reflection of the mind of one of her talented observers."

Again, at page 225, in illustration of some results of his work, he says: "The first instance I have given of the shoulder girdle (in the skate) may be compared to a clay model in its first stage or to the heavy oaken furniture of our forefathers, that 'stood pond'rous and fixed by its own massy weight.' As we ascend the vertebrate scale the mass becomes more elegant, more subdivided, and more metamorphosed until, in the bird class and among the mammals, these parts form the framework of limbs than which nothing can be imagined more agile or more apt. So, also, as it regards the sternum; at first a mere outcropping of the feebly developed costal arches in the amphibia, it becomes the keystone of perfect arches in the true reptile; then the fulcrum of the exquisitely constructed organs of flight in the bird; and, lastly, forms the mobile front wall of the heaving chest of the highest vertebrate.

Prof. W. K. Parker was a fellow of the Royal, Linnean, Zoölogical, and Royal Microscopical Societies; honorary member of King's College, London, the Philosophical Society of Cambridge, and the Medical Chirurgical Society. He was also a member of the Imperial Society of Naturalists of Moscow, and corresponding member of the Imperial Geological Institute of Vienna, and the Academy of Natural Sciences of Philadelphia. In 1885 he received from the Royal College of Physicians the Bayly medal, "*Ob physiologiam feliciter excultam.*"

In conversations shortly before his death he often spoke of looking forward throughout his life-time (alas! how quickly shortened!) to continued application of all the energy he could devote to his useful work—at once a consolation to him and a duty.

He has well expressed his own view of biological pursuits at page 363 of the "Morphology of the skull:" "The study of animal morphology leads to continually grander and more reverent views of creation and of a Creator. Each fresh advance shows us further fields for conquest, and at the same time deepens the conviction that while results and secondary operations may be discovered by human intelligence, 'no man can find out the work that God maketh from the beginning to the end.' We live as in a twilight of knowledge, charged with revelations of order and beauty; we steadfastly look for a perfect light, which shall reveal perfect order and beauty."

An unwordly seeker after truth, and loved by all who knew him for his uprightness, modesty, unselfishness, and generosity to fellow-workers, always helping young inquirers with specimens and information, he was suddenly lost to sight as a friend and father, but remains in the minds of fellow-workers, of those whom he so freely taught, and of his stricken relatives, as a great and good man, whose beneficent influence will ever be felt in a wide-spreading and advancing science and among thoughtful and appreciative men in all time.

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